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THE

CHEMICAL NEWS

AND

JOURNAL OF PHYSICAL SCIENCE:

(WITH WHICH IS INCORPORATED THE "CHEMICAL GAZETTE.")

A Journal of Practical Chemistry

IN ALL ITS APPLICATIONS TO

PHARMACY, ARTS, AND MANUFACTURES.

EDITED BY
WILLIAM CROOKES, F.R.S.

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PREFACE.

The undersigned publishers of the authorized American Reprint of the London Chemical News, in submitting to the public their first semi-annual volume, desire to acknowledge the favor with which their undertaking has been received, and to call renewed attention to its features of interest and value.

From the outset they were assured that by its republication, the cause of science in the United States, and the best use of its application to the arts and manufactures of life, together with Pharmacy and Medicine, were to be largely promoted. The welcome which our endeavor to extend real scientific information has met with, and the support it has gained, together with the intrinsic worth and wide reach of the matter contained in these pages, are our guarantee for that result.

The Chemical News is not a journal of recent origin. In connection with its predecessor, The Chemical Gazette, it has, for more than twenty-five years, fully and faithfully represented the progress of Chemistry and cognate sciences in England and throughout the world. Its present position has been attained by successive improvements, until it now numbers among its contributors nearly every chemist of note in Europe and America. In its columns some of the most important chemical and physical discoveries have for the first time been made known, and investigators frequently make use of its pages to secure priority of a discovery, by the bare mention of facts and results, before publishing their full papers. The good influence of this journal, in the progress of pure chemical research and advancement, has been felt for years. It is now everywhere cited as the great repository of chemical knowledge, discussion, and authority. Its editorial staff is made up of gentlemen in the first rank of science. Moreover, as the Chemical News is not the organ of any institution, clique, professional or trading firm, its conductors are under no liability to act or write, at any time, in other than a fearless and independent manner. No trade puffs are ever inserted here—no unworthy books or patents are ever commended. Its character in these and kindred respects is one of honest pride with its proprietor and publishers.

But however high the position of this journal as a treasury of all that is fresh and valuable in chemistry, it would be a mistake to consider the Chemical News as covering that department of science alone. It is hardly less a periodical of importance to the medical profession, for it often contains papers giving the methods and results of the thorough application of chemistry to medicine. These are of such a nature as to present the fruits of studious observation and thought in a manner elsewhere unattained. Several papers in the current volume may stand in support of this, as also to show the attention given by its editors to public sanitary questions.

The Chemical News, again, is a rich medium of information to every theoretical and practical pharmaceutist, druggist, and apothecary. In its reports of the British Pharmaceutical Society, and the British Pharmaceutical Conference, with occasional papers upon special topics, and its chemical notices from foreign sources, it constantly supplies matter indispensable to every well-trained and furnished member of this respected and important calling. It affords much gratification to the American publishers to be the means of advancing the character and qualifications of so numerous and wide-spread a class in the community.

Numbers of extensive manufacturers in this country, of many sorts, have for years past prized the Chemical News as a valued auxiliary and guide in the various processes by which their goods and fabrics have been prepared for consumption and use, and the number of such persons is on the increase.

Photography and the finer arts are the objects of watchful and conscientious notice at the hands of those who prepare the Chemical News. New processes and profitable suggestions in this connection are not seldom brought out for the first time in its pages.

In its analyses of metals, its record of the developments in mining throughout the world, its attention to mineralogy, its discussions of mechanics and electricity, it is believed to be without a competing rival.

To fill so wide a range of application is apparently difficult of accomplishment. That it is done, and in the most thorough way, no regular reader of the Chemical News need be informed.

The general features of this journal may be thus summed up in detail:

1. Leading and Editorial articles by the well-known Editor, Mr. William Crookes, F.R.S., on all topics within the proper scope of the journal. Besides these, each number contains leading articles of the same nature, by other persons eminent in the walks of science. In the space devoted weekly to "Communications Received," subscribers will recognize the shining positions held by its corespondents. When an article appears without a full signature, it is by no means to be supposed that it is necessarily the production of the responsible Editor: anonymous contributions from the most distinguished chemists of the day often grace the columns of the Chemical News.

2. Graphic pictures of Foreign Science by the Paris correspondent of the Chemical News, one of the first scientific men in France, and an expert in lucid scientific exposition, the Abbé Moigno. The interest and the availability of these letters cannot well be overstated. From a practical point of view, they add very largely to the value of the Magazine for a wide circle of students and readers.

3. Its Reports of Societies have ever been a marked feature of the Chemical News. From the first number, its readers have always had presented to them a complete account of the proceedings of the Chemical,

Royal, and Pharmaceutical Societies, and the Royal Institution. These matters are given either verbatim or in a form more or less condensed, according to the importance of the subject, the reports being in many cases prepared by the speakers themselves. The enterprise of this journal in its late admirable report of the Dundee meeting of the British Association for the Advancement of Science, printed in this volume, is but a specimen of the readiness of its conductors always to procure the best accounts of the best gatherings. The Manchester Literary and Philosophical Society, the British Medical Association, the French Academy of Sciences, the Royal Dublin Society and Quekett Microscopical Club, are among those whose transactions appear in the present volume. The Abbé Moigno is the reporter of the French Academy, and each weekly impression of the Chemical News usually presents a record of that Society's meetings from his masterly hand.

4. Fresh, prompt, and impartial notices of scientific books.

5. Columns for correspondence which are freely used, and possess a lasting as well as ephemeral value. Here each new theory, and every great step in the changing phases of notation or nomenclature which have occupied the attention of the chemical world, are, during their tentative state, the subjects of discussion. Practical recipes on matters of general and particular interest, communications concerning traffic in scientific materials, have all found, and will hereafter find, in this department, a fitting and serviceable receptacle.

6. Chemical Notices from Foreign Sources, giving a condensed account of every important chemical paper in the world, as soon as it is published. To give a regular and detailed account of such papers would fill an octavo volume weekly. In this department considerable judgment is required to decide what papers to omit altogether, and which to curtail, so as to allot to each subject its due prominence. This responsible office of selection and condensation is entrusted to a chemist thoroughly competent to carry out this design.

7. Miscellaneous paragraphs of general concern to those interested in the relations of science to all matters affecting individual and social well-being and comfort.

8. Contemporary Scientific Press. This is a new feature, recently introduced, at the suggestion and request of many leading chemists. It purports to give, as soon as possible after publication, the title of every chemical paper in the world: to compile it, every accessible scientific periodical is ransacked.

9. Lists of English Patents.

10. Notes and Queries—for the interchange of brief question and reply on a wide range of chemical, scientific, and general topics.

11. Brief Answers to Correspondents, Lists of Communications, Books Received, etc., etc.

Besides the features thus enumerated, every phase of affairs in the scientific world, with all important events, receive such regard and place in the pages of the Chemical News as they merit. For illustration, it is proper to remark upon the elaborate obituary notice of the lamented Faraday, which appears upon page 268 of this volume, as also upon the introductory address at St. Bartholomew's Hospital Medical School, by Dr. Odling, F.R.S., printed on page 306. The character and value of a journal that furnishes to its readers such a series of lectures as those delivered at the Royal Institution of Great Britain, by Dr. William Allen Miller, LL.D., on "Spectrum Analysis, with its application to Astronomy," published on pp. 29, 67, 135, 186, admit of no question.

The long period of existence of the Chemical News, and the high position which it has always taken, have gradually led to its introduction into all the public and private laboratories, the museums, institutions, and libraries in England, on the Continent, and in America. Its original and editorial articles are constantly reprinted in the Old and New World, and have been translated into German, French, Italian, Spanish, Russian, and other languages. The admission of an original paper, therefore, into its pages, secures its rapid diffusion over the whole world. With the exception of the *Philosophical Magazine*, established in 1798, and the *Proceedings of the Learned Societies*, no other English scientific journal enjoys this universal publicity.

Stimulated by their knowledge of all that has been said, the undersigned, in reprinting the Chemical News in America, with the authorization of Mr. Crookes, its distinguished editor, who regularly furnishes them with early weekly sheets of the English issue, and who will, after January 1, 1868, withdraw the circulation of the English edition in the United States, have spared no pains to make its appearance correspond to its place and character in the world of science. The American edition, printed in monthly instead of weekly numbers, has some practical advantages for its subscribers by virtue of this fact. Arrangements are made to secure and retain the best and most careful editorial ability to provide for the labor which such a change necessitates, and to superintend the reissue in its passage through the American press. The numbers of the reprint may henceforth be expected to be as close a *fac-simile* of the English edition as the case will admit of. The mechanical execution of the American edition of the Chemical News will not, we are confident, suffer any disparagement by comparison with its original.

In consequence of the increased cost of manufacture and the liberal annuity paid to the English Proprietor, the undersigned are constrained to fix the subscription price of the Reprint at $3.00 *per annum*, in advance, postage free, on and after January 1, 1868.

With this introduction, Volume I. of the American Reprint of the Chemical News is offered to the public by

THE AMERICAN PUBLISHERS.

December, 1867.

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THE CHEMICAL NEWS

AND

JOURNAL OF PHYSICAL SCIENCE.

Volume 1. July, 1867.

ENGLISH DISCOVERERS AND FRENCH ACADEMICIANS.

A COMMUNICATION on the estimation of copper, from M. de la Follye, was recently brought before the French Academy by M. Pelouze. The author says that, in attempting to estimate copper by Pelouze's process (precipitation by a standard solution of sulphide of sodium), he met with certain difficulties which led him to try another process, and he ultimately decided upon adding a standard solution of cyanide of potassium to an ammoniacal solution of the copper which was to be determined. M. de la Follye calls this "only a modification of the excellent method of that eminent chemist," Pelouze; and as that eminent chemist himself presented the paper, we may assume that he admitted the substantial accuracy of the statement.

We are so accustomed to see English discoveries rediscovered by French chemists and brought before the Academy of Sciences as undoubted novelties, that in general we refrain from drawing attention to this unfairness. It appears to be one of the stages through which an English discovery must pass before it obtains Continental recognition; but for several reasons we cannot allow this instance to pass over without animadversion. Every chemist will at once perceive that the new process of M. de la Follye is absolutely identical with that of Henry Parkes, published in the *Mining Journal* in 1851. Owing to its neatness, convenience, and general accuracy, it has become almost universally employed in the commercial analysis of copper ores, and we venture to say that there is scarcely a laboratory in Europe in which Parkes's process has not been more or less used. In all standard works on analysis, English and German, Parkes's process stands side by side with Pelouze's process; and were it not that an English writer, generally remarkable for his literary accuracy, has appeared to acquiesce in M. de la Follye's pretensions, we should say that it was utterly inconceivable how any chemist could venture to publish so old and well-known a process as original.

We are glad to see that this discreditable attempt at appropriating Henry Parkes's well-earned laurels has met with an indignant protest from E. F. Dürre, of Berlin, in the *Berg. und Hüttenmännische Zeitung* for March 22. He says that there is "nothing rare in their Western neighbours ignoring foreign merit altogether; but in such a case as this, when a well-known process is brought, as a new discovery, before an important learned Society, by a man of wide-spread reputation and acknowledged ability, it becomes the duty of every one not to content himself with the mere rectification of the error, but to enter a loud protest against such a claim."

After pointing out the utter groundlessness of MM. de la Follye's and Pelouze's pretensions, Herr Dürre concludes by saying that it is necessary that this circumstance be rightly appreciated, to show how cautious one must be in respect to a French communication, even when it is brought before the Academy of Sciences by one of its members.

THE USE AND ABUSE OF BLACKBALLING.

WE feel constrained again to draw attention to certain unprecedented proceedings which have distinguished the last two meetings of the Chemical Society. For some years the laxity of Fellows in exercising their right of blackballing candidates for Fellowship whose claims were clearly inadequate to entitle them to that honour has been a subject of general comment. Owing to the exceptional position held by the editor of this paper as journalist, and at the same time as a Fellow of the Chemical and other learned bodies, a friendly correspondence with many chemists in all parts of the country has been carried on at one time or another on matters connected with their Society. We are thus necessarily in a favourable position for ascertaining the existence of any grievance among the members.

The Council, as announced by the President at the anniversary meeting, finding that a wide-spread feeling of dissatisfaction existed at the undue facility with which anybody could join the Society, and that this privilege had been on more than one occasion seriously perverted for trade purposes, have lately had under discussion the best method of diminishing this abuse, and restoring to the title F.C.S. its former honourable distinction.

The subject of raising the qualification or restricting the admissions is beset with many difficulties; but those of our readers who are Fellows of the Chemical Society may rest assured that this question is receiving, and will continue to receive, the very serious attention of the Council. When the time comes for a proposal to alter the by-laws at a general meeting, we are convinced that the explanations and the reasons for such alterations, which will then be brought forward, will be adopted by a large majority.

The fact, then, is, that candidates have been admitted too freely. Country members complain that their brother members in London never exercise the right to blackball a candidate. It seems strange that attention should now have to be called to the fact that this undoubted right, so long in abeyance, is now apparently abused in a manner so reckless and suicidal as to seriously imperil the interests of the Society.

A few weeks ago we alluded to the abortive attempt on the part of a few junior members to rearrange the list of Council and officers. Failing signally in that endeavour, it would seem as if they were now attemptings, by a concerted plan of action, to assume the governing power in the Society by blackballing candidates irrespective of scientific position or attainments. Thus, party interests are made secondary to scientific progress, and the healthy existence of the Society is imperilled for the gratification of private pique.

Of the five hundred Fellows of which the Society consists, seldom more than forty regularly attend the meeting, and of these the majority have hitherto abstained from balloting. According to the by-laws, "when less than three-fourths of the Fellows who vote are in favour of the candidate, he shall not be elected a Fellow;" supposing then that forty-seven voters are present, it will be seen that a dozen disaffected members are able practically to control the ballot-box. Fellows should remember that the possession of a legal right to blackball a candidate, does not imply a moral right to abuse this privilege. They hold this in trust, to be exercised honestly for the good of the Society. In the "obligation" which each Fellow signs on his admission, he engages to "promote the interests and welfare of the Chemical Society," and he is not justified in recording an adverse vote simply to gratify the pitiful ambition of a small party, or to exercise a paltry spite against those who have won the confidence and support of the majority of members.

Between those elected and those rejected at the last meeting we wish to make no invidious comparisons. Few will dispute the qualifications of the fortunate candidates; but those rejected, from their position and scientific acquirements, may be fairly said to possess a very good claim to the coveted honour. In the absence of definite acquaintance with the qualifications of any candidate, the Fellows are, in a certain sense, morally obliged to vote for any one who has so excellent an array of recommenders from personal knowledge as could be seen on the certificates of the gentlemen who had to suffer, for no fault of their own, the indignity of rejection.

Before it is too late, we wish to appeal to the good sense of those who think themselves aggrieved. If they conscientiously object to a candidate, no one would ask them to violate their convictions by voting in his favour; but if they have any grievance, or wish to propose any alteration in the method of conducting business, let them adopt a straightforward course. We can promise them a patient and attentive consideration of any measure they may wish to introduce; but it is unbearable that ten or a dozen young men should, by devious strategy, attempt to overrule the wishes of the Council and of the great body of members.

Apart from the bad policy of making themselves personally obnoxious to the leading men of their science, the malcontents should consider that they are powerless permanently to control the elections; they could but snatch a momentary triumph, for immediately after the necessary alteration of the by-laws the rejected candidates, if they still cared for the honour, would come forward with a fair prospect of election. A slight modification in the by-laws for the election of Fellows, which would certainly be approved at a general meeting if brought forward by the Council, would take from any clique a power which may be so unwisely wielded. The problem to be solved is how to guard against the admission of unfit persons into the Society, and at the same time to prevent voting by ballot becoming an organ for the gratification of private pique. In several ways the necessary alteration could be made, but we hope such a step will not be forced upon the Council.

ELECTION OF FELLOWS AT THE CHEMICAL SOCIETY.

It was suggested last week that it would probably be necessary to alter some of the By-laws regulating the admission of Fellows into the Chemical Society, and we stated that the problem to be solved was how to guard against the admission of unfit persons, and at the same time to prevent the ballot becoming an organ for the gratification of personal pique. On looking more carefully into the Charter and By-laws of the Chemical Society, it appears that there is to be found a very simple solution of the latter part of this problem. Attention being drawn to the subject, it has not failed to strike most persons as manifestly unjust that the decision on an important question, brought before a meeting of the members, should be determined according to the wishes of the minority; and it shows how harmoniously the

Society and its officers have hitherto worked together that this hardship has never been felt before. On his admission, each Fellow received the regulations of the Society, together with sundry other formal documents; but we venture to say that not many looked at them a second time, and few will now be able to lay hands on a copy of the charter and by-laws, to which we now propose to direct attention. When everything is going on smoothly, the laws and regulations of the Society are dormant.

It now appears that for many years the Society has been acting contrary to the charter, in electing Fellows, honorary and foreign members, and associates, according to the existing by-laws.

By the third paragraph of the Charter of Incorporation, granted to the Society in 1848, it is declared—

> "That at all General Meetings and meetings of the Council the majority present and having a right to vote thereat respectively shall decide upon the matters propounded at such meetings."

Again, the concluding paragraph of the Charter declares,

> "That no resolution or by-law shall, on any account or pretence whatsoever, be made by the said body politic and corporate in opposition to the general scope, true intent and meaning of this our charter; and that if any such rule or by-law shall be made, the same shall be absolutely null and void to all intents, effects, constructions, and purposes whatsoever."

Now, it is very evident that the by-law, making the election of a candidate depend upon the votes of three-fourths of the Fellows present, is in direct contravention to the "true intent and meaning" of the first-quoted paragraph of the charter; and therefore such by-law is, according to the terms of the second citation from the charter, "absolutely null and void to all intents, effects, constructions, and purposes whatsoever."

But it may be argued that the third paragraph of the charter refers only to anniversary meetings or extraordinary meetings of the Society. An attentive examination will, however, show that by the term "general" meeting is intended that which is now commonly called an "ordinary" meeting. The term "*ordinary* meeting" strictly means *regular* or *customary* meeting; a *general* meeting means one public or common to the whole of the Fellows. Custom has sanctioned the omission of the word "general" as qualifying the ordinary meetings, but it is still retained in by-law, which speaks of an *extra*-ordinary general meeting, which, logically and grammatically, can only mean a *general meeting* held *extra*, or in addition, to the *ordinary* bi-monthly meetings.

The term "general meeting" occurs several times in the charter, but nowhere do we find the terms "ordinary" and "anniversary" meetings. When, however, we refer to the regulations of other learned societies, no longer can there be any doubt as to the meaning of the term "general" meeting. In the Charter of the Royal Society no mention is made of meetings of the members, ordinary, extraordinary, or general. But in the Charter of the Zoological Society the term "general" meeting is used in a sense applicable only to ordinary meetings; and in the by-laws the monthly meetings of the Society are invariably spoken of as "general meetings," or "ordinary general meetings." In the charters of the Linnæan and Geological Societies likewise the term "general meeting" is used to express the ordinary meetings of the members, and in the by-laws these are invariably termed "general" meetings; the 9th section of the rules of the latter society, in fact, state that "the *general* meetings to be held by the Society shall be of three kinds:—1. *Annual;* 2. *Special;* 3. *Ordinary.*" It cannot, therefore, be doubted that the term "general" meeting in the third paragraph of the Charter of the Chemical Society means the same as the term "ordinary" meeting does in the by-laws.

No one, we imagine, will dispute that the election of Fellows is a "matter propounded" at a meeting upon which the "majority present" are to decide. On the contrary, the question as to whether the candidate is to be admitted to the Fellowship of the Chemical Society is a very important matter propounded to those present who have a right to vote, and the question, above all others, which should be carried by a numerical majority.

In strict logical interpretation, therefore, those candidates who were blackballed at the recent meetings of the Society were excluded from the Fellowship in error. A numerical majority of voters were in favor of their admission, and at the present time the blackballees are as strictly entitled to the letters F.C.S. as are any of the blackballers.

We confess we see only one course to be pursued in respect to the by-laws which remit the decision on such important matters as the election or removal of Fellows to a small minority, in opposition to the wishes of the great bulk of those present. Strictly speaking, these restrictive by-laws never had any legal existence, and they should at once be replaced by others in conformity with the charter.

One-half of the problem now under discussion—that of preventing voting by ballot becoming an organ for the gratification of personal pique—has solved itself in a manner which leaves nothing to be desired. The first part of the question—How to guard against the admission of unfit persons into the Society—is one in which Council and Fellows are free to act, for the charter specially provides that they "may alter, vary, or revoke, and may make such new and other by-laws as they shall think most useful and expedient for the said body politic and corporate, so that the same be not repugnant to these presents." Thus the way is clear to a satisfactory settlement of the recent unwarrantable proceedings and the permanent prevention of their recurrence, and to the establishment of safeguards against the degradation of the Fellowship of the Chemical Society.

SCIENTIFIC AND ANALYTICAL CHEMISTRY.

On the Determination of Soda in the Assay of Potashes, by M. Graeger.

Commercial potashes generally contain soda. To determine the proportion by volumetric assay, we must know the quantity of pure alkaline carbonates contained in the substance to be assayed. For this object the author dissolves 6·911 grammes of the potash to be assayed in 100 c.c. of water, weighs the insoluble portion, and determines in one part of the solution the quantity of chlorine (chloride of potassium), in another the quantity of sulphuric acid (sulphate of potash), determinations which may be made with standard solutions. The estimation of the alkalies is effected on 10 c.c. of the solution by means of normal nitric acid; and from the quantity of acid added the relation of soda to potash may be calculated, as the total quantity of alkaline carbonates is known by subtracting from the total weight of substance taken the weight of the insoluble matter of the chloride and sulphate of potash.

To assist in the employment of this method, the author has drawn up a table giving the amounts of carbonate of potash and carbonate of soda corresponding to the quantity of normal nitric acid necessary for their saturation:—

KO,CO_2. Grammes.		NaO,CO_2. Grammes.		NO_5. C.C.
1·00	+	0·00	require	14·47
0·95	+	0·05	'	14·69
0·90	+	0·10	"	14·92
0·85	+	0·15	"	15·14
0·80	+	0·20	"	15·35
0·75	+	0·25	"	15·57
0·70	+	0·30	"	15·79
0·65	+	0·35	"	16·01
0·60	+	0·40	"	16·23
0·55	+	0·45	"	16·45
0·50	+	0·50	"	16·67
0·45	+	0·55	"	16·89
0·40	+	0·60	"	17·11
0·35	+	0·65	"	17·33
0·30	+	0·70	"	17·55
0·25	+	0·75	"	17·76
0·20	+	0·80	"	17·97
0.15	+	0·85	"	18·19
0·10	+	0·90	"	18·40
0·05	+	0.95	"	18·62
0·00	+	1·00	"	18·84

If, for example, it is found that the crude potash taken contained 5·1134 grammes of pure alkaline carbonate, and it required 79 c.c. of normal nitric acid, the proportion 5·1134 : 1·0 : : 79 c.c. : x will give the quantity of normal acid ($x = 15·45$ c.c.) which one gramme would have required, and on reference to the above table it is found that 15·45 c.c. of normal acid correspond to 0·22 of carbonate of soda and 0·78 of carbonate of potash, or 78 per cent.—*Journal für praktische Chemie*, t. xcvii. p. 496 (1866), No. 8.

On the Preparation of Pure Chloride of Ammonium, by J. S. Stas.

I. Chloride of Ammonium from the Ammonia extracted from Sal Ammoniac, purified by Aqua Regia.—Ten litres of a boiling saturated solution of sal ammoniac were added to a litre of nitric acid of specific gravity of 1·4. The liquid was kept boiling as long as it gave off chlorine. The sal ammoniac which separated from the liquid on cooling was dissolved in pure boiling water, and the solution was boiled with a twentieth of its volume of nitric acid as long as chlorine was produced. The liquid, diluted with pure water until it no longer deposited chloride on cooling, was poured upon hydrate of calcium contained in a large retort to set free the ammonia. The latter, first washed in water, was then placed into pure water. The ammoniacal solution produced was in its turn nearly saturated by a current of pure hydrochloric acid.

The chloride of ammonium, which was deposited by the liquid after its concentration and cooling, was dried at 100° by passing continuously a current of ammoniacal gas into the long-necked globe in which it was being dried. This being done, I sublimed the sal ammoniac with the least possible elevation of temperature, keeping the neck of the globe as far as possible full of dry ammoniacal gas.

The chloride volatilised without leaving the least trace of carbon—a proof that the sal ammoniac contained no compound ammonia. However, it was easy to perceive that the bottom of the globe, which was of ordinary glass, was very slightly attacked. On breaking it to detach the sublimed chloride, I found, in fact, that traces of chlorides of sodium and calcium were formed at the expense of the substance of the glass, and that some silica was set free. By means of spectrum analysis I found that the sublimed sal ammoniac, which was quite colourless, and remarkably transparent, contained traces of sodium, but it was absolutely free from calcium.

In order to eliminate the traces of sodic chloride, which were carried over with the vapour of the sal ammoniac, I sublimed it two more times in an atmosphere of ammonia, at the lowest possible temperature, devoting to this operation some *hard glass* vessels that I had had made expressly for the transformation of the alkaline chlorides into nitrates of these metals. At the temperature at which the chloride of ammonium was sublimed these vessels resisted its vapour perfectly. In the notice "*On the Transformation of the Chlorides into Nitrates,*" I give the composition of the glass of these vessels.

The chloride of ammonium, before being employed in this determination, was heated in the same vessel in which it was weighed, up to the point of giving off vapour, so as to drive off the condensed ammonia.

II. Chloride of Ammonium produced by means of Ammonia prepared from the Commercial Sulphate.—To prepare chloride of ammonium by means of ammonia, from commercial sulphate, I first treated the latter compound as follows:—Two kilogrammes of sulphate were heated with a kilogramme and a half of concentrated sulphuric acid up to the temperature at which the sulphate begins to decompose with effervescence. I then introduced nitric acid by degrees into the mixture until the liquid, which was of a tolerably strong blackish-brown colour, had become quite colourless. The compound ammonias and organic matters contained in the sulphate are thus completely destroyed, with liberation of carbonic anhydride.

The acid sulphate, suitably cooled, was poured into about ten times its volume of cold water, and the excess of acid nearly saturated by lime water. When the sulphate of calcium was deposited, the supernatant liquid was mixed with a sufficient excess of slaked lime contained in a very large globe, and was heated in a bath of a saturated solution of common salt, so as to drive off the ammonia it contained. The latter, after washing in water, was put into pure water.

The ammonia, when dissolved, was saturated by a current of pure hydrochloric acid. The solution of sal ammoniac produced was evaporated to dryness in a globe of *hard glass*, and the residue was sublimed in an atmosphere of ammonia obtained from part of the same chloride.

The sal ammoniac volatilised without leaving a trace of a residue. The sublimed product was absolutely colourless; it gave off an ammoniacal smell. Before being employed it was heated until vapour was given off, so as to drive off the condensed ammonia.

III. Chloride of Ammonium obtained by means of the Ammonia produced by the Reduction of Nitrite of Potassium.—To procure nitrite of potassium, I had recourse to Stromeyer's process. I heated to redness one kilogramme of nitre with four kilogrammes of lead in a small cast-iron crucible. When the vivid incandescence which at first ensued was over, and the mixture sufficiently cooled, I washed it in boiling water. I then, by means of a solution of hydrosulphate of potassium, eliminated the lead from the solution of nitrate. After removing the sulphide of lead and concentrating the remaining solution, I added fifteen litres of a solution of caustic potash of a specific gravity of 1·250. This mixture was poured into a globe of a capacity of twenty-five litres, placed in a sand-bath, and which contained a mixture of three kilogrammes and a half of granulated zinc, deprived of its carbon by fusion with a mixture of carbonate of soda and nitre[*] and a kilogramme and a half of iron, which, after having been oxidised by calcination in contact with air, had been reduced by hydrogen.

The globe communicated through a large bent tube, with an arrangement for washing and condensing the ammonia to be produced. This arrangement consisted of—1st, a large tubulated retort placed upon a furnace and containing a certain quantity of pure water, into which the tube coming from the globe was plunged; 2nd, a large Woulff's flask with three tubes, containing half a litre of pure water; 3rd, a Woulff's flask containing water acidulated with hydrochloric acid, for the purpose of retaining the ammonia carried over by the current of hydrogen, which is produced very freely when the reduction of the nitrite takes place and the liquid in the globe is boiled.

Before distilling the liquid in the globe, I allowed the materials to react during seventy-two hours, so as to reduce as completely as possible the nitrite of potassium into ammonia and oxide of potassium and zinc. After this I boiled the liquid gently for two hours, keeping the water boiling in the retort and cooling that contained in the large Woulff's flask intended for the condensation of the ammonia.

It is indispensable to boil the liquid contained in the globe *very gently*, as it bubbles up violently by the disengagement of the hydrogen at a high temperature. I recommend chemists who wish to procure absolutely pure ammonia by this means to distil the liquid decanted from the mixture of zinc and iron. As I ascertained in a subsequent trial, after the reduction of the nitrite into ammonia has taken place, the decanted liquid may be distilled without the least difficulty.

The ammoniacal solution produced smells exactly similar to the ammonia extracted from chloride of ammonium treated with aqua regia, or from sulphate of ammonium treated while hot with a mixture of sulphuric and nitric acids.

These three ammonias, though identical with each other, differed very considerably as to smell from the pure ammonia obtained from commercial chloride or sulphate of ammonium, both of which contain compound ammonias, that give it a disagreeable smell, whilst the smell of pure ammonia is simply pungent. I have already mentioned these facts in my previous work on the same subject.

To transform the dissolved ammonia into chloride, I passed through the solution a current of pure hydrochloric acid until the liquid was nearly saturated. I then evaporated the saline liquid on a water-bath, and finished drying it on a stove. This chloride of ammonium was of a dazzling whiteness. I proceeded to *sublime it in a large platinum retort*, purified at a red heat with chloride of ammonium. To exclude the air from the retort, I passed through it, during the volatilisation, a very slight current of dry ammonia. This precaution is absolutely indispensable, since, in the presence of air and of heated platinum, the vapour of sal ammoniac will readily produce nitric acid and afterwards chlorine.

The volatilised chloride of ammonium covered the upper portion of the head of the retort, in the form of a compact, crystalline, colourless, transparent ring half a centimetre thick, whilst the head and neck were filled with chloride of ammonium, as a fine dust of a dazzling whiteness. Both exhaled a strong ammoniacal smell.

Before being used to determine the proportional ratios, the compact and the powdered chloride were heated in the same apparatus in which they were weighed, until they gave off vapours of sal ammoniac, so as to eliminate the last traces of condensed ammonia.

Preservation of Sulphuretted Hydrogen Solution in the Laboratory.

At the last meeting of the Pharmaceutical Society of Paris, M. Lepage, of Gisors, brought forward a process which he has adopted for preserving solutions of sulphuretted hydrogen. All chemists know that this useful reagent cannot be preserved long in aqueous solution. The author has adopted for some years an artifice which enables sulphuretted hydrogen solution to be kept for twelve or fifteen months with scarcely any loss of strength. Instead of using water, he saturates a mixture of equal parts of pure glycerin and water with sulphuretted hydrogen gas, and uses it in the ordinary manner. None of the reactions are interfered with in the least, whilst the solution possesses almost perfect stability. The dilute glycerin dissolves less gas than distilled water will; representing the solubility in the latter liquid by 100, that in the former will be 60.

Glycerin likewise prevents solution of sulphide of ammonium from becoming coloured, and M. Lepage believes that it has a similar action on the sulphides of potassium and sodium.

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[*] Zinc may be deprived of carbon by fusion with 5 per cent. of its weight of litharge. The alloy of zinc and lead thus produced is as effectual in reducing nitrite of potassium in the presence of iron as pure zinc. In the presence of dilute sulphuric and hydrochloric acids, it disengages hydrogen with great facility.

On the Classification of Native Silicates,* *by* William Odling, *M.B.*, *F.R.S.*

Again, the function of aluminium or alumina, in some particular silicate, is occasionally open to considerable question. Aluminium salts, it is well known, are derivable from acids, either by the substitution of an atom of aluminium, Al''', for three atoms of hydrogen, as in ordinary alum $Al'''KS_2O_8.6Aq$; or by the substitution of an atom of aluminyl $(AlO)'$ for one atom of hydrogen, as in ordinary acetate of aluminium (the soluble diacetate of Crum) $C_2(AlO)'H_3O_2.2Aq$, or $Al'''\ O''(CH_3O_2)'.2Aq$. In the majority of aluminium-silicates, the aluminium would seem to play the same part that it does in ordinary alum, while in others it may not improbably function as aluminyl. Thus by viewing cyanite and topaz as aluminylic silicates, their anomalous formula $Al_2O_2.SiO_2$, would become reducible to the metasilicate type, and appear as

$$(AlO)_2O.SiO_2, \text{ or } (AlO)_2SiO_3;$$

while euclase would become an orthosilicate, thus—

$$(AlO)_2G_2O_4.Si_2O_4, \text{ or } (AlO)_2H_2G_2O_4.Si_2O_4.$$

But aluminium is not only capable of acting in two distinct fashions as a base, but it can also play the part of an anhydride corresponding to silica. Spinelle, $MgO\ Al_2O_3$, and augite, $MgO.SiO_2$, for instance, may be regarded as analogous, though heteromorphous compounds; and it has been contended by Bonsdorff and others, with considerable show of reason, that in the aluminous augites and hornblendes, the alumina does not act as a base to the silica, but is substituted for a variable amount of the silica isomorphously. Similar remarks apply to boric oxide, B_2O_3, which, in some silicates, as axinite, appears to replace a variable but small proportion of basic alumina or alminyl, while in others, it obviously fulfils the functions of an anhydride, as in datolite and botryolite.

$$Ca''_2H_2\left\{\begin{matrix}B_2O_4\\Si_2O_6\end{matrix}\right. \qquad Ca''_2H_2\left\{\begin{matrix}B_2H_2O_6\\Si_2O_6\end{matrix}\right.$$

In the great majority of well-defined aluminous double silicates, the ratio of monad and diad to triad or pseudo-triad metal is either the spinelle ratio

$$M''O \text{ to } Al_2O_3.$$

or else the cryolite ratio

$$M''_3O_3 \text{ to } Al_2O_3,$$

as exemplified below:—

Spinelle Silicates.

Ortho.	$CaAl_2O_4.Si_2O_4$	Anorthite. Wernerite.
$\frac{1}{1}$	$CaAl_2O_4.Si_2O_4.2Aq$	Thomsonite.
	$CaAl_2O_4.Si_2O_4.4Aq$	Gismondine.
Para.	$CaAl_2O_4.Si_3O_6$	Labradore.
$\frac{2}{3}$	$Na_2Al_2O_4.Si_2O_6.2Aq$	Mesotype.
	$CaAl_2O_4.Si_3O_6.3Aq$	Scolezite. Mesolite.
	$CaAl_2O_4.Si_3O_6.4Aq$	Zeagonite.
Meta.	$K_2Al_2O_4.Si_4O_8$	Leucite.
$\frac{1}{2}$	$Na_2Al_2O_4.Si_4O_8.2Aq$	Analcime.
	$CaAl_2O_4.Si_4O_8.3Aq$	Leonhardite.
	$CaAl_2O_4.Si_4O_8.4Aq$	Laumonite.
	$Na_2Al_2O_4.Si_4O_8.5Aq$	Phillipsite.
	$CaAl_2O_4.Si_4O_8.6Aq$	Chabasite.
Sesqui.	$K_2Al_2O_4.Si_6O_{12}$	Felspar. Orthoclase.
$\frac{1}{3}$	$Na_2Al_2O_4.Si_6O_{12}$	Albite.
	$CaAl_2O_4.Si_6O_{12}.3Aq$	Parastilbite.
	$CaAl_2O_4,Si_6O_{12}.5Aq$	Epistilbite. Heulandite.
	$CaAl_2O_4.Si_6O_{12}.6Aq$	Stilbite. Desmine.

Cryolite Silicates.

Basic.	$Ca_3Al_2O_6.Si_2O_4$	Gehlenite.
$\frac{3}{2}$	$Fe_3Al_2O_6.Si_2O_4$	Aphrosiderite.
	$G_3Al_2O_6.Si_2O_4$	Euclase.
Ortho.	$Ca_3Al_2O_6.Si_3O_6$	Garnet. Idocrase.
$\frac{1}{1}$	$Mg_3Al_2O_6.Si_3O_6$	Allanite. Orthite.
	$(NaCa)_3Al_2O_6.Si_3O_6$	Sarcolite.
	$(HCa)_3Al_2O_6.Si_3O_6$	Prehnite.
Para.	$Mg_3Al_2O_6.Si_4O_8$	Mica?
Meta.	$G_3Al_2O_6.Si_6O_{12}$	Beryl.

Other ratios are, however, occasionally met with, as in the following examples, and particularly the mixed ratio $MO + M_2O_3$ to $2Al_2O_3$, and the double ratio $2M_2O_3$ to Al_2O_3:—

Para.	$K_3Al_6O_{10}.Si_6\ O_{12}$	Potash-mica.
"	$Mg_4Al_4O_{10}.Si_6\ O_{12}$	Magnesia-micas.
"	$K_2Mg_3Al_4O_{10}.Si_6\ O_{12}$	
	$(LiK)_2Mg_3Al_4O_{10}Si_6\ O_{12}$	Lepidolite.
"	$Ca_4(AlB)_4O_{10}.Si_6\ O_{12}$	Axinite.
"	$Ca_6Al_2O_9.Si_6\ O_{12}$	Humboldtite.
Meta.	$H_{12}Al_2O_9.Si_9\ O_{18}$	Cimolite.
Ortho.	$Ca_6Al_8O_{18}.Si_9\ O_{18}$	Epidote.
Meta.	$Li_6Al_8O_{15}.Si_{15}O_{30}$	Spodumene Triphane.
Anhy.	$(LiNa)_6Al_8O_{15}.Si_{30}O_{60}$	Petalite.

(δ) Among so-called hydrated silicates, the determination of the function of the constituent hydrogen is often a matter of considerable difficulty. It is clear that this hydrogen sometimes exists in the form of water of constitution or crystallisation added to the proper silicate molecule, whereas in others it exists as basic hydrogen, forming an integral part of the silicate molecule; but the means for determining the particular cases in which it exists in the one state or the other, or in both states simultaneously, are usually very imperfect, and sometimes entirely wanting. Dioptase, for example, may either be considered as an orthosilicate of hydrogen and copper, $H_2Cu''SiO_4$, or else as a hydrated metasilicate of copper, $Cu''SiO_3.Aq$, and so in many other instances.

The isomorphism of basic hydrogen with certain basic metals, and more particularly with magnesium, though scarcely established beyond question, is warranted by many facts relating to hydrated silicates. Thus, in several definite silicates, while the proportion of constituent hydrogen is very variable in different specimens, yet, reckoning this hydrogen as basic water, the ratio of the oxygen of the united bases to the oxygen of the silica is almost constant, and identical with the ratio of some typical compound. A variety of talc, for instance, is sometimes represented by the anomalous formula—$Mg_6O_6.Si_7O_{14}.Aq$; but, bearing in mind the temperature required to render such talc anhydrous, it can scarcely be doubted that its proper formula is really

$$H_2Mg_6O_7.Si_7O_{14};$$

or, seeing that the proportion of hydrogen is variable in different specimens, $(HMg)O.SiO_2$.

Prehnite, again, is usually expressed by the formula $Ca_2Al_2O_5.Si_6O_{12}.Aq$; but, independently of the exceptional ratio of lime to alumina, and of base to silica shown by this formula, in reality the proportion of hydrogen or water in different specimens of prehnite varies very considerably. But reckoning the hydrogen or water as basic, the oxygen of the united bases is to the

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* Dr. Odling has kindly given us permission to publish occasional chapters from the forthcoming second part of his "Manual of Chemistry."—Ed. C. N.

oxygen of the silica exactly in the ratio $\frac{1}{1}$, and the oxygen of the triad to that of the joint diad and monad bases approximately in the ratio $\frac{1}{1}$ also. Similarly, in the different varieties of mesotype, there often exists, in addition to the water formulated as water of crystallisation, a variable small excess of water which contributes to furnish the full complement of base appertaining to the particular silicate.

(ι) It is well known that the production of crystals, either by way of fusion or solution, is much affected by the presence of different impurities, dissolved or suspended in the crystallising liquids, and that, as a rule, the finest crystals are obtained from impure liquids, and carry down with them a certain proportion of impurity. Now, native silicates have all the character of crystals formed from impure liquids, and undoubtedly do contain, in many instances, a greater or less amount of accidental impurity. Chabasite, for example, frequently contains an excess of uncombined silica, with which, indeed, it is isomorphous; while some crystals of augite are said to contain a small proportion of garnet; and similarly in several other cases. Moreover, it is not improbable that certain definite silicates may contain variable proportions of other silicates—that a spinelle-silicate, for instance, may contain some cryolite-silicate, and a metasilicate some sesqui-silicate—in a state of perfect homogeneity, and without affecting their special crystalline forms; just as the heteromorphous alloys, Sb_2Zn_3 and Sb_2Zn_2, may each contain a not inconsiderable proportion of the other, without prejudice to the distinctive character of their respective crystalline forms. Altogether, while the analysis of artificial compounds is habitually preceded by their elaborate purification, that of native silicates is performed at once upon compounds never free from more or less accidental impurity, which, occurring in an otherwise definite silicate, may interfere very seriously with the right interpretation of the results of its analysis.

(ζ) Lastly, considering the number of operations to be performed, and of precautions to be taken, in order to obtain an accurate estimation of some one constituent only of an ordinary silicate—such as the alumina, or magnesia, or soda—it is evident that the difficulty and complexity of the processes employed in the complete analysis of most native silicates must be considered as detracting somewhat from the absolute certainty of even the percentage results arrived at. To this consideration may be added the probability, insisted upon by Laurent, that the few tenths or even hundredths of water contained in many silicates, and frequently disregarded both in the statement of results and calculation of formulæ, may sometimes, at any rate, fulfil a very important function in the constitution of the several minerals, and permit the association of their respective formulæ with those of well-recognised typical compounds.

Bearing in mind, then, the many circumstances interfering with the accurate determination of their molecular composition, it would appear that the actual types of native silicates are much fewer and simpler than is generally supposed; and, that in the great majority of instances, where the chemical formula of a mineralogically well-defined silicate is deduced from the analyses of a considerable number of different specimens, the ratio of the oxygen of the alumina to the oxygen of the other bases will prove to be either the spinelle or the cryolite-ratio, or occasionally that of some closely related compound; while the ratio of the oxygen of the silica to the oxygen of the united bases will prove to be that of an ortho-, para-, meta-, or sesqui-silicate; and that in the majority of instances, where these ratios are seemingly departed from to some extent, the departure will be found really due to a partial substitution of alumina for some stronger base on the one hand, or for some silica on the other; or to a nonrecognition of some constituent water; or the presence of some accidental impurity or intermixture. Altogether it is evident that a considerable latitude must for the present be permitted in the assignment of formulæ to complex native silicates, and especially to those of which but a few specimens have been submitted to careful analysis.

On the Purification of Chloride of Ammonium by Volatilising it in a Vacuum, by Prof. J. S. Stas.

The following is the method employed for the volatilisation of sal ammoniac in a vacuum:—

Forty grammes of chloride of ammonium obtained by the direct combination of hydrochloric acid with the ammonia produced by the reduction of nitrite of potassium, were introduced into a glass tube, ninety-five centimetres long and three centimetres in diameter, closed at one end and open at the other. The chloride, first well dried, being placed at the closed end, I placed the tube in a horizontal position on the metallic support of a gas jet; I put the open end against another open tube of the same diameter, and placed over the junction a glass tube of ten centimetres diameter, which I fixed with a cement of gum lac, letting some of the cement run between the tube and the two juxtaposed portions. To the second tube there was joined a T-shaped tube, whose very short, almost capillary branch went to the branch of a steel tap in which I luted it hermetically with a resinous cement. The vertical branch of the T tube plunged into a test-tube containing mercury. Into the other branch of the tap I cemented a capillary tube in communication with a pneumatic machine capable of making a vacuum in the apparatus of 0·0005 m. To prevent the sal ammoniac, which, in a vacuum, condenses as an impalpable powder, from penetrating into the almost capillary tube and blocking it up, I filled the large tube, to which the T tube was joined, with a brush of fine platinum wires, preceded by a large quantity of calcined and still warm asbestos. Before commencing the sublimation of the sal ammoniac, I made sure that the apparatus kept a vacuum for twenty-four hours. I then proceeded to the volatilisation. For this purpose I heated directly in a gas flame the part of the tube containing the sal ammoniac, *taking the precaution of keeping the temperature as low as possible.* During the sublimation the mercury continually oscillated in the vertical branch of the T tube, and the chloride condensed partly as dust and partly as a thick colourless ring. After the tube was cool enough for all tension to be removed from the sal ammoniac, the mercury rose in the tube to the same level as before the operation—a proof that no gas was formed during the sublimation but what was condensed afterwards. I made a second and then a third volatilisation of the same sal ammoniac. During the two latter volatilisations I took care to keep the air-pump continually at work, so as to carry off any gas that might be produced.

After the tube was quite cold, the sal ammoniac, which was sublimed in a compact ring, detached itself noisily from the tube, becoming at the same time opaque instead of transparent and highly refracting, as it was whilst hot.

I determined separately, and as they came from the tube, the proportional ratio of the sal ammoniac in a compact mass and in impalpable dust. Both contained traces of sodium, which, however, could only be appreciated by spectral analysis; they probably acquired this metal from the ordinary white glass tube, in which the triple volatilisation was performed.

On the Direct Production of Stilbene from Bitter Almond Oil, by C. Greville Williams, *F.R.S.*[*]

Stilbene was obtained by Laurent by the distillation of hydride of sulpho-benzoyle, according to the equation—

$$\underbrace{8C_7H_6S}_{\text{Hydride of sulpho-benzoyle.}}=2CS_2+3H_2S+\underbrace{2C_{14}H_{12}}_{\text{Stilbene.}}+\underbrace{C_{26}H_{18}S}_{\text{Thionessal.}}$$

The simplicity of the relation between hydride of benzoyl and stilbene made me conceive that the latter might be produced directly from the former; thus—

$$2C_7H_6O+4Na=C_{14}H_{12}+2Na_2O.$$

Experience has completely confirmed this supposition. The reaction, however, as might be expected, does not take place without the formation of other products. In fact, the amount of stilbene produced is so small, and the difficulties in the way of the separation of the substances produced are so considerable, that I should have delayed publishing my results in their present state had I not seen that Claus[†] is working in a somewhat similar direction. However, as he employs sodium amalgam and an etherial solution of hydride of benzoyl, instead of hydrocarbons, he obtains bodies containing oxygen. One of the substances produced in the manner indicated is the salylic acid, $C_7H_6O_2$, of Kolbe and Lautemann, and the other appears to be identical with Church's dicresol, C_7H_7O.[‡]

To obtain stilbene I treated the bitter almond oil of commerce with an equivalent quantity of sodium, and distilled the mixture at a temperature sufficiently high to bring over everything volatile. The distillate was again treated with sodium and fractionally distilled. One portion came over below 200°, and contained volatile liquid hydrocarbons, among which benzol was observed. The fraction distilling between 200° and 244° did not yield any crystals, even when exposed to a freezing mixture of ice and salt. That portion of fluid which distilled between 244° and 265° became nearly solid on cooling. Above 265° the distillate consisted chiefly of crude stilbene. The fluid distilling between 200° and 244° contained carbon 85·3, hydrogen 8·1, oxygen 6·6. It was apparently a mixture, and probably contained a small quantity of stilbene in solution.

The solid substance was dissolved in hot benzol, and on cooling gave a crop of beautiful colourless prismatic crystals, which, when perfectly freed from benzol by exposure for some time to a temperature of 100°, gave the product *a*. The mother liquid, on standing, gave a second crop *b*, which was freed from benzol in the same manner as the first. The third crop *c* was only purified by pressure between folds of filtering paper. The residue, evaporated to dryness and sublimed, gave crop *d*. The melting points were as follows:—

a	113°
b	116°
c	78°
d	100°

The second crop was burned with oxide of copper and oxygen gas, with the annexed result:—

0·2227 gramme of crop *b* gave
0·7642 " carbonic anhydride, and
0·1374 " water.

Or, per cent.:—

	Experiment.	Calculation.	Stilbene.	
Carbon	93·59	93·33	C_{14}	168
Hydrogen	6·85	6·67	H_{12}	12
		100·0		180

Agreeing, therefore, with the formula $C_{14}H_{12}$, which is that of stilbene.

The melting point of the specimen analysed was 116°. Laurent does not give the melting point of stilbene, but merely states that it fuses several degrees above 100°.

The vapour density of stilbene as given by Laurent is 8·4, a number which is entirely incompatible with the formula. Therefore, although I only had a very minute portion (less than two decigrammes) of stilbene left, I resolved to repeat the vapour density determination, feeling sure that even if the experimental result was not so accurate as might be desired, it would still be a sufficient approximation to indicate the true formula of the substance analysed. The experiment was made in an atmosphere of mercury vapour, with the annexed result:—

Excess of weight of balloon	0·1371 gramme.
Temperature of vapour	350°
Temperature of air	17°
Pressure	764 m.m.
Capacity of balloon	95·5 c.c.
Residual air	12·5 c.c.

Experiment.	Calculation.
6·024	6·228

The large amount of residual air, and the smallness of the scale on which the experiment had to be made, make me regard this experiment as one requiring to be repeated. It is, however, quite near enough to the theoretical value to show that the number obtained by Laurent was due to some error of experiment.

It is important to observe that the specimen of stilbene which fused at 116° had its melting point raised to 120° by keeping it for some hours at a temperature of 100°. It is evident, therefore, that the fusing point of stilbene is not lower than 120°.

Recent Analysis of the Montpellier Saline Chalybeate (Kissingen) Spring at Harrogate, by Dr. Sheridan Muspratt, *M.D. (Hon.), F.R.S. Ed., M.R.I.A., &c.*[*]

As there have been so many strange analyses, some most conflicting, of the water of the above celebrated spring, I have for some months been engaged with experiments and researches upon it; and feeling that the results elicited are now the true ones, I place them before the readers of your ably conducted journal. In

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[*] Communicated by the author.

[†] *Ann. der Chem. und Pharm.* cxxxvii. 92. "Ueber die Einwirkung von Natriumamalgam auf Benzoylwasserstoff in ätherischer Lösung."

[‡] *Ibid.* cxxviii. 301.

[*] Communicated by the author.

the Chemical News for June 29, of last year, it is stated that this Kissingen spring holds the following:—

Carbonate of baryta....................	7·657
Carbonate of strontia..................	2·815

Neither of these earthy carbonates is contained, as such, in the water. The barium exists as a chloride—*i.e.*, in the same form as it does in the "Dr. Muspratt chalybeate, or chloride of iron spring." Annexed is the new analysis, collaterally with that of my friend Dr. Hofmann:—

	Grains in the imperial gallon.	
	1854. Dr. Hofmann.	1867. Dr. Muspratt.
Carbonate of iron........	2·790	3·719
Carbonate of lime........	—	21·011
Carbonate of magnesia...	41·796	2·074
Carbonate of manganese..	trace	trace
Chloride of sodium......	656·838	700·500
Chloride of calcium.....	159·278	168·450
Chloride of magnesium..	35·635	81·560
Chloride of potassium...	11·383	6·916
Chloride of barium......	—	6·364
Chloride of strontium...	—	traces
Chloride of lithium......	—	traces
Silicic acid.............	0·947	0·438
Ammonia, &c...........	traces	traces
	908·667	991·032

Cubic Inches of the Gases in One Gallon of the Water.

Carbonic acid...................	24·17	21·33
Carbide of hydrogen..........	2·40	2·74
Oxygen.........................	·51	·77
Nitrogen........................	6·48	5·92
	33·56	30·76

The quantity of chlorine in the gallon was estimated by my assistant, my brother Edmund, five other chemists, and myself, and the mean (by weight and volumetrically) was 596·472 grains per gallon. The total amount of chlorine, in June, 1865, was 510·17 grains per gallon; in March, of this year, I found 592·5 grains. From the recent analysis, the water is much stronger in its saline ingredients; besides, it has acquired others (chlorides of barium, &c.) that did not exist in it previously. When the late Mr. West, of Leeds, analysed the water from this spring many years ago (1844?), he gave 20 grains of sulphate of soda in the gallon. If this salt was present then—it could not possibly be there with chloride of barium—it is not now found in any of the strongly impregnated waters of Harrogate, "the queen of northern spas," as justly styled by Dr. Granville. The springs to which Harrogate owes its celebrity exceed in number those of any other place in the kingdom.

College of Chemistry, Liverpool, May 1.

On the Direct Estimation of Boracic Acid, by Professor F. Wöhler.

In order to estimate directly the boracic acid contained in datolite, $3(CaO,BO_3)+3CaO,3HO,4SiO_3$, place the mineral in a small tubulated retort, decompose it with hydrochloric acid, and distil the mixture to dryness; pour on to the residue the distillate (which contains boracic acid), and allow it to digest to separate the silica. In the liquid precipitate the lime by means of oxalate of potash, taking care not to add it in too great excess. Then, after filtration and concentration, precipitate the boracic acid in the form of double fluoride of boron and potassium. For this purpose, add a little potash to the material in a platinum capsule, then pour over the mixture a slight excess of hyprofluoric acid, and evaporate the solution to dryness. To remove the other salts it suffices to treat the mass with a moderately concentrated solution of acetate of potash; then allow it to digest and throw on to a filter the double fluoride of boron and potassium, and wash it with the same solution of acetate. Then wash with dilute alcohol to remove the acetate of potash; the double fluoride is then dried at 100° C, and weighed.

On Determining the Proportional Relation between Silver and Chloride of Ammonium, by Professor J. S. Stas.

The method of determination I used is that described in my former article. To prevent the loss of hydrochloric acid, which would have been set free by introducing chloride of ammonium into a hot acid solution of nitrate of silver, I neutralized with pure ammonia the excess of nitric acid used for dissolving the silver. The following is the manner in which I proceeded:—

After having added to the solution of silver in nitric acid 100 *cubic centimetres of water for each grain of metal* dissolved, I poured in, drop by drop, a solution of pure ammonia. When the liquid was alkaline, I neutralised it by a suitable addition of *pure acetic acid.*

In order that the double decomposition might take place under absolutely identical conditions, I neutralised with ammonia both the silver solution to be precipitated while cold, and that at 100°. In order that the solution should be about 100° at the moment of the double decomposition, I kept the flask or globe in which the assay was made in boiling water *for two hours*, and without removing it from the bath I introduced the chloride of ammonium. As the precipitation took place at a high temperature, the liquid cleared itself the moment a complete mixture of the reagents had taken place.

I weighed in air[*] the chloride of ammonium and the silver employed, assuming, according to Prout's hypothesis, the weight of the molecule of chloride of ammonium to be 53·50, and that of silver 108·00. The excess of the metal remaining in the liquid after the double decomposition was determined without removing the flask or globe from the bath. For this purpose I directed a pencil of yellow light to the surface of the liquid in which I wished to measure the silver. The comparative assay, made at the ordinary temperature, was performed by means of the apparatus for *titration*, described on page 137.

The following table contains the results of these three sets of experiments. I have added three determinations taken from my former work; they were made with chloride of ammonium produced at the ordinary temperature by combining directly solutions of ammonia and hydrochloric acid:—

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[*] To reduce to a vacuum the chloride of ammonium weighed in air, I weighed in the air and *in vacuo* a portion of the sal ammoniac that I intended to use. These assays convinced me of a fact that had already been stated by M. Marignac—viz., that the augmentation of weight obtained by weighing the pulverulent chloride directly *in vacuo* was always less than would have been calculated from the density. I found that 100,000 parts of pulverulent chloride weighed in air represented from 100,077 to 100,084 parts of the same compound weighed *in vacuo*. M. Marignac gives the figure 100,080 as the mean of the extremes. These weighings also showed me that the density of compact sal ammoniac differs appreciably according as it is transparent and vitreous, or opaque and amorphous.

Proportional Relation between Silver and Chloride of Ammonium.

Number of the experiment.	Weight of the sal ammoniac reduced for a vacuum.	Weight of the silver reduced for a vacuum.	Weight of the excess of silver after the double decomposition.	Chloride of ammonium which is equivalent to 100,000 parts of silver.
First Series.—Chloride produced at the Ordinary Temperature by combining Hydrochloric Acid with Solution of Ammonia, determined at the Ordinary Temperature.				
	gr.	gr.	gr.	
IX.*	11·0088	22·2236	0·0300	49·600
X.*	10·92896	22·06734	0·0280	49·599
XI.*	12·26038	24·74991	0·0305	49·598
Second Series.—Chloride sublimed at the Ordinary Pressure, Determination made at the Ordinary Temperature.				
	gr.	gr.	gr.	
I.	11·79643	23·8133	0·0290	49·598
III.	11·80844	23·8376	0·0290	49·597
V.	6·25216	12·62116	0·0140	49·593
VI.	10·71756	21·6355	0·0262	49·597
Third Series.—Chloride sublimed at the Ordinary Pressure, Determination made at 100° Centigrade.				
	gr.	gr.	gr.	
II.	39·62130	79.98313	0·0970	49·5974
IV.	13·40631	27·06320	0·0355	49·602
VII.	7·60107	15·3442	0·0187	49·597
Fourth Series.—Chloride sublimed in a Vacuum, Determination made at the Ordinary Temperature.				
	gr.	gr.	gr.	
VIII.	13·5129	27·2784	0·0355	49·598
IX.	6·2250	12·5663	0·0140	49·592

The results given in the preceding table prove that, within the limit that must be allowed in making the experiments, *temperature exercises no influence upon the composition of chloride of ammonium or of chloride of silver; they prove further that pressure is without any influence upon the composition of chloride of ammonium.* In fact, whatever may be the mode of preparation of the compound of ammonium, and the temperature at which the double decomposition takes place, *its proportional relation to silver is constant.*

If the admitted constancy of the stable chemical combinations required to be demonstrated, it seems to me that the almost absolute identity of the results of the four series of determinations is sufficient to prove it. This constancy is the more remarkable, since sal ammoniac can, as I have observed, condense ammoniacal gas or hydrochloric acid, *in the same way that a number of bodies condense gases and vapours completely foreign to them in their constituent elements.*

Among the twelve determinations given in the table there is one, No. II., which was made upon a quantity of material such as was never before employed in an experiment of this kind. I had a double object in using such large proportions. I wished to render sensible the influence of temperature upon the composition of chloride of silver, if there were any such influence; and then, as I was working with chloride of ammonium that had been sublimed three separate times, twice in vessels of *hard glass*, not attacked by the vapour of chlorine, it should be of an extraordinary purity,* and I ought to be able to deduce an important consequence with reference to the hypothesis of Prout. Now, after the double decomposition had taken place upon the weights calculated according to Prout's hypothesis, there remained dissolved in the liquid 0·097 gr. of silver, being a quantity *one hundred times* greater than that which I could have appreciated in the mass of liquid, and certainly *fifty times* greater than it is possible to measure by taking the trouble.

I invite those who think they can attribute "to errors of observation" or "to the impurity of the materials"* the differences observed between the experiment and Prout's hypothesis, to take the trouble of repeating, under the conditions necessary for exactitude, the determination of the proportional relation between chloride of ammonium and silver, and I shall wait with entire confidence the result of their investigation.

In speaking thus I do not pretend that the figures I have given are absolutely exact—that is to say, that they may not be affected by a constant error. I am even sure of the centrary, and in the conditions in which I was placed the constant error must have attained its maximum. In fact, the operation for deducing the proportional relation between the chloride and the silver involves an uncertainty to which I have already drawn the attention of chemists in my "*Recherches sur les Rapports Réciproques des Poids Atomiques.*" This uncertainty consists in the fact that an argentiferous liquid, from which nearly all the metal has been precipitated by a solution of chloride of potassium, sodium, or ammonium, but which still contains one or two milligrammes of silver per litre, precipitates both on the addition of a normal solution of silver and of alkaline chloride. I find this phenomenon is the more pronounced the less acid the liquid is, and the more alkaline nitrate it contains. Now, in the double decompositions between nitrate of silver and chloride of ammonium, I was obliged, for the reason given above, to neutralise the excess of nitric acid by ammonia, and, for the same reason, I only added a slight excess of acetic acid. The conditions, then, which are the primary cause of the uncertainty, are both present, and they must, as I have said before, bring the constant error to a maximum. But after allowing very largely for this constant error, there remains such a considerable difference between the calculated and observed results, that it is quite impossible to attribute it to any other cause than to the inexactitude of Prout's hypothesis.

TECHNICAL CHEMISTRY.

On the Discovery of Sulphate of Strontium in Upper Silesia, and its Application in Agriculture, by Geo. Lunge, *Ph. D.*†

The following fact may not be quite uninteresting to such of the readers of this journal as work in agricultural chemistry. In a locality in Upper Silesia there is found a stratum of an earthy mass, similar in colour to chalk, but crystalline under the microscope, which had been taken by the farmers of the neighbourhood for a kind of marl containing gypsum. Many hundred tons of it have been used for years as a manure, and with the best success. Quite lately Professor Krocker, of Proskan, got a sample of this mass, and found it to contain—

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* These experiments are taken from my former work; the numbers of the experiments have been retained in the table.

* *Cosmos*, vol. xvii. p. 653.

† Communicated by the author.

	Per cent.
Sulphuric Anhydride	36·00
Strontia	46·57
Lime	1·80
Magnesia	1·60
Potassa	0·50
Chloride of sodium	0·25
Carbonic anhydride	1·40
Phosphoric anhydride	0·10
Silicic anhydride	2·10
Alumina and ferric oxide	3·60
Clay, sand	4·28
Moisture and organic substances	1·80
	100·00

The mineral is, then, essentially sulphate of strontia, and it is remarkable that it has proved of some value as a manure, notwithstanding the very slight solubility of that body, and although it cannot be supposed that strontium can absolutely substitute calcium in the plants.

On the Waste of Materials in the Alkali Manufacture, by James Hargreaves.*

In the manufacture of carbonate and hydrate of soda from its sulphate there is a very considerable loss of material, which renders the actual produce very considerably less than is indicated by theory. This loss of course varies in different manufactories, according as care and skill are exercised to reduce it, or the work is carried on carelessly and at haphazard. The reduction of this loss is not only of individual, but of national importance, inasmuch as the glass, soap, paper, and other manufactures having the most immediate influence on civilization and comfort, depend for their existence and extension on a large and cheap supply of this alkali. The usual method of estimating the alkali produced from the crude sulphate of soda or "salt cake" is to multiply the weight of soda ash produced from every 100 tons of salt cake by the percentage of alkali contained in it, and divide these integers by some standard number—say, 48, 50, or 52, which gives the number of tons of ash of the standard strength from each 100 tons of salt cake used. Theoretically, 100 tons of salt cake, containing 96 per cent. of sulphate of soda, or 42 per cent. of the base, should produce 84 tons of soda ash of 50 per cent. The following list of practical results shows how different manufacturers fall short of the theoretical yield:—

	Ash from 100 parts of salt cake.	Standard percentage of ash.	Equal to alkali produced per cent.	Total loss per cent.
Soda ash	75	50	37·5	10·72
	70	52	36·4	13·44
	70	50	35·0	16·66
	66·66	52	34·67	17·46
	70	48	33·6	20·00
	66·66	50	33·33	20·65
Caustic soda	57·5	60	34·5	17·86
	55	60	33·0	21·20
	50	60	30·0	28·57

The sources of this loss are as follows:—

1. **By soda salts carried mechanically into the flues and chimney.**—I am not aware that any attempts have ever been made to ascertain the quantity thus carried away, but it must be very considerable, more than is generally suspected. Where salting pans are used, and the hot gases from the black-ash furnace are passed over the crude soda solution to partially carbonate and dry it to "salts," this salt and the "red liquor" drawn along with it generally contain from 0·25 to 1 part of sulphate of soda to every 100 parts of available alkali, more than is contained in the crude soda liquor when run into the pan; the proportion varying with the elevation of the furnace bed, the intensity of the draught, and the mechanical division of the sulphate. But this does not represent the whole of the sulphate thus carried away unutilised, the finer particles being carried beyond the pan into the flues and chimney. In the "finishing furnace," where the "black-ash salt" is heated to dull redness, to expel water, burn out carbonaceous matters, and oxidise the sulphide of sodium present, converting it into sulphate of soda; there is also a small quantity carried away in this manner, as is shown by the glazing of the bricks and deposit of alkaline dust in the flues.

2. **By volatilisation of sodium salts.**—A more considerable loss of material is sustained by the volatilisation of sodium, principally as sulphide and carbonate. The workman generally prefers to work with his furnace very hot, so as to get out his complement of "balls" as soon and with as little labour as possible. The temperature of the furnace is generally below the boiling point of any of the sodium salts present, but when a rapid current of hot gas is passing over and given off from the materials, these salts are absorbed and carried off as vapour, the action of the hot gas in this case being analogous to that of a current of air in drying up water or other volatile fluids when far below their boiling points. When the door of the lower bed of the furnace is opened while the temperature is high, a white cloud, containing sulphate and carbonate of soda may be seen rising from the bed. The sulphate is produced by the oxidation of volatilised sulphide, in consequence of the in-rush of cold air through the door, which at once oxidises the sulphide and condenses the alkaline vapours by reduction of temperature. The practical difficulties of the work have so far prevented the estimation of this and the former sources of loss, but comparison of the materials put into and drawn from the furnace leave no doubt that it is very considerable, and varying with the temperature at which the furnace is worked. There is no advantage except that of saving labour and time to the workman in having the temperature higher than is necessary to reduce the sulphate of soda, and cause the sulphide of sodium and carbonate of lime to react on each other. When the temperature is maintained too high, there is not only a great loss by volatilisation, but the increased quantity of fuel used for the purpose is thrown away, the furnace is prematurely worn out, and there is formed at very high temperatures an allotropic sulphide of sodium, which does not react on the lime present in the charge.

3. **By combination of soda with the materials of the furnace.**—This is another, but comparatively small, source of loss. The bricks, &c., taken from the furnace while undergoing repair hold in combination a considerable amount of soda, being in some pieces 6 per cent. and upwards, but in an insoluble condition, and therefore unavailable. The fused sodium salts which have filtered into the interstices of the furnace are also of no practical utility.

4. **By the formation of insoluble compounds of soda,** which, by rendering impossible the extraction of the alkali by lixiviation, still further diminishes the yield. The small coal, which is used as a reducing agent,

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* Communicated by the author.

invariably contains more or less ash, varying from 1·5 to 8 per cent. Of this ash silica and alumina form a large proportion, seldom less than 80 per cent. of the whole. The salt cake contains from 0·12 to 0·5 per cent. of silica and alumina, and the limestone from 0·2 to 3 per cent. These form with soda a compound silicate, which is sparingly soluble in a solution of caustic soda and sulphide of sodium. When the crude soda solution is oxidised, and the sulphide converted into hyposulphite, sulphite, or sulphate of soda, or when it is exposed to a temperature of 212° F., the alumino-silicate is precipitated. In the oxidising apparatus described in the Chemical News, No. 340, the precipitate is in the form of a white powder; and in oxidising towers, where the solution is oxidised by running it over pieces of coke, to expose a large surface of the fluid to the action of the atmosphere, the alumino-silicate is deposited on the coke, filling up its interstices, and if not frequently disturbed by taking the coke out at the bottom of the tower, washing and returning it to the top, the whole is in time converted into a firm cohesive mass. Another compound silicate of soda and lime is formed when the silica is in excess of what is required to form the alumino-silicate, and is quite insoluble. The loss from this cause is variable in proportion to the quantity of alumina and silica introduced along with the materials used and the quantity of these substances taken from the bricks of the furnace.

5. **By non-decomposition or imperfect decomposition of sulphate of soda.**—The loss from this cause varies considerably in different manufactories. In some the average quantity of sulphate, or its equivalent in sulphate with sulphide, &c., is as 5 of sulphate to every 100 parts of available alkali, which is equal to a loss of 2·19 parts for every 100 parts of sulphate made available. In others (where the works are too small to afford, or the managers are too "economical" to employ, scientific supervision) the loss not unfrequently averages 24 parts of sulphate to every 100 parts of available alkali, by which 9·22 parts of the sulphate remain unavailable for conversion into carbonate for every 100 utilised. The loss from this cause generally varies between these proportions, more frequently exceeding the latter than going below the former.

The sulphate may remain unaltered in consequence of deficiency of carbonaceous matter, too low temperature of the furnace, or insufficient or unskilful working; or the decomposition may have only proceeded so far as to produce sulphide of sodium, without reacting on the carbonate of lime used in mixing the charge. This is the result of either a deficient quantity of carbonate of lime or bad working in the furnace.

When the crude soda or "black ash" is exposed to too high a temperature, an allotropic modification of sulphide of sodium is formed. This modified sulphide does not react on the lime present in the charge, but remains unchanged, and the black ash, instead of a grey, is of a dull brick-red colour, and forms a blue solution with water. When chlorine is passed through a solution from this red ash to convert the sulphide into sulphate, there is only a small portion so converted; the rest of the sulphur from the sulphide is precipitated—not being soluble in the alkaline solution—and may be separated by filtration. By this it is obvious that there is no dependence to be placed on the use of chlorine to convert the whole of the sulphide into sulphate, when the black ash is "burnt," and the loss shown by analysis is less than the real loss.

6. **By Oxidation of crude soda.**—There are some manufacturers who think nothing of allowing the black ash to be exposed for several days to the weather before being lixiviated. When the weather is dry no harm is done, but in wet weather the balls are moistened, and the sulphide of calcium is rapidly converted into sulphite, hyposulphite, and sulphate of lime, which, reacting on the carbonate of soda, forms the corresponding soda salts, while the lime is converted into carbonate. To avoid this the black ash must be kept quite dry, and not kept too long a time, but it should have sufficient time to become quite cold.

This action is also frequently continued in the vats, which being filled too full with black ash, a great quantity is exposed to the air in a wet condition. Each vat should not be filled so full but that the whole of the black ash can be covered with water at once, and never allowed to be uncovered until the vat is spent and run off. The vat liquor contains more sulphate in proportion to the available alkali than is contained in the black ash as drawn from the furnace, in consequence of this oxidising action of the atmosphere. The loss of alkali from this cause is seldom less than 0·2 per cent. of the whole; but I have frequently seen black ash in which the parts most exposed to the weather have had more than one-third of the alkali reconverted into sulphate of soda.

7. **By inversion of the furnace reaction.**—When the water used in lixiviating the black ash is too hot, the furnace reaction is inverted, the sulphur combining with the sodium, and the oxygen and carbonic acid with the calcium—

$$NaOCO_2 + CaS = NaS + CaOCO_2.$$

This reaction takes place more rapidly in weak than in strong solutions, and is therefore more apt to occur where the water is run on the weak vat warm, than when it is warmed by steam in each vat separately, raising the temperature a little in each successive vat till it reaches the strongest one.

If the vats are not perfectly cleaned from the waste from former charges every time they are discharged and filled, the waste remaining, and which has been exposed to the atmosphere, becomes partially oxidised, reacts on the soda solution, and forms soda salts of the sulphur acids, and the whole of the soda thus combined is for all practical purposes lost.

8. **By imperfect lixiviation.**—If an insufficient quantity of water is run through the vats, or the water is too cold, or the vats are worked too rapidly, or the black ash is thrown into the vats in too large lumps, or is deficient in porosity, a considerable quantity of alkali is left in the waste, and thrown away with it.

This loss is greatly increased by having too small a space for lixiviating the black ash, which allows too short a time for extracting its soluble constituents, and necessitates the use of hot water, thereby offering the alternatives of sustaining loss by decomposition of carbonate of soda, or by leaving soda in the waste, or needing a great expense to concentrate the liquor; for if hot water is not used a much larger quantity of water is required to extract the alkali, and as the solution contains a smaller proportion of alkali, the use of a larger quantity of fuel is required to concentrate it. These evils are best overcome by having ample vat space.

The waste should not contain more than 0·1 per cent. of alkali, which is equal to about 0·45 per cent. of the whole alkali originally present in the black ash. But it not unfrequently amounts to ten or even fifteen times this amount where there is no proper supervision of the vats and analysis of the waste.

9. **By spilling, leakage, &c.,** in moving material from place to place. This is a mechanical rather than a chemical question, and how to prevent it is too obvious to require comment.

The foregoing shows that, without any alterations in the principle of Le Blanc's process, there is still a large margin for improvement in the details of the soda manufacture.

Appleton-in-Widnes.

Notes on Thallium and Magnesium Alloys, by S. Mellor, *Esq., Manager of the Magnesium Metal Company.*[*]

It having been suggested that if an alloy of thallium and magnesium could be easily made into wire it might be found to burn readily and to produce an intense bright green flame, which, from its portability, would be well adapted to some of the purposes for which a green flame is required, some experiments have been made with this end in view.

It was found that thallium alloys most readily with magnesium, and in any proportions. The alloys are very stable, and are easily worked up into wire and ribbon. Alloys containing 5, 10, 15, 20, 25, and 50 per cent. of thallium were prepared. These all burn brightly and steadily, but the flame is smaller and the combustion slower than that of pure magnesium. The flame is cold, and the heat-conducting property of the alloy, compared with magnesium, is sensibly diminished, showing the change in the molecular construction of the metal. The smoke produced in the combustion of these alloys is more dense, and as it curls gracefully away it is seen to be fringed with a rather pretty dark purple tint; but the magnesium light is so very intense that it almost completely masks the thallium flame, so that it is not observable in some of the alloys—indeed, the green light is scarcely recognisable even in an alloy containing 50 per cent. of thallium.

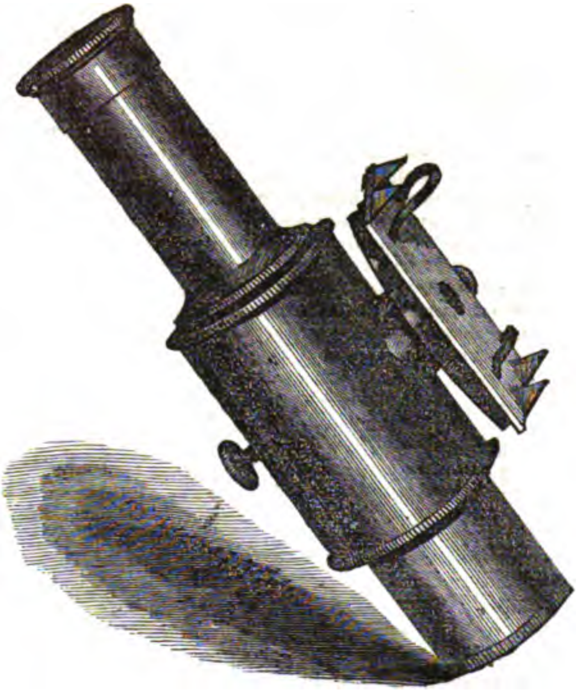
An alloy of 5 per cent. of thallium appears to render magnesium less brittle and more ductile than pure magnesium is usually produced; but the higher alloys of thallium, say those containing 25 and 50 per cent. of thallium, are more oxidisable than pure magnesium.

The metals were put together cold in a closed iron crucible; only a slow heat was required to melt them.

PHYSICAL SCIENCE.

On a New Micro-spectroscope, and on a New Method of Printing a Description of the Spectra seen with the Spectrum Microscope.

In the Chemical News for April and May, 1865,[†] Mr. Sorby, F.R.S., described his application of spectrum analysis to microscopical investigations, and especially to the detection of blood stains. For the purpose for which it was intended, this arrangement was excellent, but in general practice it was in some respects inconvenient. Mr. Browning has recently made for Mr. Sorby a modification of the spectroscope, which is intended to slip into the eye end of a microscope instead of the eyepiece. The instrument is shown in the accompanying figure. It contains a series of prisms arranged for viewing the spectrum by direct vision. The arrangement at the upper part on the right side is for the purpose of obtaining a supplementary spectrum from any object whose spectrum it is desired to compare with that of the object placed on the stage of the microscope. This object may be either a solution of permanganate of potash in a small sealed tube, a cobalt blue glass, or anything else which will furnish a standard spectrum for comparison. There are milled heads with screw motions to adjust the focus of the different parts of the spectrum, and to open and shut the slit vertically and horizontally. Powers of from half an inch to $\frac{1}{20}$th may be employed, and by using a binocular microscope the object may be brought into the field, and examined in the ordinary way through one tube, whilst its spectrum may be observed and compared with that of a standard light by means of the other tube. The object may be illuminated either by transmitted or reflected light, and any of the ordinary accessories may be used for this purpose, such as a chromatic condenser, side reflector, Lieberkuhn, &c.



Mr. Sorby has also introduced a standard spectrum, which he proposes should be used as a scale in all descriptions of spectra, as seen by the spectrum microscope. Mr. Sorby has been good enough to communicate to us the following description of this valuable method:—

The scale adopted is an interference spectrum, produced by a plate of quartz ·043 inch thick, cut parallel to the principal axis of the crystal, and placed between two Nicol's prisms. In this the whole visible space is divided by dark bands into twelve regular divisions, having in all parts the same relation to the physical properties of the light. These are counted from the red end towards the blue, their centres being reckoned as 1, 2, 3, &c., and the thickness of the plate is so adjusted that the sodium line exactly corresponds to 3½. The intensity of the absorption is expressed by the following types:—

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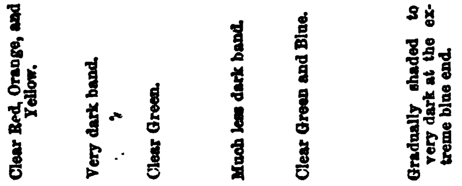
[*] Communicated by the author.

[†] Chemical News, vol. xi. pp. 186, 194, 232, and 256.

Not at all shaded	Blank space
Very slightly shaded	. . . Dots with wide spaces
Decidedly shaded	... Dots closer together
More shaded	... Very close dots
Strongly shaded, but so that a trace of colour is still seen	--- Three hyphens close
Still darker	— Single dash
Nearly black	—— Double dash

Except when specially requisite, only the symbols . . . --- — are employed for the sake of simplicity, and then as signs of the relative rather than of the absolute amount of absorption, and it is assumed that there is a gradual shading off from one tint to the other, unless the contrary is expressed. This is done by means of a small vertical line over the figure (see No. 11), which shows that there is a well-marked division between them. Definite narrow absorption bands are indicated by * printed over their centre. This will be better understood by a description of the spectrum of deoxidised hæmatin.



$$4\tfrac{1}{4}\overset{*}{\text{—}}5 \qquad 5\tfrac{1}{2}\overset{*}{\ldots}6\tfrac{3}{8} \qquad 9\,..\,10\text{---}11\text{—}$$

The following examples will show how simple or more complicated spectra may thus readily be printed and compared. I have chosen solutions of similar tint, in order to show that the spectra of those of nearly the same colour may be very different, or, if analogous, may differ in details, easily expressed by the symbols. The colour of each is given after the name. Nos. 1, 8, 9, 10, 11, 12, and 13 can be kept for a long time, sealed up in tubes, and the rest are easily prepared.

1. Cudbear in alum (pink): 3 . . . . . 8 11 . —

2. Colour of elder berries with citric acid (red pink):—

$$4\,.\,\text{-}\,5\tfrac{1}{4}\text{—}8\text{--}9\,.\,.\,.\,11.\text{-}\text{—}$$

3. Brazil wood with bicarbonate of ammonia (pink):—

$$4\tfrac{1}{2}\overset{*}{\text{—}}5\tfrac{3}{4}\,.\,.\,.\,.\,.\,.\,8$$

4. Logwood with bicarbonate of ammonia (pink):—

$$3\tfrac{5}{8}\overset{*}{\text{—}}5\tfrac{1}{4}\,.\,.\,.\,.\,.\,.\,7$$

The next four are spectra of blood, produced by the successive addition of the various reagents, as in detecting fresh stains.

5. Fresh blood (pale scarlet):—

$$3\tfrac{1}{2}\overset{*}{\text{—}}4\tfrac{3}{8} \quad 4\tfrac{3}{4}\overset{*}{\text{-}}5\tfrac{3}{4} \quad 7\,.\,.\,8\text{-}9\text{—}$$

6. Citric acid then added (pale brown):—

$$1\tfrac{5}{8}\overset{*}{\ldots}2\tfrac{1}{4} \quad 4\ldots8\ldots9\text{---}10\text{—}$$

7. Ammonia then added (pale brown):—

$$3\tfrac{5}{8}\overset{*}{\ldots}4\tfrac{3}{8} \quad 4\tfrac{7}{8}\overset{*}{\ldots}5\tfrac{5}{8} \quad 7\,.\,.\,8\text{---}10\text{—}$$

8. Deoxidised hæmatin, from blood stain two years old (pink):—

$$4\tfrac{1}{4}\overset{*}{\text{—}}5 \quad 5\tfrac{1}{2}\overset{*}{\ldots}6\tfrac{3}{8} \quad 9\,.\,.\,10\text{—}11\text{—}$$

With these may be compared the two spectra which more nearly resemble those produced by blood than any I have yet seen.

9. Cochineal in alum (pink):—

$$3\tfrac{7}{8}\overset{*}{\text{—}}4\tfrac{1}{2}\ldots5\tfrac{1}{8}\overset{*}{\text{--}}6\tfrac{1}{8}\ldots7\tfrac{1}{2}$$

10. Alkanet root in alum (pink):—

$$3\tfrac{1}{2}\overset{*}{\text{—}}4\tfrac{3}{8} \quad 5\tfrac{1}{4}\overset{*}{\ldots}5\tfrac{3}{4}$$

The following spectra of compounds derived from chlorophyll are as complicated as any I have met with.

11. Normal chlorophyll in alcohol (deep green):—

$$\tfrac{7}{8}\overset{*}{\text{—}}2\tfrac{3}{8}\text{--}\overset{|}{3\tfrac{1}{4}}\ldots4\tfrac{1}{2} \quad 6\tfrac{3}{4}\,.\,\text{--}\,7\tfrac{1}{2}\text{—}$$

12. Ditto, as decomposed by acids, or as found in some leaves (olive green):—

$$1\overset{*}{\text{—}}2\tfrac{1}{8} \quad 2\tfrac{3}{4}\overset{*}{\text{—}}3\tfrac{3}{8} \quad 4\tfrac{1}{4}\ldots5\tfrac{1}{4}\overset{*}{\text{—}}5\tfrac{3}{4}\ldots6\tfrac{5}{8}\overset{*}{\text{--}}7\tfrac{3}{8} \quad 8\tfrac{1}{2}\,.\,\text{--}\,9\tfrac{1}{2}\text{—}$$

13. Ditto, as decomposed by caustic potash, and then by hydrochloric acid (red-green, neutral tint):—

$$\tfrac{1}{4}\overset{*}{\text{—}}\tfrac{3}{4} \quad 1\tfrac{1}{8}\overset{*}{\text{—}}1\tfrac{3}{8} \quad 1\tfrac{7}{8}\overset{*}{\text{—}}2\tfrac{1}{8} \quad 4\tfrac{1}{2}\overset{*}{\text{--}}5\tfrac{1}{4}\ldots9\text{--}10\text{—}$$

These instruments and methods were exhibited and explained by Mr. Sorby and Mr. Browning at the last *soirée* of the Royal Society, where they excited the greatest interest.

PHARMACY, TOXICOLOGY, &c.

On Colchicia, by John M. Maisch.*

The collection of chemicals in the Philadelphia College of Pharmacy contains a specimen of colchicia prepared by Mr. Carter in 1857; a portion of this was used for the purpose of clearing up the contradictions in the statements of different authors. The substance is a light yellow amorphous powder, possessing a very faint odour and intensely bitter taste, sparingly soluble in ether, but easily soluble in water and alcohol, the aqueous solution being slightly turbid, most likely in consequence of the decomposition of a small portion into resin and colchicein. Heated upon platinum foil, it fuses; at a higher heat, it takes fire and burns without leaving any residue. Placed upon moistened red litmus paper, the blue colour is restored; very faintly reddened litmus becomes blue also by a concentrated aqueous solution. One drop of dilute sulphuric acid dropped from a bottle giving fifty-two drops to the fluid drachm, consequently about one-eighth of a grain HO,SO_3, when mixed with one grain of colchicia, retained its acid reaction. One drop of the acid was mixed with one fluid ounce of distilled water; in five minims of this mixture, equal to about one-seven hundred and seventieth grain HO,SO_3, one-sixteenth grain colchicia was dissolved, and the solution now had a distinct alkaline reaction on slightly reddened litmus paper; but on heating this solution to the boiling point, it had acquired an acid reaction.

The most important tests for recognising the presence of colchicia are its behaviour to dilute acids and also alkalies, by which its solution acquires a yellow colour, and the violet and blue colour which is produced by oxidising agents with dry colchicia. This latter coloration, which changes through various shades finally into yellow, is strikingly beautiful when concentrated sulphuric acid is used, and immediately some

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* Abstract of a paper in the *American Journal of Pharmacy*, xxxix. 97.

nitric acid or a fragment of a nitrate is added; strong nitric acid produces it likewise, but it changes more rapidly to yellow. Sulphuric acid, with a trace of chromate or bichromate of potassa, or of sesquichloride of iron, or of binoxide of lead, shows the same reaction at the point of contact with colchicia; the liquid itself has a green colour with the first two reagents, owing to their intense yellow colour.

One grain of colchicia was dissolved in one fluid ounce of distilled water, slightly acidulated with muriatic acid; by repeated trials it required 114 drops from this phial to make one fluid drachm; this measure had been carefully gauged with a pipette graduated into $\frac{1}{100}$ c.c. In making the following experiments, a sufficient amount of the reagent was added to enough distilled water to make one fluid ounce, and the solution of colchicia was carefully dropped in until, after stirring, a permanent turbidity was observable. Under these circumstances, it was required of

Mayer's iodohydrargyrate of potassium, 15 drops; turbidity quite distinct.
Sonnenschein's phosphomolybdic acid, 20 drops; turbidity distinct.
Tannic acid, 100 drops: turbidity scarcely observable.

It follows from this that the following amounts of colchicia may be detected by

Mayer's test.......	·01645	grains, or 1 part in	27700	water.*
Sonnenschein's test	·02193	" "	20778	"
Tannic acid........	·10965	" "	4156	"

Solutions of colchicia in water acidulated with sulphuric and with muriatic acid were evaporated and three times taken up by water and again evaporated; the aqueous solutions were finally filtered from the separated resin, and the filtrate slowly evaporated with an excess of carbonate of lead, the residue then treated with strong alcohol and slowly evaporated. Colchicein was obtained in yellowish crystals, which were free from acid and lead. Dissolved in water it still yields precipitates with tannin, phosphomolybdic acid, and iodohydrargyrate of potassium; but neither in solution nor in substance does it produce any reaction on red or blue litmus paper. Rendered faintly alkaline by ammonia, the solution occasions precipitates with the soluble salts of barium, calcium, and lead, which are soluble in dilute nitric acid. Towards acids it behaves similarly to colchicia.

The resinous matter remaining on the filter when colchicein is filtered off was dissolved in alcohol, and the solution evaporated; an amorphous brown-greenish mass was left, in which alcoholic solution has a decided acid reaction. Concentrated nitric acid dissolves it with an evanescent yellow colour; on the addition of sulphuric acid the solution takes place with a purplish brown, rapidly disappearing; pure sulphuric acid dissolves it with a brown colour.

Having looked in vain in every portion of the decomposed colchicia for glucose, or a compound which would reduce an alkaline solution of copper, the observations of Oberlin, Ludwig, and Hübler are confirmed.

Taking all these results together, no doubt colchicia must be looked upon as an alkaloid, the salts of which are soluble in water, but decomposed, with the formation of colchicein, on keeping them in solution as well as on evaporating them. The crystalline mass, obtained by Mr. Carter on evaporating sulphate of colchicia, was undoubtedly colchicein.

Aschoff and Bley observed already that colchicia combines with bases, and that when it is evaporated with a solution of the carbonate of an alkali, the residue contains no carbonic acid. Hübler makes it probable that colchicein is formed under these circumstances. Colchicia is a very weak base, and colchicein, if it can be regarded as an acid, is certainly a weak one, and resembles the alkaloids in its behaviour to some reagents. If colchicia and colchicein have the same composition, the acid resin formed together with the latter can scarcely be different.

In preparing colchicia the action of alkalies and acids, particularly when heat is applied, must be avoided.

FOREIGN SCIENCE.

(From our own Correspondent.)

Paris, May 1, 1867.

I am now enabled to send you a list of prizes to be awarded by the Society of Encouragement, Paris, and their dates. The aim of the Society is the close alliance between practice and theory. It represents invention, improvement, and application. Every new discovery or invention to ameliorate the state of our national industry emanates from this Society. It makes no distinction between the high-placed theorist and the practical man; those who work in a laboratory, in the study of a *savant*, in a workshop, or as labourers, all are equally welcome and of the same grade. The value of the prizes exceeds 6000*l.* The Society does not confine itself, in their distribution, to industrial matters; it extends its recompenses to individuals. Foremen, workmen in manufacture and agriculture, inventors, pupils of industrial schools, persons invalided by work of hand or brain, receive encouragement, recompenses, and *substantial* aid.

Grand Medals.—Gold medals of 40*l.* to French or foreign inventors who have made the most important discoveries applicable to French industry, to be distributed annually in the following order:—1867, Commerce, bearing the profile of Chaptal; 1868, Fine Arts, that of Jean Gougon; 1869, Mechanical Arts, Prony; 1870, Chemical Arts, Lavoisier; 1871, Agriculture, Thénard; Economical and Physical Arts, Ampère.

Grand Prize of 480*l.*—This is given by the Society to the author of a discovery deemed to be the most useful to French industry.

Prize given by the Marquis Argenteuil.—The same amount as the last, awarded to the discoverer of a special invention or improvement, principally with regard to objects in France which have not been yet able to compete with foreign markets, either as to quality or cheapness.

Prizes for 1868, 1869, 1870, 1871, 1872, 1873, *and* 1874.

Mechanical Arts.—Prize of 120*l.* for the best machines for steam navigation, which, with slight draught of water, can enable the machinery and coaling room to be diminished, and thus increase the available space; 1869.

Prize of 120*l.* for a locomotive able to take a goods-train at the rate of thirteen to eighteen miles an hour, with a minimum of expense of prime cost and combustion of fuel; 1870.

Prize of 240*l.*, for a motor engine, from 25 to 100-horse power, burning at the most $1\frac{1}{4}$ lbs. of coal of best quality per indicated horse-power, weighing less than 661 lbs., and costing from 12*l.* to 16*l.* per horse-power; 1871.

Prize of 40*l.*, founded by the Princess Galatzin, for a hydraulic motor for a small workshop, able to work a shaft representing a force of 6 to 20 kilogrammetres* per second; 1868.

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* One fluid-ounce water = 455·669 grains.

*A kilogrammetre is a kilogramme (2·205 lb.) raised to the height of a metre in one second.

Prize of 160*l.* for improvements to be effected in the mechanical weaving of linen and hempen goods; to be awarded in 1872, in favour of the manufacturer who can produce commercially linen threads of a fineness of 100 metres to the gramme, or hemp threads 15 metres. This must be obtained by an economy of at least 15 per cent. of the motive power, and with such a diminution of temperature that there is little or no steam. The manufacturer must have delivered to commerce at least the value of 800*l.* worth of threads of linen or hemp, according to the above stated conditions.

Prize of 120*l.* for a file-making machine able to cut all sorts; 1870.

Prize for a practical and economical means of cutting millstones, whilst diminishing the insalubrity of this branch of industry; 1869, or, if necessary, 1875.

Prize of 80*l.* for a water-meter, acting under a pressure of from 1 to 5 atmospheres with a temperature of 0° to 100° C., and giving the volume of water to within a hundredth part; 1870.

Prize of 40*l.* for a regulator for gas burners.

Chemical Arts.—Prize of 80*l.* for the best process of making oxygen on a large scale; 1869.

Prize of 120*l.* for the industrial application of oxygenated water.

Prize of 80*l.* for extracting the nitrogen of the air in the form of nitric acid or ammonia; 1869.

Prize of 80*l.* for the economical production of cyanide by the nitrogen of the air; 1871.

Prize of 120*l.* to be awarded (1870) to the manufacturer who shall be the first to produce sulphuric acid, quite free from arsenic, from pyrites.

Prize of 40*l.* for the industrial employment of any cheap and abundant mineral; 1868.

Prize of 40*l* for the utilisation of the refuse of factories; 1869.

Prize of 40*l.* for the useful application of the newly-discovered metals; 1870.

Prize of 40*l.* for new applications of simple substances, non-metallic; 1870.

Prize of 40*l.* for the discovery of a new alloy useful in the arts; 1871.

Prize of 120*l.* for the artificial production of graphite for the fabrication of pencils; 1872.

Prize of 120*l.* for the artificial preparation of the compact black diamond; 1873.

Prize of 160*l.* for the discovery of processes capable of furnishing, by any organic transformations, useful substances, such as quinine, indigo, alizarine, or cane sugar.

Prize of 160*l.* for the artificial production of fatty acids and of waxy substances.

Prize of 240*l.* for a theory of cast-steel founded on certain experiments; 1872.

Prize of 120*l.* for the disinfection of the refuse from the purifying of gas; 1869.

Prize of 40*l.* for a process capable of disinfecting and clarifying, quickly and durably, sewage water; 1868.

Prize of 60*l.* for the discovery of an ink which will not rust metallic pens; 1869.

Prize of 120*l.*, 60*l.*, and 20*l.*, for the employment of boracic acid and borax in the ceramic arts; 1868.

F. MOIGNO.

PARIS, May, 8, 1867.

THE learned societies of the Scientific Association of France held their annual public meeting at the Sorbonne, Wednesday, Thursday, and Friday of Easter week. The meetings were not well attended, and the few subjects discussed were not very interesting; the attraction of the Exhibition threw them into the negative pole of sight-seeing or lecturing. The distribution of the prizes took place on Saturday, April 28, at noon. M. Blanchard, Professor of Natural History at the Museum of Natural History, and member of the Academy of Sciences, gave a good summary of the scientific works carried on in 1866–1867. His good nature and lively imagination were actually necessary to conceal or counteract a truly lamentable sterility of subjects. We must say, notwithstanding the rather timid protestation of the Minister of Public Instruction, that the sciences, mathematics, physics, geology, botany, and meteorology have lost in France much of their ground; our mathematicians, physicists, and naturalists have let themselves be outstripped by foreign *savants* on the field of pure science. The Prussian needle-gun and the Enfield rifle have taken the palm out of our hands; they have, we may say, silenced the fire of our rifles.

This is a sad statement, but, alas! too true. The Congress that we have above mentioned was only a gloomy shadow compared with a meeting of the British Association for the Advancement of Science.

Apropos of the ozone-generating machine experimented upon by Mr. Beanes at the last *soirée* of the Royal Society of London, let us humbly call to mind that we were the first to make known the nature and application of this mysterious agent. In 1845, on the first news of the curious observations of M. Schönbein, we proceeded to Basle, and visited the celebrated chemist and professor. He condescended to repeat before us his numerous experiments, and we wrote to the *Epoque* a letter inserted on Dec. 31. The following very important passage occurs: "It is necessary to return immediately to the ideas of Ampère, and consider the atoms of bodies as having two states—first, with the essential primitive electricity or in a nascent state; second, with their electricity more or less disseminated, or their atmosphere of electricity in a neutral state. The ozone of M. Schönbein is, in our eyes, only a molecule of oxygen in a nascent state, with only negative electricity in its atmosphere. I am, I think, able to rigorously prove and account for the wonderful properties of this agent that we cannot lay hold of, and of which so much has been said." We ask all the chemists in general of that time, and Dr. Thomas Andrews, of Belfast, in particular, whether at that period any one had so clearly defined the essential nature of ozone, so much talked about, written upon, and discussed without coming to a decided conclusion.

Two years afterwards, when uncertainty yet reigned in all minds, we inserted in the *Nouvelle Revue Encyclopédique* of M. Didot, in the number for July, 1847, the following more explicit lines: "Sufficient attention has not been yet paid to the important fact that oxygen disengaged by plants is not in a neutral state. We are perfectly convinced that this nascent oxygen, without its positive atmosphere, is the ozone discovered by M. Schönbein, with an odour *sui generis*, and possessing, in the highest degree, all the properties of electro-negative substances. The bleaching of linen stuffs, ivory, wax, &c., in the open air, on grass, the formation of nitric acid and saltpetre, also many other phenomena, are only caused by the powerful action of oxygen in a nascent state, or with its negative electricity developed." From 1845 to 1867, thousands of contradictory opinions have been written on the subject of ozone, to return again to the idea that we so clearly pointed out. We have so often pleaded the cause and defended the interests of others, that we must be pardoned for establishing, once for all, and very humbly, our own claims.

The Society for the Encouragement of National Industry has decided that, during the whole time of the Exhibition, it will hold weekly meetings, not on Wednesdays, as on that day too many members of the Council will be elsewhere engaged, but on Friday, when one is in general more free from domestic or social engagements. The first of these meetings took place on May 3, under the presidency of M. Dumas, and the aspect of the hall, the tables covered with crystal and glass, gas-burners, objects in aluminium, &c., show at once a successful departure from the habits of the Society. The correspondence was, as usual, opened by the two secretaries, but it contained nothing interesting. M. Tessié de Mothay, in his name and that of M. Maréchal, of Metz, read a description of the processes of phototype which have led them to the definitive solution of the great problem of the indefinite reproduction, with thick and indelible inks, of photographic images. M. Tessié regarded as antiquated the anterior essays with regard to M. Davanne, of whom we have already spoken. He need not then speak of the photographic processes of MM. Nièpce de St. Victor, Lerebours, Lemercier, and Barreswill, long since practised by the eminent photographer, M. Lemercier. Meanwhile, M. Barreswill thought proper to call to mind this first solution, and even thought that he was able to add that he had given results almost identical with those of the new process. We venture to affirm the contrary, and we are sure we shall not be contradicted on this point by M. Lemercier, who is very glad to substitute the new process for his own, the success of which was uncertain. The employment of so large a quantity of ether rendered difficult and unhealthy the process, which had already given way to the incomplete method of M. Poitevin.

M. Dumas then gave the *parole* to M. Paul Bérard, who directs, with M. Paul Audouin, the Laboratory of Essay for the Illuminating Power of Gas, established in the Rue du Faubourg Poissonnière for the Municipal Administration of Paris, under the head direction of MM. Dumas and Regnault. The young chemist resumed, and confirmed by many experiments results obtained, having a double view—1. Two flames of equal density being given, one produced by a carcel lamp burning under fixed conditions, the other by a gas burner, burning as much as possible under the same conditions, to determine the respective consumptions of oil and gas, in a given time, for each of the apparatus; 2. To study different burners, and the best conditions for the combustion of the gas.

The first problem was completely resolved by a series of photometric apparatus, very well constructed by M. Deleuil, and which comprise a carcel lamp burning at the normal rate of oil, a Foucault photometer with starched glass plates, and a telescope and movable plates, a standard burner and an argand one with 30 holes, and an automatic balance indicating by a scale, with the precision of 1 centigr., for a charge of 3 kilos., the quantity burned by a carcel lamp. M. Audouin said nothing of the photometric method; he did not even mention the name of M. Deleuil, but he enumerated very rapidly the conclusions of the experiments on burners. Let us mention them, as they are truly well defined. With *bat's-wing burners* the maximum of illuminating powder corresponds to a slit $\frac{7}{10}$ths of a millimetre wide. The same quantity of gas can give, when it burns in a good burner, four times the light given by a bad one. The increase of illuminating power corresponds to a very rapid diminution of pressure, and consequently to the diminution of the velocity of flow; in other terms, with equal consumption of gas of a constant composition, the greatest illuminating power corresponds to the lowest pressures, the maximum corresponding to a pressure of 2 to 3 millimetres. The proportion between the diameter of the nipple and the expenditure, keeping the same width of slit, $\frac{7}{10}$ths of a millimetre, has next to be determined. The gas flows with the same velocity or under the same given pressure, always with the same illuminating power, whatever be the bat's-wing in which it burns. For very different intensities the dimensions of the flame vary very little, its height being sensibly constant and terminated by a right line. *Other burners than bat's-wings.* Bougie burner, a nipple with a hole in the centre. For the same height of flame, the illuminating power always coincides with weak pressures and a hole of $\frac{7}{10}$ths of a millimetre; it increases almost indefinitely with the height. The great expenditures of gas are more advantageous than the weak ones. Manchester burner, a nipple pierced with two holes. When the diameters of the holes are very small, two bougie burners give a light equal to that of a Manchester burner, which they can form by their union. But the superiority of the Manchester burner over the two bougie burners becomes more and more considerable according as the holes increase in diameter. The maximum lighting power corresponds always to the minimum pressure, and to a diameter of $\frac{7}{10}$ths of a millimetre. *Burners with a double current of air.* The argand burner of 30 holes, $\frac{7}{10}$ths of a millimetre, proved the most advantageous of all, and it is much to be regretted that it was not compared with the Monier burner, which is much more economical again. The lighting power increases indefinitely with the expenditure; the height of the chimney should not exceed 20 centimetres. The quantity of air burned by a burner is not proportional to the consumption of gas; all the burners do not require the same amount of air in order to give the maximum of lighting power. The introduction into common gas of 6 or 7 per cent. of air diminishes its lighting power by a half. 20 parts of air mixed with 30 parts of gas gives no light.

The standard carcel lamp consumes 42 grammes of oil per hour. According to the treaty between the town of Paris and the General Gas Company, 25 litres or 27½ litres of gas burned in a standard burner under a pressure of two or three millimetres, should furnish a flame equal in intensity to that of a carcel lamp burning during the same time 10 grammes of purified colza oil.

M. Debray then resumed the history of the preparation, properties, and uses of aluminium discovered by M. Wöhler, and brought into use by M. Henri Sainte-Claire Deville. The principal progresses made in this industry are—Utilization of bauxite, a clay very common in the south of France, composed nearly exclusively of alumina and sesquioxide of iron; to this is due the purity of the aluminium of commerce. The employment as flux of salt and cryolite (double fluorine of aluminium and sodium, very abundant in Greenland). This metal is used for optical instruments, and many other objects of jewellery, &c., and even for culinary purposes. Aluminium bronze, composed of copper 90 to 95, aluminium 5 to 10, is its principal use. M. Debray stated as extraordinary facts that this bronze, containing 95 per cent. of copper, was very little attacked by acids; also that chlorine was a deleterious gas, and sodium poisonous, but that chloride of sodium (table salt) was innocuous!

When M. Debray sat down, M. Dumas rose and indicated in a few words the aim of these weekly meetings. What characterises, he said, the Exhibition of 1867 is the enormous progress made in the application of the sciences to industry and fine arts; everywhere in these immense galleries we see the facts and theories of pure science become materialised into practical applications of great value. Is it not very natural, then, that the Society of Encouragement should become the revealer and appreciator of the successes obtained in what may be called its special department?

We are surprised that the illustrious President does not try some other thing than a reduced copy of our own programme in the conferences of the Exhibition of 1867. He attempts to perform far from the galleries, in a very confined space, in presence of a very limited auditory, what we aspire to do in the midst of the Champ de Mars, in a great amphitheatre holding 500 auditors. How does it happen that the Commission of three members, the triumvirate, Dumas, Michel Chevalier, and Perdonnet, charged by the Imperial Commission with its organisation and direction, have not signified to us, by the intermediation of M. Perdonnet, their intentions issued as follows?—1. That the entries shall be gratuitous. 2. That none of the lectures shall be paid. 3. That no exhibitor shall be admitted, either by himself or by a third party, to exhibit or make known the progress he has accomplished. Is it not tyrannically unjust to make gratuitous a lecture-hall, constructed according to a very severe specification, entailing an expense of 50,000 to 60,000 fr.? Is it not barbarous to prevent 100 or 200 francs from being taken from the money received at the doors and given to the *savant* who may have succeeded in interesting a vast auditory, and initiated them agreeably and usefully into the nature and advantages of some novel branch of industry? St. Paul characterised this barbarism in remarkable terms in his Epistle to the Corinthians, when he told us not to muzzle the ox while eating.

Lastly, it is astounding that what is permitted, honourably, legally, and praiseworthily, under the patronage of the illustrious President of the Council of the Society of Encouragement, could at once become illicit and blamable in the eyes of the Imperial Commission, of which he forms part, even of the *triumvirate*. The letter of M. Perdonnet we keep with great care. The triumvirate is totally ignorant of the fact that the lecture-hall was mounted (it is very nearly finished) at private expense, on the same terms as the other establishments of the Champ de Mars Park. A lecture-hall on the subjects of objects exhibited seems to us an indispensable adjunct; yet the Commission did not think it worth their while to build one; they gave the concession of ground to the celebrated photographer, M. Pierre Petit (who had already given £2800 for the site for his pagoda), on condition that he would build a lecture-hall and furnish lectures, the profits of course to be his own. Not so. Right about face with the Commission. The lecture-hall being done, and the expense paid for, M. Petit is told that all his labor in that direction is in vain. The Commission should, at least, reimburse the sum he laid out in bricks and mortar to please the Imperial Commission. Do they want to rob him also? Every establishment for public amusement is there opened on the adjoining grounds, and they place *what price they like* for entrance; but intellectual and practical information, the food of the mind, seems to find no favour with the triumvirate. F. Moigno.

Paris, May 15, 1867.

The Academy of Sciences includes in its body four sorts of members—titular, foreign associates, free academicians, and correspondents. According to the spirit which animated its creation under the ancient monarchy, the free academicians were ordinarily high-placed men, or at least men occupying an elevated position in society, well known to be the friends of science and disposed to patronise it. They had all the rights and privileges of titular academicians—that is to say, they took part in all the elections, could form part of all the administrative or other commissions, &c. They did not receive the small stipend attached to the rank of member of the Academy, but they took their share of the presentation tickets. In this primitive state of things, the title of free academician was superior to that of the titular one, and it was ridiculous to ask the first-named to abdicate in favour of the latter. At the time of the re-organisation of the Institute of France and of the five academies which compose it, the democratic spirit which predominated at the discussion of the regulations—at least as far as concerned the Academy of Sciences—ended by putting the primitive and very legitimate institution of the free academicians in a false position, and in reducing their body to an inferior number, and thus subjecting them to humiliation, though they were always chosen from the first ranks. For example, they are deprived of their votes in the elections of titular and corresponding members, and they cannot take part in any elections except for free members like themselves. In our opinion this inferiority, contradictory in itself, ought to have disappeared long ago, the more so as it keeps up in the Academy an antagonism much to be regretted, and a mutual silent but deep-seated discontent, which has already given rise to violent discussions, and, we are sorry to say, combats with arms less noble and worthy of the members. At this moment the fire is smouldering under their feet, and may burst out as a volcano. Among the ten free academicians, three—Marshal Vaillant, M. Antoine Pussy, Count Jaubert—are present or former Ministers of State. The most ardent advocate in defence of the rights of the corporation is the Count Jaubert, well backed by the most ancient of his colleagues, Baron Sequier, a distinguished amateur mechanical engineer, formerly councillor at the Royal Court of Paris. Marshal Vaillant does not take any visible part in the conflict, but he nevertheless is mixed up in it, perhaps with a sort of disgust at the proscription with which the free academicians are struck, and he hopes, openly, but timidly, to push into the list of the titulars. A most distinguished officer of the French military engineers, universally informed, an ingenious and popular writer, an eminent agriculturist and meteorologist, he had, in the judgment of François Arago, and, we may say, of everybody else, all the talents and qualities for a titular academician; no one could be more fitted to fill a chair actively. It was a good occasion for granting his request. Not without his high influence the Academy entered into possession of their new chair; if the Marshal had not objected, the Emperor would have, according to the accustomed usage, named by decree the first titulars, and in the first place Marshal Vaillant. In how many circumstances the Marshal has been the ready and powerful intermediary between the Academy and the executive for the foundation of new prizes, the increase of the value of existing ones, or the concession of scientific missions! He has been advised to resign his position as a free academician, and to be proposed as candidate for the place of titular academican in the section corresponding to his works. He will not do that; he cannot, on account of the respect for those who share the seat with him—a seat awkwardly lessened not in dignity but in rights. He declares that he puts himself solely at the disposal of the Academy, and that, although he will not resign his present chair, he will accept with gratitude the chair of titular academician.

What is wanted more? Can we understand how a body formed of such celebrated scientific men can much longer hesitate in acquitting what we do not fear to call a debt of honour and gratitude? Marshall Vaillant had two votes in the last election, and we have heard them named as unconstitutional. This is a farce, and these two votes must absolutely become unanimity for the election of the Marshal in the Section of Geography and Navigation. Thus, if the Academy of Sciences does not decide upon rendering to the free academicians their primitive rights of participation in all the elections, it ought at least to suppress the derisive term; for it is repugnant to common sense now to have only the mutilated honour of a free academician.

We have not succeeded in ascertaining at all definitely the series of the deliberations and the awards of the juries. We know, however, that in Class 72, Group VII—sugars and confectionary products—the jury propose to award four gold medals: the first to Prussia, the Zollverein exhibitors, for the *ensemble* of their products, and especially the sugars manufactured directly and ready for immediate delivery for consumption; the second to the Mauritius for the considerable increase in the production and the progress accomplished; the third to M. C. A. Say for the products of his refinery, an excellence demonstrated by the enormous quantity and the extent of his exhibition; the fourth goes to France, considered in the light of its being the mother country of the great industry of sugars. Here, again, the needle-gun carries off the palm, and we are well beaten.

The Society of Encouragement held on Friday, the 10th inst., their second extraordinary meeting. The correspondence, summed up by MM. Tresca and Peligot, presented nothing of any interest.

M. Huzard read, in the name of M. Bells, a very favourable report on the shearing machine of M. de Nabat. For clipping horses and sheep-shearing it has been most successful. We prefer by far the little shearer of M. Cazon, which is held and set in motion by the hand, whilst that of M. Nabat is a real machine, like a knife-grinding one, set in motion by the foot.

M. Dumas read a letter, by which our friend, M. Galibert, in gratitude for the success of his respiratory apparatus, placed at the disposal of the Society of Encouragement the sum of 1,000 fr., to be employed in forming a fund for the prize, for the best application of the endosmose of gases, to be awarded in 1868. This is one of the questions put forward in the programme of prizes which are most in relation with Galibert's apparatus. M. Dumas congratulated M. Galibert on his generosity, and tendered him the thanks of the Council.

M. Dumas read a second letter from M. Taborin, one of the oldest manufacturers of files, who, spontaneously on his part, and in thankfulness for his success, took upon himself to hand over 3000 fr. towards the prize to be awarded in 1868 for a file-cutting machine. This mark of gratitude to the Society was most warmly applauded, and the noble veteran, much affected, rising to give thanks to M. Dumas and the Council, to the great surprise of the auditory, said: "To cut files is hard work, and the Society has done well to appeal to mechanical skill for alleviating it; but the forging of them is worse again, and I beg the Society instantly to found, at my expense, a second prize of 3000 francs for a machine for forging files." Much moved, in his turn, M. Dumas exclaimed, "Honour to the intelligent and energetic man who commenced his industrial career forty years ago with only two and a half francs in his pocket, who has now founded three vast establishments, and who has arrived at a trade of several million francs with honour and profit."

M. Balard presented, in the name of M. Carré, his new ice-producing machine, of which we have often spoken and given a description in our report of the Academy. The principle of this machine consists of sulphuric acid marking 59° to 66°, circulating in a thin stream, through which passes vapour of water drawn along by a vacuum created pneumatically. The evaporation of this produces the cold. The recipient of the acid is formed of an alloy of lead and antimony in the proportion of 5 to 6 per cent.; it supports, without alteration of form, a pressure of five or six atmospheres, while the pressure in practice cannot exceed one atmosphere. The copper pump is preserved from the contact of the sulphurous acid, always disengaged by the acid recently introduced, by an arrangement which necessarily and constantly oils the inside surface. The valves are opened mechanically, and cannot get deranged. The apparatus keeps the vacuum for several months; the acid is extracted when it has become diluted to about 52°; the congelation commences generally three or four minutes after the commencement of making the vacuum; if cold water at 3° or 4° C. is required, two minutes suffice, and a little shaking up for some instants restores the air which it has lost. Other substances can be substituted for sulphuric acid (which is, however, the cheapest agent to employ), such as caustic potash or soda, or chloride of calcium, which cause a congelation sufficiently prompt and intense. In the application, M. Carré mentioned the adaptation of the apparatus on board ships and in cellars where the temperature could be indefinitely kept at 5° or 6° C. in all latitudes, also for the refrigeration of apartments.

M. Dumas is of the same opinion as M. Balard, with regard to the success of the new ice-making machine, especially if sulphuric acid can be replaced by other substances more inoffensive; he indicated that oven-dried bran, a powerful absorbent, might be tried, and requested M. Thénard to give his opinion, and to state what advantage agriculture would derive from this mode of producing cold. M. Thénard first called attention to the curious and important fact discovered by milk-women, and of which he cannot find an explanation. If milk, a few minutes after being drawn from the cow, be cooled with very cold rain-water, it keeps fresh for many days, and can be sent to a long distance. Carré's apparatus can advantageously replace the cold water, especially in agricultural distilleries, which employ a good deal of sulphuric acid and keep at the same time a great number of cows.

M. Peligot afterwards gave an account of the process of M. Paris for the fabrication of enamels, of which numerous beautiful specimens were laid before the Society. We will resume this subject further on while speaking of the Exhibition.

M. Peligot also called attention to a quite new fact that he had discovered. It arose from the devitrification of a piece of St. Gobain glass, prepared a long time ago by M. Pelouze; the glass had lost its transparency, but not its density. Placed in a drawer, the piece of glass, supported by one extremity, was found, after some days, by M. Peligot, to be curved under its own weight, it having become a malleable glass; the surface was also covered with efflorescence. Pliny speaks, in his history, of a glass that could be bent and unbent; and the story goes that Richelieu ordered an inventor to be put to death for proposing to divulge a process for making malleable glass.

M. Bouillet, in his name and that of the celebrated M. Christofle, mentions two great improvements made in their electro-metallic manufacture. 1. Round bossed galvanoplastic objects could be obtained more economically by the substitution of electrodes of lead for the insoluble platinum wire electrodes used at first by M. Lenoir, the inventor of the process. 2. A series of pieces in bronze, decorated with inlaid work in silver, platinum, and yellow or green gold, had been obtained by electricity, and which perfectly imitated the charming objects coming from China or Japan.

F. Moigno.

PARIS EXHIBITION OF 1867.

(From our Special Correspondent.)

Group VI.—Class 44: Chemical and Pharmaceutical Products—Industry of Colouring Matters extracted from Coal.

This great industrial art, illustrated for the first time at the International Exhibition of 1862 with great *éclat*, has not ceased to develope itself, less, however, in France than in Switzerland and Germany, where the progress has been considerable. With us progress was fettered by the fact that aniline red, the basis of all the other colours, was the exclusive property of the Fuchsine Company, and was, in consequence, a general monopoly. The great problem under these conditions was to arrive by direct process at violet, blue, and green colours without passing through the red; it has at length been resolved for violet by MM. Poirrier and Chappat, jun., of Saint Denis, with the able assistance of their chemist, M. Bardy. The methylaniline and dimethylaniline violets, exhibited under the name of "Paris Violet," are the gems of the chemical section, and have most attracted the attention of practical men, also of the jury, who have awarded them a gold medal.

Before describing this discovery, we may remark that the Fuchsine Company have kept up their reputation, and that the display in their glass case is really magnificent; their sphere of radiating crystals, so sharp and voluminous, of chlorhydrate of rosaniline, along with all the salts of rosaniline, have a wonderful effect. They find a powerful rival, however, in M. Müller, of Basle, who also exhibits a large collection, the principal article being a cup containing a pound of rosaniline so pure that it is almost colourless. We have not found chrysotoluidine or malvaudine except on stuffs of silk and cotton, of which specimens are exhibited by MM. Hucotte and Berryer, also the blues of MM. Girard and Laire, the process of which shall be described presently.

The Paris violet, by MM. Poirrier and Chappat, No. 23 Rue d'Hauteville, stall No. 2, is produced from the methylaniline and dimethylaniline which Dr. Hofmann discovered and made known by a process very costly and not put into practice. The celebrated chemist employs as a reducing agent the very volatile iodide of methyl, and this would have entailed a considerable loss if 800*l.* per day were constantly risked—that being the quantity necessary to produce 150 kilogrammes of methylaniline that MM. Poirrier and Chappat send to the market every day. In order to arrive at a remunerative product, they have substituted a cheaper substance, nitrate of methyl, for the iodide; it is by this means that they were able to produce their first *ton* of Paris violet at their chemical works. Still this mode of fabrication was fraught with danger, and was discontinued. Happily, M. Berthelot pointed out a process less dangerous and cheaper, the treatment of ammonia by alcoholic radicals.

To obtain methylaniline or dimethylaniline, MM. Poirrier and Chappat place in contact in a closed vessel, at a high temperature, and under pressure, aniline and hydrochlorate of aniline, or methylaniline and hydrochlorate of aniline; and this process, which has required the construction of special apparatus, the application of means for regulating the reaction in order to obtain at will either methylaniline or dimethylaniline, &c., works at present with perfect regularity; they have extended, with success, the process towards the preparation of other alkaloids based on an alchoholic radical. There remained yet the means of converting methylaniline and dimethylaniline into violet, soluble in water, as there is no complete success without this condition. New agents had to be searched for, that do not require a too high temperature, in order to produce the reduction, or the return would not be so great, nor the colour so bright or of so decided a shade. They finished by transforming the perfectly pure methylaniline from toluidine into violets of all shades not inferior to those derived from the rosaniline of Hofmann.

MM. Tessié de Mothay and Maréchal, of Metz, exhibit a new process for bleaching fibres, threads, and woven stuffs of cotton, hemp, linen, wool, and silk.

The fibres, threads, and tissues contain two sorts of colouring matters—one soluble, after oxidation in alkaline lixivia; the other substances inherent to the cellulose, which should be bleached by the oxygen of the air and light, or by chemical compounds able to disengage oxygen in its nascent state.

The methods hitherto employed for bleaching or decolorising tissues depend upon the alternate application of two sorts of agents—1. Oxidising substances; 2. Solvents.

But these methods, perfect as they are in their way, have the following faults:—The employment of an oxidising agent which acts with extreme slowness when it is taken from the atmosphere, or with a destructive combustible power when it is several times placed in a medium containing chlorine or the chlorated compounds, such as the hypochlorites, for example: the use of alkaline solvents which act with extreme slowness in dissolving the quantity of colouring matter altered by the oxidising agents. For these latter the most suitable substitutes are—1. Permanganic acid, produced by the decompositition of the permanganates by means of hydrofluosilicic acid. 2. The alkaline permanganates, with the addition of chlorides, sulphates, and alkaline fluosilicates capable of forming salts, having for base permanganic acid, at the moment when this acid is decomposed by the fibres; passing, themselves, into a basic state, as is shown presently.

In order to employ practically the oxidising agents and solvents above mentioned, the operation is thus:—

For bleaching stuffs or threads of cotton, linen, or hemp, all the grease or fatty matter is extracted by an alkaline bath. They are then steeped in a solution of permanganic acid or permanganate of soda, with the addition of sulphate of magnesia. Afterwards (fifteen minutes' interval generally) the substances to be bleached are removed and transported either into alkaline solutions or into baths containing sulphurous acid, nitrosulphuric acid, or peroxide of hydrogen.

In the first case the substances are heated to boiling-point in alkaline solutions for several hours until the oxide of magnesia which covers them is partially or wholly dissolved.

In the second case the substances to be bleached are steeped in baths containing either sulphurous acid or nitrosulphuric acid or oxygenated water, until the layer of oxide of manganese, with which they are coated, is entirely dissolved; after this they are washed and resteeped, first in a solution of permanganic acid or the permanganate, afterwards in alkaline solutions or in the solvents above mentioned, and so on till the bleaching is completed.

A bleaching-bath, containing, according to the nature of the fibres or tissues to be bleached, from 2 to 6 kilos. of permanganate of soda, is sufficient to bleach effectually a hundred kilos. of cotton, hemp, or flax, raw or woven.

This method of bleaching is the same for wool and silk, except that the alkaline liquid is a weak solution of soap, and sulphurous acid is alone employed.

The industrial results obtained in the factory of M. Verlay, at Comines (Nord), by the above-mentioned process show that hemp and linen threads are completely bleached without alteration in one day; that their tissues are bleached in three days; that the cost for complete bleaching is on an average 3½d. the kilo. for threads, and 5s. per 100 metres for the woven stuffs.

By the present methods of bleaching, even the most rapid and economical, for all textile substances or tissues, threads require, according to the daylight and weather, at least fifteen days and at most thirty; tissues from thirty to sixty days. Also the cost of bleaching, on the other hand, amounts in similar cases to about 4½d. per kilogramme for threads and 7s. 6d. per 100 metres for tissues.

In order to obtain the practical result which we have just mentioned, new economical processes were necessary to be found:—1. The production of manganate of soda; 2. To transform this manganate into permanganate.

Lastly, we mention that manganate of soda is now prepared and sold at the rate of one franc per kilogramme to bleachers.

Its transformation into permanganate is easily and cheaply made, either by means of sulphate of magnesia, chloride of magnesium, or chloride of calcium.

The following formulæ show the transformation:—

$$3(\mathrm{KO,MnO_3}) + 2(\mathrm{MgO,SO_3}) =$$
$$= \mathrm{KOMn_2O_7} + 2(\mathrm{KO,SO_3}) + 2(\mathrm{MgO,HO}).$$

Long ago the researches of Messrs. Deville, Lechatelier, and Kessler proved that fluosilicic acid, if it could be produced cheaply, would most advantageously replace sulphuric acid in the great industries of potash and soda. On the one hand, some facts seem to indicate that, under certain conditions, silica, melted with fluoride of lime at the highest temperature of our furnaces, produces fluoride of silicium and silicate of lime; on the other hand, celebrated German and English engineers affirmed that the furnaces for smelting copper with fluoride of calcium disengaged at the furnace mouth fluoride of silicium, which seems to indicate, conformably with the analyses of M. Berthier, that carbon played a great part in the production of fluoride of silicium by the dry way. Starting from these principles, M. Tessié de Mothay caused to be melted in a closed crucible a mixture of two equivalents of silica, three equivalents of fluoride of calcium, and four equivalents of carbon; and he proved that at the temperature of melting iron a great quantity of fluoride of silicium was evolved. The slag resulting from the calcination, when analysed, showed that the fluoride of calcium had lost 52 per cent. of its fluorine; and direct observation proved that the fluoride of silicium produced was always accompanied by carbonic oxide gas. Convinced by these preliminary experiments of the possibility of the industrial production of fluoride of silicium, M. Tessié de Mothay made, along with M. Ede Kescher, of Sarrebruch, in a melting-pot of the works at Ars-sur-Moselle, a first essay at reduction. This was crowned with complete success, and they proceeded immediately to construct furnaces for the production on a great scale—first, of fluoride of silicium and fluosilicic acid; second, of caustic potash and carbonate of potash, extracted by the action of fluosilicic acid from the chloride of potassium of the Stasfurth mine. The quantity of fluosilicic acid obtained in the blast furnace of Gröbleterstroff, near Sarroquemines, is already sufficient to enable them in some months to deliver a ton of potash per diem at a cheap rate to the trade.

This process of fabrication is very efficacious and simple, since, by aid of fluosilicic acid, they collect 68 per cent. of the fluorine contained in the fluoride of lime. It consists in —1. Kneading, as in a brick-making machine, carbon with a mixture of silex, clay, and fluoride of lime, in quantities proportionately equivalent, and the formation, after fusion in the blast furnace, of a bibasic silicate of alumina and lime. 2. Mixing the cakes with the proportion of coke necessary for the fusion. 3. Filling all the furnaces through a double chamber to hinder the passage of the gases by the furnace mouth. 4. To melt the cakes by an intense heat produced by powerful blowers, and to collect the gases in condensers whose surfaces are continually wetted, so that the immediate contact of the water decomposes the fluoride of silicium into hydro-fluosilicic acid.

In Class 51 is exhibited a complete plan of the works and blast furnaces of Grösbleterstroff; and in Class 44 a series of bottles containing fluosilicic acid of 180°, fluosilicate of potash, of soda, and of barytes, caustic potash, and soda, which the fluosilicic acid has separated from their combinations with sulphuric acid.

Chemists, only a few years ago, would have refused to believe that one could procure so easily and certainly, and on so large a scale, a product hitherto confined to the laboratory, but now destined to modify in the most successful manner one of the most important of modern industries.

Paintings on glass required to be transparent must have a thickness of enamel four or five times greater than that of paintings on ceramic paste, which are to be viewed by reflection. Hence it follows—1. That the designs made to be transferred to glass cannot be printed from ordinarily engraved copper plates, since, after baking, it would not have the necessary thickness and opacity; 2. That the organic matters serving as a vehicle for the vitreous flux to make the impressions must be increased in a quantity proportional to the amount of the enamels they are to contain.

In order to resolve the problem of impression by means of applied drawings of enamelled pictures vitrefiable on glass, it was necessary to have recourse to the employment of deeply engraved plates, similar to those for paper-hangings and stuffs, and organic inks containing the enamels in a state of combination.

But all the vehicles hitherto employed for printing enamels, porcelain, and earthenware, when mixed with colouring fluxes in sufficient proportion to permit of the impression, cause on the glass, during the baking, the deformation of the designs, and, in numerous places, non-adherence to the surfaces they cover. The same effects of deformation and non-adherence are equally produced with inks composed solely of resinous siccatives, essences, bitumens, resins, and other analogous vehicles.

MM. Tessié de Mothay and Maréchal have happily proved that the organic inks favour, on the contrary, the union of the vitreous flux with the sheets of glass; in fact, the solvent of the colouring matters used in painting on glass is in general silicate of potash and lead, or a silico-borate of the same bases. This combination, rendered plastic by the addition of more or less resin dissolved in the turpentine, is a perfect ink, which, printed in a thick layer and transferred to glass, is burnt and vitrefied without deformation or air-bubbles. Thanks to it, it is possible to employ, for the reproduction of ornamental or plain prints, the rollers with deeply cut lines, which serve at Mulhouse for the printing of stuffs. Worked by steam power, these rollers produce in an hour more than 250 skilful designers could do in a day. Many thousands of plain patterns and mosaics of stained glass produced by this process already adorn our churches, and the low price at which they are produced and sold tends every day to multiply the number.

The same investigators have also discovered a new method applicable to the production of photographic images of all sorts on glass, enamel, lava, porcelain, earthenware, &c. It comprehends a series of ten operations, which we will summarily describe in their order:—1. Four parts of caoutchouc are dissolved in a hundred parts of benzol. To this solution is added one part of normal collodion. This compound is poured upon any of the substances on which a vitrefiable portrait is required to be produced. It is then dried, either in the open air or in a stove, until a very coherent coating is formed. 2. On this first coating, thus dried, iodised collodion is poured. This second coating unites intimately with the first, and thus acquires a resistance equal at least to a layer of caoutchouc of the same thickness, a resistance which no ordinary collodion possesses. 3. After having immersed the double coating, thus prepared, in a bath of nitrate of silver, an image is produced on it, either by a camera or by superposition. 4. The latent image thus produced is developed by any of the agents generally used. 5. It is then fixed by successive action of two baths, one containing a solution of an iodocyanide, and the other an alkaline cyanide. 6. The image thus fixed is steeped for some instants in a solution of sulphate of protoxide of iron, pyrogallic acid, or any other substance that will reduce the salts of silver. 7. The image is intensified by the action of pyrogallic acid, gallic acid, formic acid, or sulphate of protoxide of iron, mixed with an acid solution of nitrate of silver. This strengthening requires, on an average, four to six applications, when the image is to be seen by reflection, and twelve to fifteen for those which are to be seen by transparency. During this operation of reinforcement the images are washed three or four times in alternate baths containing iodo-cyanides and alkaline cyanides; then, immediately afterwards, in sulphate of protoxide of iron, pyrogallic acid, or any other reducers of the salts of silver. The consecutive employment of baths of iodo-cyanides, and of alkaline cyanides, has the effect of completely dissolving the non-adherent silver precipitated over the whole plate in each reinforcing bath, and this without destroying the original image, which alone is intensified. The washings in the reducing bath rendering neutral the metallic surface, increase powerfully the ulterior action of the reinforcing baths. 8. The photographic image being developed, fixed, and reinforced, is immersed for several hours, either in a bath of chloride or nitrate of platinum, or in alternate baths of chloride of gold and nitrate of platinum, or, again, in baths of chloride of gold. During the steeping, the silver of the image is either partly replaced by platinum, by a mixture of platinum and gold, or by gold alone. The purpose of the different substitutive baths is in order that the colour and nature of the layer of silver of the image may be changed after vitrification. In fact, if it is desired to obtain by the muffle, and by the reactions of silicic or boracic fluxes, images of a greenish black, they are previously immersed in a bath of chloride or nitrate of platinum; if, on the contrary, a black colour be required, they are steeped successively in baths of chloride of gold and nitrate of platinum. When, lastly, gilt images are to be produced, they are plunged into baths containing exclusively salts of gold. 9. The image, on coming from the platinum or gold baths, is washed in a solution of alkaline cyanide or concentrated solution of ammonia; it is then covered with a thick varnish of caoutchouc, or with gutta-percha, and submitted to the action of fire, in a muffle, when the organic matters are consumed and the metal left. 10. Lastly, the image, thus freed from the collodion and other organic matters, is covered with a silicic or boracic glaze, and submitted to an orange-red heat, which vitrefies it. This method is calculated to effect the perfect preservation of photographic images.

Group V.—Class 44: *Chemical and Pharmaceutical Products.*

The glass case of Messrs. John Casthelaz & Co., of Paris, contains a very rich collection of chemical products obtained by newly-improved processes, which reflect great credit on the firm. We shall enumerate them rapidly. They decompose daily two tons of nitrate of soda by sulphuric acid to obtain either nitric acid, monohydrated nitric acid of 48° to 50°, or pentahydrated from 36° to 40°. Nearly all these acids are employed on the very spot where they are produced. The monohydrated acid serves to produce the nitrated products of benzol and toluol; the acid at 35° is used to transform arsenious into arsenic acid, phenic acid into trinitro-phenic or picric acid, the bichloride of naphthaline into phthalic acid, &c. They transform daily a ton of benzol into nitrobenzol and aniline, and fabricate, on a very large scale, picric acid, crystallised and fused. The weight of some of the specimens attains one or even two kilogrammes. They have invented also, for purchasers, an apparatus termed *picrometer*, which permits them to verify, without trouble, the purity of the acid delivered. Starting with the fact, noticed by MM. Paris and Ernest Depouilly, that the basic phthalate of sulphate of lime at 300° is changed into benzoate of lime, they make naphthaline serve for the production of benzoic acid. Phthalic acid results from the decomposition of the naphthalic bichloride. Phthalate of ammonia distilled gives phthalimide of lucuine; distilled with powdered quicklime, the phthalimide produces benzonitrile, and benzonitrile distilled with caustic soda gives benzoate of soda, from which chlorhydric acid precipitates benzoic acid. Attacked by nitric acid, the bichloride of naphthaline leads to an oil, and forms binitrated chloride or the binitro-chloroform of M. Berthelot, of which the odour is so penetrating, and the action on the eyes and respiratory organs so terribly deleterious. The small vial of this oil which figures in the above-named glass case contains enough to burn the eyes of thousands of visitors. Near it we see the picrates of baryta, iron, lead, and mercury; chloroxynaphthalates of baryta, iron, zinc, nickel, and copper; with the bichromate of potash aniline violets, the soluble garnet colour, isopurpicrate of potash, a fulminating substance which must be kept wetted with water, the product of the reaction of cyanide of potassium on picric acid, which dyes wool in the richest colours with a saving of 25 per cent.

The pure and crystallised products from the laboratory of Messrs. Coblentz Brothers—phenotoluol, azobenzol, nitranilines, binitrobenzol, binitrotoluol, toluyldiamine, and paraniline—do them the highest honour. They are eminently skilful in transforming into colouring matters the direct products of coal tar. They exhibit an enormous block of nitrotoluol admirably well crystallised, of a pale yellow colour, and nearly free from nitrobenzol. They have discovered a very cheap process for transforming nitrobenzol into aniline, and nitrotoluol into toluidine. They take cast-iron turnings, roughly ground to powder, cover them with a layer of metallic copper, by plunging them in a solution of sulphate of copper. These galvanised turnings are then placed along with nearly an equal quantity of non-galvanised turnings, and surrounded by a sufficient quantity of water. Nitrobenzol or nitrotoluol is then added, and a galvanic action takes place in the liquid. The water is decomposed, and the hydrogen makes the nitrate body pass into the state of aniline or toluidine, which is then rectified and rendered pure. By treating the residues with sulphuric acid, the copper is dissolved, and can serve for another operation.

A magnificent and curious experiment will be made in a few days in the middle of the Exhibition, in the portion allotted to M. Flaud, one of the constructors admitted to furnish motive power for the gallery of machines. The experiment is imagined and organized by M. Henry Giffard, the well-known inventor of the injector; but it is to be produced under the name of Mr. Young, formerly the companion of M. Giffard in his steam essay of aërial navigation. It will consist of an anchored balloon, which will rise with twenty or twenty-five persons to a height of about a hundred metres. It will remain some time in the air, and then descend, to ascend again with a fresh party, and so on all day long. The problem of captive aërostation is, it is well known, one of the most difficult of physical mechanics. Arago said it was almost impossible. M. Giffard solves the question on the most rational side, and he will succeed. His balloon is a perfect sphere 21 metres in diameter, formed of two very fine and close tissues stuck together with several coats of black india-rubber American varnish, coated also with linseed drying oil to prevent all osmose or diffusion. This cloth is sufficiently impermeable to hydrogen to keep it in for a time sufficient for prolonged experiments, even for a whole day. The dimensions of the balloon are calculated so as to assure an ascensional power in such a manner that the horizontal component of the wind will only produce a slight deviation in the vertical direction of the cable holding the balloon. Upon this depends all the successful solution of M. Giffard, and it is certain that the angle of deviation will never exceed 40°. The cable unrolls and rolls up on a drum-wheel very solidly fixed. The hydrogen for the balloon will be temporarily procured by the ordinary process—iron and sulphuric acid; but in a few weeks it will be obtained by the decomposition of water by red-hot charcoal, at a price less than 15 c. the cubic metre. Oxygen of M. Tessié de Mothay at 50 c., and Giffard's hydrogen at 15 c. the cubic metre! What a fortunate combination this will make for the use of many industries!

We must not forget to mention, in a few words, the celebrated proprietor and director of the Pont-Labbé works, a real model establishment, M. L. Paisant. Founded in 1840, for the manufacture of starches and their conversion into alcohol, syrup, and dextrine, it was very soon affected by the potato disease. The establishment is not now confined to its original manufactures; it produces all sorts of chemical substances from the seaweed so abounding on the Brittany coast. It consumes at present 1800 tons of raw material, and delivers to commerce in places far inland 160,000 litres of kelp or lixiviating ash produced from seaweed. It is extensively used as an excellent manure over a great portion of Finisterre.

M. Paisant is a great manufacturer, much esteemed by all the inhabitants, and the jury should take him into consideration. He has developed his establishment on a large scale; he has paid great attention to the welfare of the population among which he dwells, and has ameliorated the condition of the working classes physically and morally, increasing their wages, constructing their dwellings, which leave nothing to be desired as to salubrity, and has founded a mutual helping savings bank, in case of the stopping of work or sickness, and for the relief of women and children.

In the first rank of the manufacturers of chemical products we must place M. Deiss, of Paris, Marseilles, and Lyons, the originator of the fabrication of sulphuret of carbon, and its use in the extraction of all fatty matters. In 1847, sulphuret of carbon, produced in a laboratory, cost 60 francs the kilogramme. In 1867, M. Deiss sells it at the rate of 35 centimes (3½d.) a kilogramme. In 1848 the treatment of caoutchouc by sulphuret of carbon was hardly known. Sulphuret of carbon now takes up, from the residues of many industries, several millions of killogrammes of fatty matters hitherto thrown away; and this application is one of the most brilliant discoveries of modern times. No matter how powerful the presses, a considerable quantity of oil is left in the cakes of oleaginous seeds. The sulphuret of carbon supplies, wonderfully, a remedy for this imperfection, by taking out all the remaining oil. Paris, Brussels, Lyons, and Marseilles possess large establishments for the extraction of the hitherto lost fatty matters. Since 1862, these industrial works have been well appreciated by the juries. Dr. Hofmann placed them in the first rank. Since a few years back, they have attained an enormous size. At the Carthusian Friary of Marseilles, Boulevard Achard, a gigantic extractor treats, in 36 hours, 43 cubic metres of pulp or lees of olive, using 45 tons of sulphuret of carbon, which penetrates into the whole mass, takes up the oil, and deposits it in the distilling apparatus. The sulphuret of carbon is then passed through a worm, regenerated, and condensed, without any sensible loss, ready to take up another quantity of oil. Thirty to thirty-five tons of oily substances are thus extracted at each operation.

At Lyons they used to throw into the Rhone, annually, the oil that might have been used for making 5000 tons of soap. Now, on the contrary, the application is complete: oil in its neutral state is entirely utilised, and the process of M. Deiss is welcomed in the industrial arts.

M. Collas, our esteemed friend and neighbour, No. 8, Rue Dauphine, was the first to introduce in France, about 1840, the manufacture, mechanically, of medical lozenges. They are stamped out as in the Mint, like coins of different shapes, and bear inscriptions indicative of quality and the method of using them. He discovered, in 1848, benzine in coal oil, and at once introduced it into commerce for cleansing stuffs; also nitro-benzine, which is a cheap substitute for the essence of bitter almonds. The creation of this substance really led to the beautiful aniline colours. The Society of Mulhouse said of him, in their meeting of March 22—"M. Collas has introduced into commerce benzine and nitro-benzine; the first seems for taking out stains from stuffs, the second is used in perfumery; . . . . he has not found out any colouring matter, but we are now in possession of a product hitherto impossible to be procured. Dr. Hofmann repeated the experiments of M. Runge on kyanol (aniline). . . . . Perkin reproduced a reaction first indicated by Berzelius, and a great industry has been created. . . . ." Thus the Mulhouse Society give all the credit to M. Collas for the means of making aniline colours.

Phosphates of lime, considered in the light of physiology and pharmacy, have been one of the favourite studies of M. Collas. He discovered and demonstrated by experiment that gelatinous phosphate of lime possessed the singular property of facilitating the putrefaction of animal matters. It acts under these circumstances not as forming, but as aiding powerfully, the development of those infusoria which determine the decomposition of animal matters. Thus it is a powerful digestive agent, and as experiments prove that diabetes and glucolucie depend upon the nature of the blood, and that the excellent preparations of phosphate of soda of M. Collas, his phosphoric lemonade, and milk of hydrated phosphate of lime are most successful.

During these last years, stimulated by the experiments of M. Lucie, M. Collas prepares iron reduced by electricity, quite pure, by aid of one Bunsen pile acting on chloride of iron in solution. The iron is deposited as a coating, while the several impurities fall to the bottom, leaving the iron quite pure. Obtained thus in grains or in very friable plates, it is easily reduced to an impalpable powder, like the copper of M. Oudry. It is, however, very oxidisable, and is obliged to be enclosed, for medical use, in gelatine capsules. Each capsule full of iron weighs 10 grammes and 50 centigrammes (or about 162 grains); the maximum dose per diem is five capsules, and observations continually prove the striking efficacy of the administration of metallic iron.

We recollect having seen in the glass case of M. Rousseau, another first-class maker of chemical products, a great phial of iron reduced by electricity, which was very astonishing to behold. It was not black like that of M. Collas, but it had the grey metallic colour of iron. He founded in 1843 an establishment for the fabrication and sale of chemical products and apparatus necessary for physical manipulation and the instruction of students. For twenty years he resided in the Rue de l'Ecole de Médecine, and three years ago he removed to the Rue des Ecoles. His customers have increased daily under our very eyes, and this augmentation proves that he has well fulfilled his engagement towards the public. In class 44, M. Rousseau shows what he ordinarily manufactures and delivers for sale:—1. Specimens or samples of products and reagents. 2. A series of oxides and salts for colouring silicates, the cost of which he has lowered without diminishing the quality, so as to compete favourably with foreign markets. 3. Masses of sodium that he was the first to manufacture commercially and cheaply, so as to render aluminium available at a moderate cost, and on which account M. Sainte-Claire Deville made him one of his partners. 4. Magnesium also prepared for sale commercially, along with other metals recently discovered—such as cæsium, rubidium, thallium, &c. 5. Pyrogallic acid for photographic purposes, of which he sells 1500 to 1800 kilogrammes per annum. 6. Benzoic acid employed in the preparation of aniline colours; of this he undoubtedly extracts several thousands of tons every year from the urine of cows, carefully collected from the numerous dairies about Paris. Before he began his work all the benzoic acid consumed in France came from Germany. The great aim of M. Rousseau is to give to the commercial world, at a reasonable price, materials which were unattainable with profit by the manufacturing world. He exhibits in Class 51 a stove, which advantageously replaces that of Gay-Lussac, and which only costs 25 fr. instead of 45 fr.; a new densimeter easily employed; plates and cylinders of artificial carbon for the cheap use of Bunsen's batteries. He also shows in Class 90, marked at the price of 200 fr. (8*l.*), a collection of products for the elementary instruction of chemical students, with an explanatory volume. We mention other innovations made in France by the same distinguished hand. In 1839 he patented the first process for obtaining sulphuric acid from pyrites; there was at that time a great establishment at Javel, but sulphur, then at a high price, suddenly fell in the market, and pyrites could no longer compete with it. To M. Rousseau is due, also, the first idea of the agglomerated coal, or "Charbons de Paris," extensively used in France for culinary purposes. M. Popelin-du-Cazze, who bought the patent, has been very successful on the large scale. In 1849 he organised the mode of extraction of sugar by the double action of lime and carbonic acid, which has now been followed for the last twenty-four years under the name of Rousseau's process by all Europe. He also discovered a decolorising charcoal black at a cheap price—6 fr. the 100 kilogs. (220·5 lbs.) for refining, in place of the cumbersome and dirty substances formerly used.

M. Huillard, the elder, of Paris, exhibits as the special objects manufactured by him orseille, orcine, orceine, eriphthine, eryphic acid, &c. The other mineral substances, the protoxide and sesquioxide of cobalt, and five carbonates, the arseniate, phosphate, silicate, borate, sulphate, nitrate, and chloride of cobalt; and the carbonate, nitrate, and chloride of cadmium are admirably crystallised. These are very creditable, and reflect much honour upon M. Jourdin, the young and energetic superintendent of the works, who is actively engaged in solving the following problems:—1. The delivery to commerce of orseille, liquid orseille, orcelline, extract of orseille, or imperial red. 2. The production on a large scale of cobalt blues to rival those of Germany. M. Huillard has succeeded in rendering lower the price of cobalt blues for stuffs and wall-paper, while at the same time they are more durably fixed. The Bank of France has adopted them for the 100 fr. bank-notes, inimitable by any photographic process; also for the 20 centimes (blue twopenny) postage stamps, though they are of a greenish hue by candle-light, but readily distinguishable from the others.

M. Eusèbe, of Paris, exhibits some very fine aniline greens and reds obtained by carbò-humic acid. He has yielded the full right of manufacture to M. J. J. Müller, of Basle. He has two magnificent collections of samples which can rival with any of the other manufacturers in the Palace.

M. Jean Rod has already received our compliments for the splendid specimen of crystallised and almost white rosaniline. He challenges the first markets of Paris or London, and the greatest houses to produce better. He produces daily 175 kilos. of muriate of rosaniline, aniline blues, violet, and greens. He also produces, with the same substances, and delivers to the commercial world—hydrochlorate of aniline, a red dye; hydrate of monophenylic rosaniline, giving a reddish-violet tint; hydrochlorate of diphenylic rosaniline, giving a blue violet; hydrochlorate of triphenylic rosaniline, giving a blue dye.

In another collection of violets exhibited by him under the name of Parma or Alexandria violet, in the preparation of which ethyl replaces phenyl—Hofmann's process—exhibited also by M. Menier, M. Rod exhibits a cup of 500 grammes (1⅛ lb. avoirdupois) of cyanide or quinoline blue. To enumerate the splendid display of M. Rod would be too much for our space; suffice it to say that we warmly recommend him to the attention of the jury.

London University.—Professor Williamson, Ph.D., F.R.S., and H. Debus, Esq., Ph.D., F.R.S., have been re-elected Examiners in Chemistry for the University of London. The salary of each office is 175*l.* per annum.

PROCEEDINGS OF SOCIETIES.

CHEMICAL SOCIETY.

Thursday, April 18.

Dr. J. H. Gladstone, *F.R.S., Vice-President, in the Chair.*

In continuation of our report of last week, we now give an account of the papers read by Mr. Chapman, and of the short communication from Dr. F. C. Calvert.

The first, entitled "*Oxidation of the Acids of the Lactic Series*," by Messrs. E. T. Chapman and Miles H. Smith, asserts that the acids of this series may be divided into two classes, according to the nature of the products of oxidation which they respectively furnish; the first containing hydrogen and an organic radical, giving rise to the formation of aldehydes, whilst the secondary acids, containing two organic radicals, produced ketones. Dimethoxalic acid yielded, on oxidation, acetone and carbonic acid, and ethomethoxalic acid (prepared from the corresponding ether of Frankland and Duppa) gave carbonic acid and methylated acetone, boiling at 82–83° C., and having a fragrant odour, very similar to propione. Of this compound an analysis was made, which furnished results in close accordance with the numbers demanded by theory. Like common acetone, it combines with alkaline bisulphites, but with more energy, evolving a considerable amount of heat, and forming a compound which is very soluble in water. On further oxidation of this substance nothing but acetic acid was produced. Similar results were obtained in the oxidation of diethoxalic acid. The authors recommend a resort to this method for the easy preparation of the ketones, and, finally, propose by this means to investigate problems of isomerism occurring amongst the fatty acids, illustrations of which are sketched in the paper.

The Chairman insisted upon the value of an extended optical method of research carried out in conjunction with the principles indicated by the authors in this communication.

A preliminary note "*On Limited Oxidation with Alkaline Permanganate*," by the same authors, was then read. The authors point out the differences in the products of oxidation of common alcohol by the use of acid and alkaline solutions of permanganic acid; thus, whilst, in the presence of sulphuric or other mineral acid, a mixture of aldehyde and acetic acid is produced, on the other hand, in an alkaline solution nothing but oxalic acid is formed. Lactic acid under like circumstances gave similar results. So that much depends upon the maintenance of unalterable conditions during the progress of oxidation with permanganate of potash, and the authors submit that Truchot's and Berthelot's anomalous results obtained in the examination of amylene are attributable to a want of appreciation on their part of this fact. The authors described a method of distinguishing between tartaric and citric acids, founded upon the circumstance that the latter acid does not carry the reduction further than the green manganate in a strongly alkaline solution, whilst tartaric acid furnishes the brown hydrated binoxide.

The Secretary then read a paper by Dr. F. Crace Calvert, "*On the Presence of Soluble Phosphates in Cotton Fibre, Seeds, &c.*" The author points out the fact that seeds contain relatively more mineral phosphates than other parts of the plants upon which they are borne, and alludes to the common practice of burning off the organic matters before proceeding to search for the phosphates therein contained. From his experiments Dr. Calvert has, however, been led to conclude that the whole of the phosphoric acid or phosphates is merely held mechanically distributed throughout the organic tissue, and in such a condition that they may be wholly extracted by the action of water. Cotton yarn steeped for several hours in distilled water furnished a solution containing appreciable quantities of phosphoric acid, lime, and magnesia, and quantitative experiments were made upon seven characteristic varieties of cotton, which had been carefully prepared and carded in Manchester, for the purpose of determining the extent to which the phosphates could be removed by washing. The results showed by the uranium process amounts varying between ·035 and ·055 per cent. of phosphoric acid thus dissolved, whilst traces only of this constituent could be detected in the ash left upon burning the washed and dried cotton. Similar experiments made upon wheat, French beans and walnuts gave like results, much phosphoric acid and magnesia being discovered in the aqueous solution. The author promises to continue these researches and communicate the results.

The Chairman moved a vote of thanks in favour of the authors, and adjourned the meeting as already reported.

Thursday, May 2.

Professor W. A. Miller, *M.D., Trea. R.S., Vice-President, in the Chair.*

On the minutes of the previous meeting being read, the Chairman stated that the election announced to have taken place on the last occasion must be declared null and void, by reason of an insufficient number of voters taking part in the ballot. The name of another candidate who was then returned non-elected would also be again suspended, since the ballot in both instances was informal. The minutes were then confirmed. The following gentlemen then signed the statute book, and were admitted Fellows of the Society, viz., Messrs. Henry Weston Eve, C. R. A. Wright, William K. Waite, and J. W. Hudson, LL.D.

The candidates now proposed for election were Augustus Alfred Wood, 74, Cheapside; William Phipson Beale, barrister-at law, Stone Buildings; and Alfred Coleman, Plough Court, Lombard Street. For the second time were read the names of Robert R. Tatlock, Millhouse, Kyles of Bute, and Walter William Fiddes, gas engineer, Sothernhaye, Clifton. For the third time were read the names of John Cargill Brough, 4, Norman Terrace, Stockwell; F. W. Peterson, of her Majesty's Mint, Calcutta, 51, Myddelton Square, London; and three other candidates. Before proceeding to the ballot, the Chairman said that the measure which had been under the consideration of the Council, and which was intended to raise the standard of qualification required for a member's admission into the Society, was not to be construed as being retrospective in its operation, and it would be manifestly inconvenient if the regulations were made more stringent in the case of the candidates proposed for election on this occasion. Upon the ballot being opened, the Fellows to the unusually large number of thirty-two recorded their votes, and the balls in certain instances had to be very carefully counted. The result was the return of Mr. Brough and Mr. Peterson as Fellows of the Society, but the other three candidates were declared non-elected.

The Chairman referred to the circumstance of so many of the leading members being in attendance at the Paris Exhibition and elsewhere, and said that a difficulty had been encountered in the attempt to provide formal matter in the shape of papers to be read; he would, therefore, invite communications from any Fellow or visitor in the room, who chanced to have any subject of interest in a sufficiently forward state to bring before the notice of the meeting.

Dr. Odling claimed the indulgence of the meeting in regard to the remarks he was about to offer, with a view of starting a subject for discussion. In the first place, he would mention, for the information of the manufacturing chemists, that the French had apparently succeeded in working out a great commercial problem with respect to the recovery of sulphur from the waste residues of the alkali manufacture. Great blocks of sulphur were shown in the Paris Exhibition, which were said to have been prepared from this source, and it was proposed to employ it in making gunpowder and other sulphur products. So far as the speaker was aware, no published statement of the *modus operandi* had appeared, but it was generally believed that manganese in some form, and possibly the refuse of the chlorine retorts, was made available.[*] In the event of these antici-

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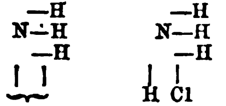
[*] It has been suggested that the sulphur may possibly have been extracted by the method of M. Emile Kopp, described last year in the *Moniteur Scientifique*.—Ed. C. N.

pations being realized, there were vast accumulations of waste material in the north of England which could thus be economised. Turning to another topic of more abstract scientific interest, Dr. Odling observed that he found a difficulty in recognising the law of combination by saturation capacities, in what had been termed the "atomicity" of nitrogen. For instance, this element was found at one time entering into combination with *three* atoms of hydrogen, and thereby forming ammonia, whilst at other times, as in sal ammoniac, the nitrogen was united with *five* associated atoms. The constitution of these bodies might be thus represented:

I.—Ammonia. II.—Sal Ammonia.



According to this view, nitrogen had always five bonds, two of which, in the first example, controlled one another's activity, and all five were brought into play and united either with hydrogen or chlorine in the ammonia salt. In the union of chlorine and nitrogen, there must be an abstraction of heat, since the decomposition of the chloride of nitrogen was attended with the disengagement of a considerable amount of heat, yet, if chlorine be passed into ammonia, there was still a development of heat, and, nevertheless, the chlorine we are told, leaves the hydrogen in order that it may unite with nitrogen, an element for which it has a very slight affinity. It might then, from this reaction, be doubted whether nitrogen was really pentatomic. Again, sodium exposed to air produces Na_2O. and this dissolved in water (with disengagement of heat) produced a hydrate, from which the water could not again be driven off by heat. But inasmuch as sodium gives out more heat than hydrogen by its union with oxygen, the conversion of the molecule of sodic oxide into hydrate would produce exactly as much cold as the conversion of the molecule of water into hydrate would produce heat. Thallium differed from sodium in these respects, and its hydrate lost water by the action of heat. These facts prove that sodium exerts upon the hydrogen an action which thallium does not. Other illustrations were quoted: thus in the slaking of lime there was great heat evolved, whereas the contrary result might have been expected, since there was, so to speak, "an unburning of the calcium." In the instances of barium and magnesium there seemed also something requiring explanation. The speaker concluded by offering an apology for having brought forward these considerations in an incomplete form, but he trusted that the attendant circumstances would justify his having taken such a step.

Professor Williamson remarked that chemists were very much in the habit of losing sight of one most important fact —viz., the changes of properties which elements undergo in combination. When they found that one element had got certain combining forces, they were apt to suppose that it must retain them when partially saturated by some other element. Thus, admitting that in potassic hydrate an atom of oxygen was combined, on the one hand, with an atom of potassium, on the other with an atom of hydrogen, it was not correct to assume that the force which bound it to the hydrogen was the same as that which bound it to each of the two atoms of hydrogen in water. The atom of oxygen in potassic hydrate was altered by its combination with potassium in such a way that it combined more powerfully with hydrogen than in water. So also in sal ammoniac, an atom of nitrogen was united with five atoms, four of hydrogen and one of chlorine. By uniting with hydrogen it had become more basylous, and could hold chlorine more firmly than it could do when united only with chlorine. Professor Williamson added a few remarks on the subject of the word "atomicity." He was of opinion that all we know on the subject was represented by the word equivalence, and he recommended the retention of the word equivalence. "Atomicity" was used by some persons to denote an immutable equivalence, and in such sense was untrue and mischievous.

The Chairman entertained an objection to the use of the word "atomicity," since it implied a theory, whilst the term "equivalence" was merely a statement of the facts.

Dr. Thudichum said that in taking note of the fact that three successive speakers had expressed their dissatisfaction with the term "atomicity," he begged to state that he had never used it, but had from the beginning of his teaching described the property of atoms which it was intended thereby to signalise as "dynamicity." He recognised a priority of publication of the conception of the necessity for this change on the part M. Würtz,* although this chemist had not made the change nor abandoned the questionable word. No one who valued logic could use expressions which signified the "indivisibleness of the indivisible," or the "undividness of the undivided," with the effect of thereby defining anything; but the expression would appear still more illogical when it was considered that it was used to define a power the very essence of which, as it appeared from one point of view, was divisibility. Indeed, the expression "tomicity," if properly understood to refer to the power, and not to the matter or body of the atom, would be correct for a far greater number of cases than the same word with the privative "a" prefixed could ever reach. Those who had hitherto been most active in developing the theory of "atomicity" had implied that it was a power possessed by the atom absolutely, and that in cases where apparently only a portion of the maximum power which an atom could exercise was employed, the rest of its power was free, and open to engage itself. Late developments of science had, however, made it very doubtful whether this was a correct appreciation of the facts observed, and whether the changes in the amount of power exhibited by atoms were not actual changes of the amount of power possessed by them in concrete cases. Indeed, if he had rightly understood some theoretical considerations of Kolbe and of Würtz, these chemists seemed to admit that what they called "atomicity" was a changeable power of atoms, and that was the conclusion towards which his own studies and reflections had been driving him (Dr. Thudichum) for some time. The positive part of his opinion would appear from a few definitions which he begged leave to lay before the meeting. The idea of atom included, of course, that of element, in the chemical sense; and most chemists defined atom as the smallest quantity of an element that could exist in any chemical compound. All atoms manifested various qualities of power, and of some qualities of power many atoms manifested varying quantities. The first obvious power of atoms was that of polarity, the differentiation of which, in two opposite qualities (conveniently termed positive and negative, or + and −), explained the formation of the elementary molecule, or molecule consisting of two atoms homogeneous in every respect except that of polarity. Atoms further manifested chemism, or power to effect interchange of place, or substitution, or combination with heterogeneous atoms, polarity determining the place which each atom should take with reference to any other. Chemism is a stronger power than more polarity; it separates the homogeneous molecule; but, in forming a new and heterogeneous molecule, it adds the entire amount of the polarity of the atoms employed to its own effective power. Thus, out of the combination of a molecule of hydrogen $(\overset{+\,-}{HH})$ and a molecule of chlorine $(\overset{+\,-}{ClCl})$, there result by the action of chemism two molecules of hydrochloric acid, equal in all respects except the polarity of their constituent atoms. The next quality which atoms manifested was dynamicity, which might be defined as the faculty to vary—that is, to decrease and increase—the amount of power of combination with other atoms. This power might be exercisable in multiple directions at the same time, but was not separable as to seat. For the intensity with which an atom was held, or exercised

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* The reader is referred to the footnotes, p. 184 of Würtz's "Philos. Chimique." Paris. 1864.

itself, in combination, dynamicity afforded as yet no measure. That intensity was no doubt the result of polarity, chemism, and dynamicity, and probably other influences, combined. But for the numeral quantity of power or the number of units of power of combination as measured by certain assumed standards, dynamicity afforded a correct expression. The unit or minimum of dynamis manifested by an atom, he termed monad; the greatest number of units constituted its monado-atomic equivalent, or full dynamis. The assumed standard of unit of dynamis hitherto accepted had been hydrogen, and the maximum number of units of power, or monads, hitherto attributed to any atom had been six, or once, in the case of iodine, seven. Now, the great error of the atomicity doctrine, most useful as it had been in evolving new facts and ideas, was this, that it assumed the number of monads of the atoms of each element to be invariable. The application of the doctrine of dynamicity to a broad field of chemical facts would, however, soon show that the dynamicity of atoms was variable, called forth or imparted by influences external to the atom, and similarly withdrawn. Heat, light, electricity, vital power, and pathogenetic power were such influences, governing not only the polarity and chemism, but also the dynamicity of atoms. Hydrogen manifested itself most commonly as monodynamic; it could be substituted by iodine and nitrogen. Now, iodine might be tridynamic, as in ICl_3; nitrogen might be tridynamic, as in ammonia. In the latter case the defenders of the invariable pentadynamicity of nitrogen would say that it was only active with three monads, and that two were free. In the case of iodine, however, when it substitutes hydrogen, the assumption that it only functioned with one monad out of its three was inconvenient, as it opened the door to the admission of the same variability on the part of hydrogen, and therefore iodine was here commonly admitted to be monodynamic. When nitrogen was made to substitute hydrogen, it lost apparently all dynamicity, though exercising only one, according to the assumption of the invariable monodynamic character of hydrogen. Out of such difficulties there were, no doubt, ingenious modes of extrication, such as the assumption of molecular powers on the part of combined groups of atoms not having the character of radicals. But the simplest cases were most simply explained by the hypothesis that the dynamicity of an atom was variable and dependent upon external influences calling forth its manifestation. Iodine had already been shown to be monodynamic and tridynamic; in imide, nitrogen was as monodynamic as when it substituted hydrogen in azobenzoic acid; in amide, nitrogen was didynamic; in ammonia the atom of nitrogen exhibited tridynamic combining powers; in some of the compounds lately discovered by Griess it showed tridynamic substitution power, replacing three atoms of hydrogen. The generalisation of this conception would no doubt destroy the simplicity of the present doctrines, but lead to a better appreciation of all the powers and influences determining combination or separation. In conclusion, the speaker apologised to the Society for the imperfection of his remarks. They had been quite *impromptu*, and dealt with a great and difficult subject, which in many respects required consequential development. But he recommended his terminology[*] to the attention of the members, feeling sure that a slight familiarity with it would prove its great convenience.

Professor **Foster** agreed with Dr. Odling that, when the two molecules Na_2O and H_2O were converted into $2NaHO$, the change undergone by one molecule was the converse of that undergone by the other, and, therefore, that it was difficult to understand that the quantity of heat developed in the one case should differ from that absorbed in the other; but he observed that there was no direct proof that the reaction in question was attended with any evolution of heat: when water acted on oxide of sodium, a great part of the observed evolution of heat was certainly due to the combination of NaHO with excess of water, and it remained to be proved that this action was not the source of the whole. The conversion of BaO and H_2O into BaH_2O_2 he regarded as a reaction not perfectly comparable with the above, since, in this case, the two molecules BaO and H_2O coalesced into the single molecule BaH_2O_2. With regard to the different behaviour of what appeared to be similarly constituted molecules when subjected to the same treatment, as, for example, that of sodic and thallic hydrates at high temperatures—an instance to which Dr. Odling had referred—he considered that the explanation of them was to be sought in the influence exerted by each atom in a complex molecule upon the properties of all the rest. To help in forming a definite conception of the way in which such an influence might be exerted, he suggested that a diatomic atom (such as O) might be compared with a magnet with its two poles, while monatomic atoms (such as Na and H) might be compared with a single isolated magnetic pole, if such a thing were capable of existing. Then just as the south pole of a magnet would be strengthened by induction on bringing another south pole into contact with its north pole, so by bringing an atom of sodium into contact with one pole of an atom of oxygen, the attraction of the other pole for atoms of a certain kind might be increased, although its attraction for sodium might be lessened. If this comparison of a diatomic atom with a magnet was admitted, he pointed out that, in all cases, if an atom of a given kind applied to one pole of a diatomic atom tended to strengthen the polarity of the latter, an atom of the same kind applied to the other pole would tend to weaken it in an equal degree; and hence that, as a general rule, if two symmetrical molecules, such as NaONa and HOH, came together, the forces with which the atoms were held together would be, on the whole, increased by an interchange of atoms such as would convert the above molecules into NaOH and NaOH. Professor Foster added that he did not offer these remarks as affording a solution of the difficulties pointed out by Dr. Odling, but simply as indicating a direction in which he thought such a solution might reasonably be sought for.

Mr. **Chapman**, in allusion to an illustration made use of by the last speaker, said that if two bar magnets were placed end to end, the north pole of one being adjacent to the south pole of the other, the sustaining power of the free end of the lower magnet was *not* augmented; but that if they were laid parallel and alongside, with similar poles adjacent, the magnetic energy of each magnet would be greatly increased. (This statement was received with manifest indications of dissent.)

Professor **Wanklyn** had likewise a great objection to the continued use of the word "atomicity." Ammonia and salammoniac presented differences in constitution which were not easily explained, but it might happen that the *hydrogen* in these compounds had different values.

The meeting was then adjourned until Thursday, the 16th instant.

Thursday, May 16.

F. A. **Abel**, Esq., F.R.S., *Vice-President, in the Chair.*

The minutes of the previous meeting were read and confirmed; Mr. F. W. Peterson was formally admitted a Fellow of the Society; and the names of William Phipson Beale, barrister-at-law, Stone Buildings; Alfred Coleman, Plough Court, Lombard street; and Augustus Alfred Wood, 74, Cheapside, were read for the second time.

Mr. **Perkin**, F.R.S., said he had a few observations to make upon some experiments with which he had been lately engaged. Coumarine, it is well known, is the crystalline principle of the Tonka bean, and was first analysed by Delalande and afterwards by Bleibtren. The formula of this substance is $C_9H_6O_2$. When heated with potash, it assimilates an equivalent of water, and becomes converted into coumaric acid $C_9H_8O_3$. This, again, if fused with potash, yields salicylic and acetic acids, with evolution of hydrogen. On account of this latter transforma-

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[*] In Dr. Thudichum's Tables of New Atomic Weights, Molecular Weights, and Functional Types for the Lecture-room and the Study, published by Hardwicke, Piccadilly, this terminology is adopted, and the theory of the variable dynamicity of atoms indicated on the basis of some of the best-established facts.

tion, coumarine is viewed as a derivative of salicyle. Having been lately a good deal engaged with the study of salicyle derivatives, he had often endeavoured to obtain some clue to the constitution of coumarine, hoping that he might eventually be able to build it up from some salicyle compound. About ten days or a fortnight ago he obtained a beautiful crystalline product, possessing both the odour and composition of coumarine. When heated with potash, it yielded an acid, apparently identical with coumaric acid, and on fusing it with potash it was converted into salicylic acid—it was, in fact, artificial coumarine. He had not compared it side by side with the natural product, but expected to do so in the course of a few days. As he hoped shortly to bring an account of his results before the Society, he would refrain from entering into further details, except to state that he obtained his product from the sodium derivative of salicyle by means of acetic anhydride. Knowing the interest that chemists generally take in the artificial formation of natural products, he thought these remarks would not be unacceptable.

An elaborate paper "*On the Constitution of the Phosphites*," by Professor C. **Rammelsberg**, was read by the Secretary. The author commences with an historical notice of previous researches, which appear to have left unsolved the amount of water, or rather the condition of the hydrogen, contained in the salts of phosphorous acid, so that the constitution of these bodies might be represented by one or other of the following formulæ:—

$$H_2R''_2P_2O_6 \text{ and } H_4R''_2P_2O_7.$$

To elucidate this question, the author prepared and analysed a great many different phosphites, and the results in the case of the dyadic metals warrant the adoption of the second formula. Thus the barium aud nickel salts have precisely the above composition, the strontium and calcium salts containing extra two atoms of water, and magnesium salts were prepared containing respectively five and twelve atoms of water of crystallisation. Zinc and magnesium appear, however, to give rise to the production of salts of two classes, whilst lead, copper, manganese, &c., always form compounds containing one atom of hydrogen which cannot be replaced by a metal.

Class A.	Class B.
HK_2PO_3	$H_4Ba_2P_2O_7$
$HMnPO_3$	$H_4Ca_2P_2O_7$
$HZnPO_3$	$H_4Mg_2P_2O_7$
$HPbPO_3$	$[H_4Zn_2P_2O_7]$
$[HMgPO_3]$	

Phosphorous acid seems to be incapable of forming acid salts. The crystallised acid itself contains one atom of water, and any attempt to expel the latter by the application of heat destroys the substance, with evolution of gaseous phosphide of hydrogen.*

After the **Chairman** had moved a vote of thanks to the author, and invited discussion upon the subject of the communication,

Dr. J. H. **Gladstone** said it was sometimes easy to build up a compound and speculate upon its constitution, if we knew already the nature of the body from which it was prepared. The starting-point in this case was the terchloride of phosphorus, which, undergoing decomposition by water, furnished the acid in question:—

$$PCl_3 + 3H_2O = 3HCl + H_3PO_3.$$

The acid itself was stated, by Prof. Rammelsberg, to have the following composition:—$H_4P_2O_5 + H_2O$. To obtain this result, there were two stages, the first or intermediate product being the oxychloride of phosphorus—

$$\text{I. } 2(PCl_3) + \left.\begin{matrix}H\\H\end{matrix}\right\} O = \left.\begin{matrix}PCl_2\\PCl_2\end{matrix}\right\} O + 2HCl.$$

$$\text{II. } \left.\begin{matrix}PCl_2\\PCl_2\end{matrix}\right\} O + 4H_2O = \left.\begin{matrix}P(HO)_2\\P(HO)_2\end{matrix}\right\} O + 4HCl.$$

The **Secretary** then read a paper by Dr. A. **Dupré** "*On the Changes in the Proportion of Acid and Sugar present in Grapes during the Process of Ripening.*" It has frequently been inferred that the tartaric and malic acids in grape-juice become transformed into sugar during the process of ripening; but the author's experiments tend to disprove this assertion. Dr. Dupré collected and experimented upon a hundred berries of Riesling grapes gathered at intervals of a month, commencing with September last, and the amounts of tartaric acid, free and combined, and also of sugar, were determined in the separated juices. The proportion of sugar increased in order of time from 2·98 to 12·10 and even to 16·2[illegible] per cent. in the juice of the perfectly ripe fruit; whilst the entire berries showed but a slight diminution or no appreciable change in the total amount of acid present. The saccharine matter could not, therefore, have been directly derived from the organic acid or its salts contained in the grape; but the author thinks it possible that the presence of such acid effects a change resulting in the production of sugar similar to that known to occur in the conversion of starch into sugar by the action of sulphuric and other acids. Further experiments, even more decisive in their character, were made upon Gutedel and Muscatel grapes, gathered at the same time and from the same vine, but in various stages of ripeness. In some of the unripe berries there was absolutely no sugar, whilst in others nearly ripe 8·87 per cent. of sugar was found; but the amount of free acid estimated in a hundred grapes was almost the same in three samples of Gutedel, and actually increased with the ripening of the Muscatel.

Another paper, by the same author, was then read. It is entitled, "*On some of the Effects produced by the Addition of Plaster of Paris to Must.*" It seems to be a common practice in the wine-growing countries to add the substance named in the heading to grape-juice either before or during the process of fermentation, the alleged object being to increase the richness of the must by the absorption of water. Although this action doubtless occurs, the author points out that the loss of material in the course of such treatment (by mechanical retention in the gypsum) never compensates for the augmentation of sugar in the remaining juice, and suggests that a partial evaporation should be resorted to, or some sugar added instead of the earthy sulphate. Numerical results are quoted in support of this assertion, and the chemical changes induced by the employment of the plaster are fully stated. Consisting, as it does, of sulphate with a little carbonate of lime, it not only decomposes the tartrates in the grape, with liberation of the purgative sulphate of potash, but also neutralises and removes some of the free tartaric acid—an essential constituent of wine—leaving the malic acid still soluble, thus deteriorating, says the author, the condition of the juice, and assimilating it to the quality of that obtained from unripe fruit. Wine made according to the above system will be characterized—1st, by a more or less complete absence of tartaric acid; 2nd, by containing sulphates in solution; and 3rd, by giving, on incineration of the residue, a larger proportion of ash and diminished amounts both of phosphate and carbonate.

Dr. **Odling**, in answer to Colonel **Yorke**, said that chloride of barium always indicated the presence of dissolved sulphates in sherry, but gave no precipitate in a true claret.

The **Secretary** then proceeded to describe "*An Adapter to be used in connexion with Sulphuretted Hydrogen Apparatus*," by the Rev. B. W. **Gibsone**, M.A., B.Sc. This arrangement consists of an ordinary glass funnel, the limb of which fits into a conical bung of vulcanised india-rubber, whilst the top is covered with a flat plate of the same material, perforated with three holes, through one of which a current of sulphuretted hydrogen is made to enter, another aperture serving as the exit pipe for the excess of gas, which it is proposed to absorb by passing through a succession of Woulff's bottles charged with ammonia. The third is a spare hole usually closed with a stopper. Within the funnel itself is a long glass

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* An earlier statement respecting the phosphites, by the same author, appeared at page 241 of the present volume.—Ed. *C. N.*

tube, with a thistle-shaped expansion at top, into which is fitted a cork and short piece of tube for connexion with the central aperture above it, and this funnel tube has also within it a cane of glass rod of greater length than itself, with a bead placed inside the thistle as a means of lifting it from close contact with the apex of the larger funnel, which is kept moistened with water. When thus lifted a current of the gas makes its escape from the lower orifice of the funnel, and is made available by passing through any solution contained in the bottle to which the bung and adapter are fitted. When the action is completed the flow of gas is stopped simply by removing the vessel, when the weight of the glass rod causes the funnel tube to descend, and no more gas can pass. For a description of his sulphuretted hydrogen generator the author referred to a sketch of the apparatus which had already appeared in the Chemical News (*vide* p. 240 of present volume).

An elaborate paper "*On the Practical Loss of Soda in the Alkali Manufacture*," was then read by Mr. C. R. Wright, B. Sc. The author has had an opportunity of studying the minute details of Le Blanc's process as carried out in a large factory making upwards of 150 tons of alkali weekly, and the products were examined at every stage for the purpose of ascertaining the loss of soda during the process of conversion. A capital series of analyses are given, which show the average composition of salt cake, black ash, soda ash, refined ash, and the dried vat waste, which last seems to contain a notable quantity of alkali, both in the soluble and insoluble states. The total loss of sodium in the process of converting salt into refined soda ash is set down at 20·24 per cent., and consists of the following items:—

	Per cent.
Previous to Lixiviation.	
Sulphate of sodium left undecomposed	3·49
Insoluble sodic compounds formed	5·44
Volatilisation of sodic compounds	1·14
During and after Lixiviation.	
Soluble alkali left in vat waste	3·61
Oxidation of sulphide of sodium	—
Leakage and losses in white ash process	6 56
Total loss	20·24

The speaker made reference to the previous researches of Mr. Kynaston,[*] and also to those of Mr. James Hargreaves lately published in the Chemical News. The last-named gentleman affirms that one-seventh part of the chloride of sodium escapes conversion into sodic carbonate or is lost in the process of manufacture.

The Chairman moved a vote of thanks to the authors of the respective communications, and, at a late hour, adjourned the meeting until June 6, when Sir Benjamin Brodie will deliver a lecture "*On Ideal Chemistry.*"

ROYAL SOCIETY.

Thursday, May 2, 1867.

At the usual weekly meeting this evening the President read from the chair the names of the following fifteen candidates recommended by the Council for election. They will be balloted for on June 6:—William Baird, M.D.; W. Boyd Dawkins, Esq.; Baldwin Francis Duppa, Esq.; Albert C. L. G. Günther, M.D.; Julius Haast, Esq., Ph. D.; Captain Robert Wolseley Haig, R.A.; Daniel Hanbury, Esq.; John Whitaker Hulke, Esq.; Edward Hull, Esq.; Edward Joseph Lowe, Esq.; James Robert Napier, Esq.; Benjamin Ward Richardson, M.D.; J. S. Burdon Sanderson, M.D.; Henry T. Stainton, Esq.; Charles Tomlinson, Esq.

QUEKETT MICROSCOPICAL CLUB.

The usual monthly meeting was held at University College on the 26th instant, Mr. Ernest Hart, President, in the chair.

Dr. Halifax described his ingenious method of obtaining thin sections of insects, soft vegetable tissues, minute seeds, &c., by immersion in wax, and afterwards slicing them upon the ordinary section table.

Mr. Higgins gave a lengthened and interesting description of the "otolithes" or earbones of fishes, to the study of which he has devoted himself with remarkable industry for the last eighteen years. The result of many thousand examinations of fossil and recent fish has enabled him with positive accuracy to identify species, and in many instances genera. His remarks were illustrated by an extensive series of "otolithes," which were displayed in cases in the room, containing specimens obtained from the largest to the smallest fish, both freshwater and marine.

The meeting, which was attended by upwards of 130 members and their friends, terminated with a *conversazione.* Eight members were elected.

MANCHESTER LITERARY AND PHILOSOPHICAL SOCIETY.

Ordinary Meeting. April 16, 1867.

Edward Schunck, *Ph.D., F.R.S., &c., President, in the Chair.*

Mr. Henry Charles Beasley was elected an ordinary member of the Society.

"*On a New Form of the Dynamic Method for Measuring the Magnetic Dip*," by Sir William Thomson, M.A., D.C.L., F.R.S., &c., Honorary Member of the Society.

Seven years ago an apparatus was constructed for the natural philosophy class of the University of Glasgow, for illustrating the induction of electric currents by the motion of a conductor across the lines of terrestrial magnetic force. This instrument consisted of a large circular coil of many turns of fine copper wire, made to rotate by wheel work about an axis, which can be set to positions inclined at all angles to the vertical. A fixed circle paralled to the plane containing these positions, measured the angles between them. The ends of the coil were connected with fixed electrodes, so adjusted as to reverse the connexions every time the plane of the coil passes through the position perpendicular to that plane. When in use, the instrument should be set as nearly as may be in the magnetic meridian. The fixed electrodes being joined to the two ends of a coil of a delicate galvanometer, a large deflection is observed when the axis of rotation forms any considerable angle with the line of magnetic dip. On first trying the instrument I perceived that its sensibility was such as to promise an extremely sensitive means for measuring the dip. Accordingly, soon after I had a small and more portable instrument constructed for this special purpose; but up to this time I had not given it any sufficient trial. On the occasion of a recent visit, Dr. Joule assisted at some experiments with this instrument. The results have convinced us both that it will be quite practicable to improve it so that it may serve for a determination of the dip within a minute of angle. I hope, accordingly, before long to be able to communicate some decisive results to the Society, and to describe a convenient instrument which may be practically useful for the observation of this element.

"*Observations on the Alteration of the Freezing Point in Thermometers*," by Dr. J. P. Joule, F.R.S., V.P.

Having had in my possession, and in frequent use, for nearly a quarter of a century, two thermometers, of which I have from time to time taken the freezing points, I think the results may offer some interest to the Society. Both thermometers are graduated on the stem, and are, I believe, the first in the country which were accurately calibrated. Thirteen divisions of one of them correspond to one degree Fahrenheit. It was made by Mr. Dancer, in the winter of 1843-44. My first observation of its freezing point was made April, 1844. Calling this zero, my successive observations have given

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[*] *Journ. Chem. Soc.* xi. 135.

0	April, 1844.
5·5	February, 1846.
6·6	January, 1848.
6·9	April, 1848.
8·8	February, 1853
9·5	April, 1856.
11·1	December, 1860.
11·8	March, 1867.

The total rise has been, therefore, ·91 of a degree Fahrenheit. The other thermometer is not so sensitive, having less than four divisions to the degree. The total rise of its freezing point has been only ·6 of a degree; but this is probably owing to the time which elapsed between its construction and the first observation being rather greater than in the case of the other thermometer. The rise of the two thermometers has been almost identical during the last nineteen years.

"*On the Casting, Grinding, and Polishing of Specula for Reflecting Telescopes, Part IV.*," by James Nasmyth, Esq., C.E., Corresponding Member of the Society.

In this part of his paper the author gives detailed descriptions, illustrated by diagrams, of his methods of mounting the specula of reflecting telescopes, and of testing the figure of the great speculum; he also offers some very useful remarks on the general management of Newtonian reflecting telescopes, and on the atmospherical circumstances which affect their performance.

PHOTOGRAPHICAL SECTION.

April 9, 1867.

J. Baxendell, *F.R.A.S., Vice-President of the Section, in the Chair.*

Mr. Brothers read the following "*Note on Photography in 1787.*"

It is generally supposed that the earlier attempts to use nitrate of silver for producing pictures of lace, leaves, and other objects on white leather or paper were made by Wedgewood and Davy about the year 1802; but it will appear from the following extract that at least fifteen years earlier than the date named, and within ten years of the time when Scheele investigated the subject of the action of light on the salts of silver, the possibility of utilising the action of light was known. The title of the book from which the extract is taken is "Rational Recreations in Natural Philosophy," &c., by W. Hooper, M.D., 1787; and the paragraph is headed "How to print letters by sunlight."

"Dissolve chalk in aqua fortis to the consistence of milk, and add to that a strong dissolution of silver. Keep this liquor in a glass decanter well stopped, then cut out from a paper the letters you would have appear and paste the paper on the decanter, which you are to place in the sun in such a manner that its rays may pass through the places cut out of the paper and fall on the surface of the liquor. The part of the glass through which the rays pass will turn black, while that under the paper will remain white. You must observe not to move the bottle during the time of the operation."

Mr. Coote exhibited some snow scenes, the negatives of which were taken on collodio-albumen plates. Some of these beautiful views were slightly defective in the high lights, a number of vein-like markings appearing in the sky and foreground.

Mr. Wardley stated that these defects were entirely caused in the development, and had no connexion with the character of the collodion used, or with the preparation of the plate. He considered that the imperfections were produced entirely by the repellent or nonmiscible nature of the solutions, containing acids and salts, used in development, acetic acid being one of the chief causes of the defects. Another source of the evil may be a low temperature and the developing solution being allowed to rest, even for a moment, on the plate. Such defects may be produced in abundance on any kind of dry plate if the developing solution is allowed to rest.

ROYAL INSTITUTION.

Tuesday, May 14, 1867.

*A Course of Four Lectures on Spectrum Analysis with its Applications to Astronomy,** by William Allen Miller, *M.D., LL.D., Treasurer and V.P.R.S., Professor of Chemistry, King's College, London.*

Lecture I.

Nature of the Prismatic Spectrum.—Spectra of Solids.—Spectra of Gases and Vapours.—Spectra produced by Absorption.—Spectra of Rays of Heat, of Light, and of Chemical Action.

My object in the course of lectures upon which we enter to-day will be to endeavour to place before you distinctly the mode in which, by the optical analysis of light from various sources, we have learned not only to distinguish the physical condition of the body which emits that light, but also, in many cases, to ascertain its composition.

A new method of investigation is thus placed in our hands. The mode of analysis, it is true, has been practised before to a certain extent, but it involves the application of principles which, until quite recently, had been overlooked. It enables us to deal with matter at infinite distances, for, provided we can see it, we are able to examine it by this optical method. It also enables us to examine matter in quantities so minute that no balance can estimate its amount. We are therefore brought face to face, by its means, with infinitude of space and distance, and infinitude of minuteness.

I know that those who have been in the habit of attending lectures at this Institution have from time to time watched with interest the progress of this new branch of investigation. They have had opportunities from time to time of seeing the steps by which it has been raised to its present position, illustrated and experimented on; but although many who may be honouring me with their attendance on this occasion may not for the first time be considering these wonderful phenomena, the intrinsic interest which they possess—the variety and beauty of experimental illustration of which they admit—induce me to hope that even those who are in some measure familiar with the facts will yet be able to follow me with interest through the theoretical considerations which I may have to bring before them, and through the somewhat minute detail upon which, from time to time, I shall have to enter.

Without further preface, then, let me proceed to the examination of the method of inquiry—the method of optical analysis.

No doubt every one present knows what is meant by the prismatic spectrum. If we take a beam of white light, and transmit it through a triangular mass of glass properly cut and polished, we divert it from its original direction—we refract it from its course. Now, light may be artificially produced in various ways, and one of the most general of these modes of obtaining light consists in raising the temperature of the body which is to be experimented upon. Every opaque solid object gives out light in large quantity when its temperature is sufficiently raised. At a temperature of about a thousand degrees of Fahrenheit every substance becomes what is called red-hot—that is to say, begins to emit red light. As we raise the temperature, the colour of the light becomes more brilliant, and it passes at length into a dazzling white. That is a general effect whenever any solid opaque substance is sufficiently heated. I shall take advantage of this fact in order to be able to produce the beam of light upon which we shall have to experiment in a part of this investigation, and for this purpose it will be my desire to show you, first of all, the solid body itself which is

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* Reported specially for this paper, and revised by the author.

producing the light, because, hereafter, I shall have to show you other sources of light which are not solid, and which are giving out light of a different kind.

In the first place, then, I shall, by the powerful heat produced by the current of electricity of the voltaic battery, cause intense ignition of two points of charcoal. You will excuse me if from time to time in these lectures I bring before you what appear to be very simple experiments. Philosophically, nothing is trifling if it proves a principle. In this case I shall throw upon the screen the image of two charcoal points. My object here is simply that you may see that although we get an intense light and a white light, it is produced by a body in the solid state. The charcoal points do not melt, and there is no visible vapour given off. I do not say there is no vapour given off, but there is none visible. On throwing this image upon the screen, we shall see, first of all, the incandescent points themselves, separated from each other to a considerable distance. Having shown you the points, we will then examine optically the nature of the light which those points are emitting.

You will observe that the image of the line of light is produced by allowing simply a thin slice of light to pass through a very narrow slit. It is now falling upon the surface of a mirror, and from that mirror it is reflected upon the screen. We will now withdraw the mirror altogether, and allow the light to pass through the prisms which have been arranged for its dispersion. So long as these points of ignited matter are in the solid condition, we have that beautiful elongated image which now appears upon the screen, and which is, in fact, composed of a series of slices of light, each of which is of a particular and definite colour. You will notice that the extremity which is nearest to me—the red—is that which is least refracted from the original direction, and the extremity which is farthest from me—the violet—is that which is most refracted from the original direction. Now, the point I particularly wish to insist upon in this observation is that we are here dealing with a *solid* body, which is not capable of being converted into vapour so as to produce flame. If time allowed, I should like to show that this, which is observed in the case of charcoal, is a general fact with regard to solid bodies. For example, if, instead of heating charcoal points by the voltaic current, I were to take a cylinder of lime and introduce it into the intense heat of the oxyhydrogen jet, or if I were to take a piece of magnesium, a piece of silica, a mass of iron, or a mass of platinum, and heat it so as to produce incandescence or intense ignition, the spectrum which would be obtained from its light, would, in every case, when examined, be seen to be continuous, like that charcoal spectrum. [I dare say you noticed flashes of light which crossed that spectrum from time to time. They were due to slight impurities in the charcoal points, and these impurities become volatilised in the intense heat. With them we shall have presently to deal.] The particular fact upon which I wish now to insist is that, as a general rule, whenever a solid body is heated, it gives out light, which, when examined by the prism, furnishes us with a continuous spectrum. That is to say, this light is made up of an infinite number of slices of light. By means of a rough model, I may make this more clear. Suppose that this white bar represents to us a band of white light—the light, in fact, which we first got before it is dispersed by the prism. If we cause it to pass through the prism, it is spread out in this fan-like manner, every piece of that spectrum being composed of a slice of light of the particular colour which is there represented; and these slices of light, overlapping each other continuously, produce that blending of colour which is inimitable by art, but which is so brilliant and so beautiful in its effect.

These, then, are the results of the examination of bodies in the solid condition, and for the purpose of recalling this to your mind we have here a diagram representing the spectrum of any sufficiently luminous solid body. And, further, if the solid happens to melt, it still gives out a continuous spectrum. A mass of melted cast-iron, for instance, would furnish a continuous spectrum, as in the case of charcoal. Or if we were to take any other metal which can be heated sufficiently to be melted without boiling and volatilising, we should obtain a similar result. I cannot show you this experiment, because I am not able by means of the voltaic battery sufficiently to regulate the heat.

Here let me notice that in spectra of this kind we have no clue to the chemical nature of the body which produces the spectrum. But if we go a step further, and heat the body sufficiently to convert it into vapour, the spectrum which is then obtained is quite of a different nature. Instead of having a continuous spectrum, we shall have an interrupted spectrum; and I am going now to prove to you that the body upon which I am experimenting is converted into vapour before it furnishes a spectrum, and for this purpose shall introduce into the lamp a charcoal cup, upon which we shall place a piece of silver. That silver will immediately melt with the intense heat of the current, and as the current is continued, it will not merely melt, but it will actually boil, and become distilled. In fact, silver has been distilled on a large scale by Stas, for the purpose of its purification. [The image of the charcoal points on which the silver was placed, was thrown on the screen.] That beautiful green arc is the vapour of silver. The silver, you will remember, appears on the upper part of the screen, in consequence of the inversion of the image. What in the lamp is the lower part is here upon the screen the upper part. You will see a number of little glowing points, which are the distilled silver. Well, now, if we could manage to open out these glowing points of melted silver (but which we cannot in the present form of our experiment), we should see that they were giving out light of all colours, but the arc itself is giving out light of a particular colour.

We shall now examine the mode of discriminating between silver and other bodies, by means of this process of optical analysis. In another lamp I have exactly the same arrangement as that which you have here seen thrown upon the screen; but, instead of making the image of the points fall directly upon the screen, I shall let a slice of light only fall upon a lens in front of the slit, and then upon a prism. Now, you will notice that the instant the temperature rises sufficiently we get these two magnificent green bands. These two bands are the characteristic marks of silver in vapour. They constitute the arc of light which we saw just now upon the screen, and which is here, by means of the prism, spread into its component parts. You will notice that there is on the ground of these bands a small amount of light, which is due to the dispersion of the light from the charcoal points. We cannot avoid that. In these cases we do not get the pure light of the substance upon the points, because the heat which is produced is sufficient to give us a tolerably strong spectrum of the charcoal points. But you will observe how very much those green lines predominate over the other light in that spectrum.

The next fact which I have to bring before you is that though every substance which is capable of being converted into vapour gives a spectrum, it gives a spectrum *of its own*. If you take substances which to the naked eye appear to possess exactly the same colour, the moment you place them in the voltaic arc the vapour given out by each is capable of being distinguished from the others, and you have a different spectrum for each body. I will illustrate this by means of four different bodies, each giving a green light. I have shown you the spectrum of silver. Now I will take three other bodies, each giving a green light, differing a little in its shade from that of the others. The first of these will be metallic copper, which, like other metals, will boil in the intense heat we can here produce. The vapour of copper will give us a spectrum of its own, which, although green, will not be the same as the spectrum of silver. [The spectrum was shown on the screen.] There you see a series of green bands, but in the case of copper there is also a large quantity of red and orange light. You see a series of channeled or grooved spaces through a pale background, due to the light from the charcoal points themselves; but the brilliant portion of the spectrum is produced by the volatilisation of the metal. You will be able readily to contrast this copper spectrum with the spectrum of the silver which we had before. Here, you observe, we have a green, but a green of a different order. Copper is a more fixed metal than silver, but by means of this intense heat it becomes converted into vapour. Solid copper, if heated, would not give us this line, but a continuous spectrum like lime and like charcoal.

I am now going to take another metal, which also gives us a green light, and I have a special reason for selecting this metal—magnesium—because we shall consider it from another point of view hereafter. Magnesium is a metal which, when burned with proper precautions, gives rise to a beautiful green light. I dare say we shall see some blue also, but the principal part of the magnesium spectrum will be a green light, but a green of a different shade from the others. In this case you will not see the fact that the green light of magnesium is concentrated into three bands. These three bands are so exceedingly close together that the apparatus we have at our disposal for throwing them on the screen does not enable us to separate them. In this spectrum of magnesium you will see a certain blue line, and there is also a faint line nearer to the yellow.

The fourth metal which I have selected also gives a green light, and this is likewise an extremely interesting body. It is thallium, a metal which was discovered by the application of the method of spectrum analysis. [The spectrum was produced.] That is the band which is perfectly characteristic of thallium. Whenever we have pure thallium we have that single intense green line.

From these experiments it is obvious at any rate that these four substances have totally distinct spectra, although the light which these metals emit appears similar when viewed with the prism. If we were to throw slices of their light directly upon the screen, we should not be able to distinguish these metals from one another; yet, by spreading them out in this fan-like form by prismatic analysis, it is easy to distinguish one from the other.

We have then here a second set of spectra, and these spectra are such as are represented in the second of our diagrams. They are spectra which are not continuous—interrupted spectra—spectra with bright lines. These occur in the case of flames and ignited vapours and gases. It is true that in one instance a solid body has been known to give out lines like this, but it is a substance which I suppose not a dozen people have ever seen. Still it is an important fact that the metal erbium, in the form of its oxide erbia, even in the solid, has the power of giving out bright lines when its spectrum is viewed. This is an awkward fact for the theory of spectrum analysis, but at the same time it is one which must not be ignored. We are always discovering facts which do not square with our theories; and the more these facts are examined, the more surely are we led on to correct our theories, formed, as they always necessarily are, from partial knowledge only. This exception in the case of erbium does not, however, invalidate the general conclusion that whenever we see a body which emits a spectrum having bright lines, that body is in a gaseous condition. We do not as yet know why erbium makes an exception, though at some future time, probably, we shall be able to account for the apparent anomaly, the discovery of which we owe to the observations of Bunsen, to whom also we are indebted for a great part of our knowledge of spectral phenomena.

We must now turn our attention to the cause of these remarkable phenomena; and here I must ask you to follow me for a few moments through a little speculation—not that it is new, but it is speculation which is necessary to connect our ideas—speculation as to what light is. The notion of the nature of light which is at present adopted by philosophers is of this kind:—Filling all space, and filling the interstices of all kinds of matter, there is a subtle something which, for want of a better name, is called "ether." This ether has no weight. It is not light itself, although it is the means by which light is manifested to us. (Mind, this is all speculation, but still it is necessary.) When this ether is thrown into vibration, the vibrations are transmitted through space in right lines, radiating in all directions from the point at which the vibration is produced. When these vibrations have a certain degree of frequency, they produce the phenomena of radiant heat; when they have a somewhat greater frequency, they produce phenomena which are manifested to us in the shape of light; and when these vibrations are more frequent still, they produce phenomena which are manifested to us in chemical effects, or in those effects which Professor Stokes has taught us to associate with the term "fluorescence." Now, the different degrees of frequency with which the ether can be made to vibrate give rise to certain phenomena in light. Not merely is light a vibration of one particular frequency, but the different kinds of light are produced by vibrations differing in the degree of their frequency. Those portions of light which are least refracted (the red) are produced by vibrations of the ether of the lowest frequency; those portions which are most refracted (the violet) are produced by vibrations which are of the greatest frequency; and intermediate between these we have vibrations which produce all the intermediate colours. Now, although it is true every one here is prepared to hear wonderful statements of this kind, I was going to say I scarcely dare to mention the number of these vibrations which it is calculated must occur. Let me first tell you what the width of a wave of light must be. Every wave of this ether, as I have stated, is liable to vary in width according to the colour of the light. In red light the width of a wave is about the 34,000th part of an inch. A wave of violet light is a little more than the 60,000th of an inch—that is to say, there would be a series of 60,000 of these little waves in the space of an inch. That seems a tolerable number, but when we come to the frequency with which these undulations succeed each other, it is perfectly marvellous. In red light there are 482 millions of millions in a second of time, and there are upwards of 707 millions of millions in the case of violet; and we may fill up the interval of the intermediate colours with every conceivable variety between the two.

Now, it is very difficult, when one is looking at these things for the first time—and, indeed, it is always difficult—really to grasp these things, and therefore it is needful that we should come down to something a little more within our ordinary range of conception. I shall, therefore, take an illustration or two from another set of vibrations which you had most admirably illustrated not long since by Professor Tyndall. I cannot help reminding you of one or two beautiful experiments which he brought before you, and which show the beautiful analogy between colour and sound, for, in point of fact, red is the bass of light, and violet the treble. You all know that musical notes are produced by a certain series of vibrations which occur in definite number and at perfectly regular intervals, each note having its own specific number of vibrations—the middle C, for instance, in a pianoforte, having 256 vibrations in a second. C in the octave above has double that number, or 512. Here is a tuning-fork which will give us a note which will cause the air in this box to vibrate. [The fork was struck, and held over the mouth of the box, whereon a musical note was obtained]. This fork is producing a certain number of vibrations, which vibrations correspond to a column of air of a particular length contained in the box. Now, if I take another of these forks and hold it over the next box, we get a note which is the octave above. [Experiment performed.] There are twice the number of vibrations in that sound as there were in the sound which I produced first. In the case of musical notes we have a series of these sounds succeeding each other in order. Now we take another fork, and hold it over the next box; then, again, the next; and then the last, this being the highest of all. [A musical sound was evoked in each case, each successive note being higher than the preceding.] Now, these are, in point of fact, to sound what colours are to light. In the case of light, we cannot get through the octave. If we assume that the proportion of vibrations in red light be 100, those for the violet light will not exceed 175; so that the number of vibrations, immense as it is in the case of light, does not embrace so wide a range as in the case of sound, for we may have musical vibrations ranging from 16 in a second to upwards of 2000 in a second.

I have no doubt that many of you know the beautiful experiment which I first saw performed by Dr. Tyndall here, and it is so beautiful that I cannot help desiring to show it to you again, although I may not succeed so well as he does, because it is a matter which he has made his own. The experiment is this:—If we take two tuning-forks, one of them an octave above the other, and each having a thin wire attached to the limb of the fork, and cause them to vibrate, and if we then draw them at the same rate across a piece of smoked glass, we shall have a sinuous line, which will represent the motions of each fork. [A piece of smoked glass was marked as described.] We will put this into the lantern, and you will see on the screen two sinuous lines, one above the other. The lower line is that which is produced by the bass sound; the upper one is that which is produced by the higher sound. The sinuosities in the upper line are double the number of those in the line below. In this we have ocular proof of the difference between the rate of vibration of the two forks. If we were to rule lines across, we should find that the bends in one of these lines were twice as numerous as in the other. This is one of the simplest and, at the same time, the readiest proofs we can have of the difference in the vibrations.

Now I must ask your attention to a third set of spectra. We produced, first of all, a continuous bright spectrum, then an interrupted bright spectrum; but what would happen if we interposed between a continuous spectrum and the light something which would take part of the light away? If we put an opaque body, we should arrest the whole of the light; but if we introduce a transparent coloured substance, we shall intercept portions of the light. Now, by properly choosing our media, we may produce interrupted spectra with black or dark lines upon them; and these are spectra of a high degree of interest. I shall exhibit one or two of the methods by which we may produce these effects. When light is transmitted through a solution of permanganate of potash without using the prism, the liquid has a splendid purple or red colour, according to the degree of dilution. We will first show the spectrum of white light, and then we will interpose the permanganate. You will see, when this solution is placed in a glass cell with flat sides, so as to intercept the spectrum from the charcoal points, we obtain a certain number of bands, which occur at intervals upon the screen.

I now proceed to the examination of one of the salts of the rare metal called didymium. Nitrate of didymium and potash furnishes a solution of a very pale red colour. When this is introduced in the course of the ray you see two remarkable bands, one in the orange and the other in the green. Besides this, portions of the blue rays are also cut off. This is a spectrum of absorption produced by a liquid which has so faint a colour that it is scarcely perceptible to the naked eye, yet its spectrum is perfectly characteristic. Dr. Gladstone, who first pointed out this peculiarity of didymium, was enabled by its means to discover didymium as an impurity in other bodies previously supposed to be free from it. These two last spectra, you will observe, are absorption spectra produced by liquids. Such absorbent actions are important, as they enable us in many cases to distinguish the nature of the bodies which are held in solution. Professor Stokes has insisted particularly upon the value of studying this class of actions; and quite recently a valuable paper upon the subject has been communicated to the Royal Society by Mr. Sorby. But, interesting and important as this branch of inquiry is, it would lead us too far astray from our immediate subject, which is specially concerned with the examination of the spectra of bodies at a very high temperature, and the action upon such spectra of gaseous bodies, either at ordinary or at elevated temperatures. The phenomena with which we have at present to deal, enable us to examine the constituents of the gases in furnaces, in active volcanoes, in the fixed stars, in meteors, and in those still more enigmatical bodies, the nebulæ.

For this purpose we will now examine the absorbent action of a brownish-green coloured gas, the peroxide of chlorine. You see there are bands coming out, there being an absorption, particularly of the blue and violet portion of the spectrum, to a very considerable extent. The bands are still more marked in those portions of the spectrum which are not visible until they are received upon a fluorescent screen. The next gas which we will introduce in the same manner, interposing it exactly in the track of the ray, is the peroxide of nitrogen (the red nitrous fumes which are produced whenever a metal, such as copper or mercury, is acted upon by nitric acid). The bands in this gas are in a different portion of the spectrum. The green is almost abolished, and in the green and orange we have a variety of dark bands appearing. These bands are of particular interest because they were the first absorption bands which were observed. Sir David Brewster, who discovered them, thought that he saw in them a clue to the explanation of certain bands known as Fraünhofer's lines which are observed in the sun's light. This turned out to be only a partial foreshadowing of the truth, but still the fact is interesting in the history of these discoveries.

I have now shown you absorption by two coloured gases, both of them, however, at the temperature of the air. Suppose we now take a substance which is highly heated, and then examine what will be the effect of transmitting light through a vapour of this description. For this purpose I will now place in the lamp a substance which gives a light of one colour only. Sodium, when converted into vapour, gives out light concentrated into two extremely narrow bands, very close together; these are actual mathematical lines of light which cross the spectrum in the midst of the yellow. I cannot show you these lines on the screen, for the form of the apparatus is not calculated for the production of these extremely sharp lines. As the temperature rises, the sodium becomes converted into vapour, and it will gradually become more and more brilliant until you will see that this bright line is crossed by an intense black line, showing itself upon the screen in the position which was, a few moments ago, occupied by the sodium line itself. It flickers for an instant, and then it gradually fades. Again it appears, and you may now see distinctly the black line of the sodium thrown upon the screen. That is an experiment which, simple as it looks, is really the foundation of the whole, and therefore it is that I have taken a little more time than usual in obtaining the result. I need not apologise to you, I am sure. You are accustomed to look at these things, and to value them, not for the brilliancy of their appearance, but for their real importance with regard to the subject in hand. Now, this sodium light has, as you will observe, the power of causing a black absorption band, when light, produced at a very intense temperature, is allowed to fall upon the incandescent vapour or flame of the metal at a lower temperature. Those are the conditions under which the absorption band is seen, and the person who first pointed out the real significance of the fact was Kirchhoff. The actual fact was first observed and described by Foucault, and Mr. Stokes suggested an explanation, which turned out to be the true one; but although he divined it, he did not directly prove it by experiment, or publish it to the world, and so make it his own. Kirchhoff, however, not only saw that this sodium vapour absorbed the light of the luminous body behind it, but that in this fact lay the explanation of those remarkable dark lines in the sun's light, which ever since they were first indicated by Wollaston, and carefully examined by

Fraünhofer have been a mystery to all philosophers. The black lines of Fraünhofer are represented in this diagram, which is a repetition of the upper spectrum, *plus* a certain number of black lines which cross it at intervals. The black sodium line has a corresponding black line in the solar spectrum exactly at that part which is marked D. I say "*a* black line." It does, in fact, consist of two lines, which can be discriminated from each other by the use of telescopes of sufficient power.

Now, I desire, if I can, in a few words, to explain to you how this black line is produced. It certainly seems a remarkable thing that the addition of two lights should apparently produce darkness. We know this, however, for a fact in other cases; for light, and in some cases sound, produces the phenomenon of interference, as it is called; but this is not a true case of interference—it is a case of absorption. The spectrum of sodium has the power of absorbing only that thin line of light which it made upon the screen, just as, when we take one of these forks and hold it over the particular box which responds to it, and which absorbs its vibrations, it produces a sound; but the fork produces no sound when held over a second box of different dimensions [holding it over another box]. In the first case, we have a powerful resonance, but there is nothing perceptible from either of these other boxes. Well, this furnishes us an analogy—though a rough one—to the way in which the sodium vapour acts in taking up the vibrations which are produced in that particular slice of light from the sodium, appropriating them to the actual raising of its own temperature, and then radiating them out—wholly absorbing them first, and then reradiating them.

How is it that this is a black line? In the black line, as it appears upon the screen, it is true that there is a greater amount of light than the sodium alone could produce, and it is black only by contrast. That part of the spectrum which looked black just now would have appeared bright if seen alone, but as I had a more brilliant spectrum behind it, the light of that spectrum, in falling on the screen, produced upon the eye the effect of a contrast, which led you to believe that the comparatively feebly illuminated sodium line was actually black. That is the cause of the black lines we appear to see in the spectrum of the sun, of the fixed stars, and of a variety of other lights.

I intended to say a word or two upon the composite nature of the solar spectrum before I concluded this part of our subject; but I need not dwell long upon this point, as it has already been insisted upon more than once in the theatre of this Institution. I stated just now that if we obtained the solar spectrum, and examined it—spread it out—we should have a certain amount of light, which we may represent by that band of coloured light which is seen in this diagram. If you notice, you will observe that that coloured stripe is bounded by a curved line above. This is the red end, and here it goes off into the violet. Now, this curve is the result of measurements made very carefully by Fraünhofer for the purpose of ascertaining what is the distribution of light in the different parts of the solar spectrum. A second curve traces the outline of this black mountain, which Dr. Tyndall has been working at, and is intended to indicate the distribution of heat in the spectrum. It is very important to remember that the light given by the sun is but a small portion of the force which it is radiating upon the earth. The portion of the curve filled up with black represents that part where the vibrations are least rapid. Then, when the vibrations increase in rapidity, we get red light. Still more rapid vibrations give us yellow; still more rapid give us green; then we get to the blue and violet. The principal curve in the diagram represents to the eye very roughly, but to a certain extent correctly, the distribution of heat in the visible portion of the spectrum. The heat increases in intensity as we approach to the red, and the light diminishes. The third curve represents the distribution of the chemical rays. If you allow the spectrum from the light of the electric spark taken between points of silver to fall upon the photographic surface of a collodion film, you get a very long strip of light, in which you have a series of the same sort of bands as you have already seen in the luminous portion of the silver spectrum. The vapour of silver gives us a series of interrupted bands, which exert a powerful effect in photographic experiments.

One experiment before I conclude, in order that I may show you the photographic image of the spectra obtained by transmitting a series of powerful electric discharges between wires of the four metals which I spoke of just now. First, the spectrum of silver. There you will see the image prolonged. This is the most refrangible part. The photographic image of the electric spark between wires of silver is five or six times as long as the visible spectrum obtained from silver when heated in the voltaic arc. Here is the photographic spectrum of magnesium, and here is a photograph of the spectrum of thallium. About one-tenth only of the length of any of these spectra is visible to the eye. Finally, here is the photographic spectrum of copper.

PHARMACEUTICAL SOCIETY.

Wednesday, May 15, 1867.

G. W. Sanford, *Esq., President, in the Chair.*

The twenty-sixth annual general meeting of this Society was held on Wednesday, the 15th inst., when the report of the Council was received, and the Council and Audit Committee for the ensuing year were elected. The meeting then resolved itself into a special general meeting, convened for the purpose of taking the sense of the members of the Society relative to the 19th clause in the amended Pharmacy Act. This was one of the largest meetings of members ever held. Mr. Abrahams, of Liverpool, moved, and Mr. Boyce, of Chertsey, seconded, a resolution condemning the policy of applying for an amended Act when the Society was in such a prosperous state. Messrs. Pedlar, Richardson, and others thought that by admitting all chemists without examination they would be doing an injury to those members who had already passed the examinations. After a long discussion, Mr. Collins, of St. Pancras, moved, and Mr. E. Vizer, of Pimlico, seconded, the following amendment:—"That, in the opinion of this meeting, the proposed amendment of the Pharmacy Act is both wise and expedient, as, by enlisting the support of those members of the trade outside the pale of the Society, the way is cleared for carrying into effect the primary objects of the Society—viz., the consolidation of the whole trade, and legislative provision for the compulsory examination of all persons entering the same after a given time. This meeting would further express its entire approval of the action taken by the Council, and pledges itself to support, by all possible means, the passage of the Bill through Parliament." Messrs. Edwards, Squire, Orridge, Morson, Savage, and Randall supported the amendment, which, after considerable discussion, was carried by a large majority.

ACADEMY OF SCIENCES.

April 29, 1867.

(From our own Correspondent)

The sitting of the Academy was very short to-day.

Sir David Brewster presented the opening discourse which he had made at the Edinburgh Royal Society.

M. Baer, of St. Petersburg, warmly thanked the Academy for having given him the Cuvier prize of 1866.

M. Baumhein, present at the meeting, called the attention of a considerable number of members, particularly M. Fizeau, to a forgetfulness on the part of the commission appointed to award the prize for the determination of the length of the waves of the solar spectrum. He deeply regretted that they had not known the remarkable memoir of M. van der Willigen, director of the Teyler Physical Cabinet at Haarlem, which was more complete than that of M. Mascart in 1866.

M. Dumas read a letter, in which M. Pasteur, at present at Alais, and who is finishing his experiments and observations on the precocious rearing of silkworms, announced an important discovery made by him. The organisms that he terms *corpuscles* are propagated, or at least multiplied, by *scissiparity*. They contain a sort of kernel, which ordinarily presents the first indications of scissiparity. M. Pasteur has examined under the microscope the corpuscles of the internal mucous coats of the stomach and the kernels, at all states of division, commencing, in progress, or terminated.

M. Dumas, in the name of M. Naquet, presented the second edition, in two volumes, of his "Principles of Chemistry founded on Modern Theories." It is a well-written work, quite up to the modern progress of science. M. Dumas, however, reproaches the author with having regarded facts too much in a personal light, and enumerating the results without mentioning the processes. Formerly, said the celebrated chemist, the processes were an important item in the teaching of chemistry; and also he has further remarks to make on this reserve, which he will make on a future occasion.

M. Fremy presented a memoir on "*Phenol*," in which M. Dussard announced that he had succeeded in perfectly and easily producing the phenaphtic acid of Berzelius, and preparing diatomic phenol possessing very remarkable properties, and which will bear the same relation to the monatomic phenol as the glycol of M. Wurtz bears to monatomic alcohols.

M. Henri Sainte-Claire Deville presented a note, by M. Cailletet, on an amalgam of sodium with which he has already obtained considerable success. He called to mind the important results obtained by the modern researches of Messrs. Crookes, Matthiessen, Regnault, &c., on alloys. Mr. Crookes, for example, had rendered great service to metallurgical operations by proving that the addition to mercury of a small quantity of sodium renders incomparably more easy and profitable the extraction of the precious metals.

Mr. Sterry Hunt read the summary of his researches upon certain reactions of magnesian salts and magnesian rocks. The author attacked the theory of MM. Haidinger and Suckow, who explain the efflorescence of sulphate of magnesia by the reaction of sulphate of lime and carbonate of magnesia. He believes that the magnesian silicates which form portion of the dolomites in the environs of Paris are the representatives of the unaltered formation of steatites; that the talcs and serpentines are formed aqueously; that the greensands of the Paris basin are of the same composition as serpentines, &c.

May 7, 1867.

M. Bertrand read a very favourable report on the memoir of M. A. Cornu, entitled, "*Théorie Nouvelle de la Réfraction Crystalline de Fresnel.*" The principal conclusions of the report were, that the luminous vibrations were normal to the plane of polarisation, as Fresnel and Cauchy announced a long time ago, though the direct proofs hitherto proposed are open to discussion.

M. Charles Robin resumed the result of his researches on the origin, development, and completion of the dorsal cord, called the cord of Owen.

The Academy proceeded to the election of a Correspondent for the Geometrical Section in place of M. Rieman. The choice almost unanimously fell upon M. Plucker, of Bonn, the well-known Professor. He received the Copley Medal of the Royal Society for 1866, and many honours which we cannot now enumerate.

M. Salmon, of Dublin, author of "Lessons in High Algebra," &c., obtained the vote left by M. Plucker.

The Academy then proceeded to the election of an Anatomical and Zoological Correspondent. M. Siebold, brother of the Japanese traveller, was elected by a large majority.

M. Balard presented an ice-making machine, made by M. E. Carré, brother of the well-known inventor of the ammonia one. Its action depends on the absorption of vapour of water by sulphuric acid, and the congelation is most rapid, as soon as the vacuum is produced.

M. Regnault, in the name of M. Soret, of Geneva, communicated a new note on the determination of the density of ozone. Experiments of absorption lead to the conclusion that the density of ozone is one and a half times that of oxygen. He applied Graham's law of diffusion—viz., that the diffusion takes place in the inverse proportion of the square of the density. He then diffused two mixtures—one of oxygen and chlorine, the other of oxygen and ozone. Thus compared, the density of ozone to that of chlorine or oxygen was found to be 1 : 5.

May 13, 1867.

(From our Special Correspondent.)

Dr. Nelaton begged the Academy to inscribe his name among the list of candidates for the vacant chair in the medical and surgical section, by the death of M. Jobert de Lamballe. Is he not rather late in the field—when, during all his life, he was no more interested in the Academy than if it never existed; and when he never made his appearance but once, to read a work made in collaboration with a young recruit? He must have depended upon the little respect paid by the Academy, sometimes, to itself, to offer himself as a candidate in such an unprepared manner. For my part, I shall not give my vote to M. Nelaton, first-rate surgeon though he be among us.

M. Boussingault communicated a new series of researches relative to the deleterious influence exerted by the vapour of mercury on the vitality of plants. He has repeated and modified the very curious experiments made by some Dutch *savants* in 1797. They placed under a bell glass a plant, with a small vessel containing mercury, and they found that, at the end of a few days, or even a few hours, the leaves of the plant were spotted and blackened, and that it ultimately perished. But when they fixed a small piece of sulphur on the inside surface of the bell glass, the deleterious action of the mercury ceased, and the plant remained healthy. It was not difficult for M. Boussingault to assure himself, by a series of careful observations, that the mercurial vapours had a sort of elective affinity for the sulphur—that sulphuret of mercury was formed, which is inoffensive. M. Boussingault has varied his experiments relative to the action of vapours on plants and the precious metals, silver and gold. He has measured their tension, and appreciated their action on the colours and weight, &c. M. Regnault thinks that the best reagent against the vapours of mercury is an iodised daguerreotype plate ready to be coated and exposed to the light. M. Boussingault maintains that the sensibility of the plates is nothing as compared with that of plants.

M. Pietie, of Geneva, was then elected as member of the Section of Anatomy and Physiology, by forty-two votes against one, given to M. Sace, of Neufchatel.

M. Becquerel, the elder, communicated his series of experiments on the influence of the capillary action of surfaces upon decomposition and chemical combination. He takes a tube with two branches reversed, and makes in it a fissure, the width of which is infinitely small. He pours therein a solution of nitrate of copper, and has found that no liquid passes by the fissure; but when placed in a vessel containing liquid protosulphuret of sodium, an electrical action takes place, and decomposition and recomposition ensue, manifested by the crystals which appear on both sides of the fissure. M. Becquerel has demonstrated the new and curious phenomenon that the capillarity of the fissure has a real influence on the nature of the products of the decomposition; that the salts or the crystallisations are not always those indicated by theory; that the double decomposition often goes to the extent of reduction of the metal.

M. Elie de Beaumont presented, in the name of M. Civiale, an immense collection of urinary calculi, arranged according to their form and structure, composition, &c. He read a long note, giving details of each group.

M. Ch. Sainte-Claire Deville recounted his studies "*On the Periodical Variations of Temperature.*"

The author has established, in one of his former memoirs, that there exists a certain depending connexion in the movement of the mean temperature of four days, placed on the ecliptic at an angle of 90° one from the other, for the four months, opposed two by two, of February, May, August, and November, which contain the critical days, known by the ancients under the name of the *three saints of ice* (May 11, 12, 13), and the *summer of Saint Martin* (November 11). In this new work he shows that the fact is general, and that this connexion or mutual dependence of the four opposite days exists during the whole of the year; whether we take into consideration a considerable cycle—110 years at Berlin, 90 years at Vienna, 50 at London, 40 at Prague and Edinburgh, 30 at Brussels, 24 at Toulouse, 21 at Paris — or that we take in this point of view an isolated year (1864) on several European stations.

The former, depending upon the same data, establishes, in fine, that this connexion is evident also when we combine twelve by twelve the days separated one from the other by 30° of the ecliptic.

The latter phenomenon constitutes the *meteorological month*, as the *season* was established by the consideration of the quadruple days.

M. Marié-Davy presented his ninth memoir "*On the Mechanical Theory of Electricity*." The *résumé* that we shall shortly publish is a sort of synthesis of the principal phenomena of nature. These are his conclusions:—In a ray of light the vibrations are not in the direction of the wave. In a hot substance, in which μ varies symmetrically all round each material centre, the vibrations take place generally in the same manner on the three axes. In the circuit in activity the electric vibration takes place in the direction of the propagation of the current, and the *vis viva* set at liberty by the chemical action passes along that channel. But light, heat, and electricity have the same *vis viva*, having the same mechanical equivalent.

In this hypothesis of the vibrations, the positive electricity is ether condensed in excess; negative electricity is ether in deficiency. It is a long time ago that we sustained this theory, on which we made a memoir presented to the Academy in 1845. Baron Sequier, in the name of M. Stamm, of Milan, communicated the plan of an association between a horse and a steam motor, inspired by a lecture that he gave last year before the Academy, and in which he drew such a clear comparison between inanimate motors and animate ones working by their own will. M. Stamm, in his vehicle with a steam engine commanded by a horse, has so disposed it that the horse gears the machinery when in motion, and ungears it when he stops; so that he arranges the valves in a manner that, when backing, the steam is reversed, and the intelligence which the machinery wants is supplied by the horse.

May 20, 1867.

M. **Delaunay** presented, in the name of M. Camille Flammarion, a note on a change remarked on the surface of the moon in the crater of Linnæus. It is well known that this crater has recently been subjected to an essential modification. The attention of astronomers having been called to this fact by M. Jules Schmidt, of Athens, M. Flammarion chose the moment when the sun rises at the meridian of Linnæus to study this spot. The sun, being only yet elevated a few degrees above the horizon of the crater in question, lit it up very obliquely. The slightest irregularities in the conformation of the surface were most distinctly visible. An attentive observer would remark at once that Linnæus is no more a crater; there is no exterior shadow, no shade in the centre. In its place there is only a cloudy, white, circular spot, or rather a white stain on the ground. Far from being elevated as a crater, it has a greenish color, like the Sea of Serenity, and seems to be neither in relief nor sunken, but resembles a lake of a lighter colour than the neighbouring plain.

This crater has therefore descended to the level of the plain—fallen in—or else the plain has been raised to about the level of the crater. The interior appears also filled up, for no shadow is distinguishable, whilst smaller craters, such as A and B of Bessel, A and B of Linnæus, and those in the neighbourhood of Posidonius, show the dark shadow very perceptibly. If Linnæus had this aspect at the time when Beer and Maedler laid down their selenographic map, it would have been impossible to have indicated it as a crater. In the map constructed eight years ago by Lecouturier the height is not marked. It appears that it was very deep, ten kilometres in diameter, and that it served as a fixed point for Lorkmann and Maedler.

On May 11, the sun being more elevated, Linnæus presented the same aspect as on the evening before. The evening of the 12th was rainy; the 13th the atmosphere, being very pure, permitted the author to distinguish in the Sea of Serenity a multitude of small disseminated craters. The plain was brilliant, and Linnæus had the same relative brightness.

M. Chacornac, who observed the same things at Lyons, arrived at similar conclusions. Father Secchi, of Rome, has already presented to the Academy his own observations. It is, then, proved for a certainty that a movement has recently taken place in this region of the lunar world. The magnifying power used was 230 to 300 times.

Baron von Liebig read a note "*On an Alimentary Preparation for Replacing Human Milk for Children.*" Human milk of a person in good health contains, per cent., caseine, 3·1; sugar of milk, 4·3; butter, 3·1. Baron von Liebig concluded therefrom that woman's milk contains:—Blood-forming principles, 1 part; heat-producing principles 3·8 parts. By mixing flour and milk in certain proportions, it is easy to compose a food in which the two nutritive principles are in the same proportion as in human milk—viz., 1 to 3·8. Cows' milk contains, on an average, 4 per cent. of caseine, 4·5 of lactose, 2·5 of butter. If we take, then, 10 parts of milk, 1 part of wheat flour, and 1 part of ground malt, we have a mixture satisfying all the necessary conditions. For preparing this the author recommends the following method:—A mixture is made of 15 grammes of wheaten flour, 15 grammes of ground malt, and 6 grammes of bicarbonate of potash; 30 grammes of water and 150 grammes of milk are then added. The whole is then heated and continually stirred until the mixture begins to thicken. It is then taken off the fire and stirred all the while. After five minutes it is boiled, and then strained through a wire or hair sieve. The ground malt necessary for this preparation is easily furnished by barley malt, obtained at any brewery. It can be ground in a common coffee-grinder, and then passed through a sieve. If this preparation is well made, it is as sweet as the natural milk; it is fluid enough, and keeps for twenty-four hours. In Germany the use of this food is very extensive, and its nutritive qualities are found to be excellent. It has a slight taste of flour or malt, to which children get accustomed—in fact, they soon prefer it to any other food.

M. Charles Robin presented on the part of the family of M. Godard, a work of this doctor entitled "Medical and Scientific Observations made in Egypt and Palestine." This *savant*, who died at Jaffa of a liver complaint contracted at Jerusalem, where he was studying leprosy, founded an annual prize of 1000 fr. already twice awarded by the Academy of Sciences.

M. Paul Thenard announced that M. Michel Perret by an ingenious process, the result of theory and practical studies, had made a great improvement in the art of making wine. He avoids at the same time acetification, and obtains better colour and more spirit.

This paper was followed by one on chemical researches on the water found in a bronze vase at Pompeii. On the 29th of March last, while making some excavations in a house at Pompeii, a bronze cooking-pot was found on an iron tripod. A cover, also of bronze, fitted exactly upon the top of the vase, so that water falling upon it could not penetrate into the interior. On the ground were found three handles, also of bronze, two of which belonged to the vase, and the other, formed of two dolphins, belonged to the cover. They had originally been soldered to the vase and cover. The vessel was found full of water. The diameter of the vase was 15 centimetres, and it was 20 centimetres high. The water was perfectly limpid, and was hardly rendered turbid by a prolonged ebullition with a feeble alkaline reaction. At the temperature of 20° C. its sp. gr. is 1·001, about that of distilled water. The quantity of fixed matters left by evaporation was 1·032 gr. per litre. The gases disengaged by ebullition consisted of air and carbonic acid. Lime and magnesia were found in it; also phosphates in small quantity; also some traces of sulphates, and even silica and iron. There was not the slightest trace of copper.

NOTICES OF BOOKS.

Discorso di Apertura del secondo anno della Facoltà di Chimica. Letto dal Fondatore, Prof. Carlo Cassola. Napoli. 1867.

The address of the President at the opening of the second year of a chemical college could hardly, our readers might say, contain much novelty, or any details which they could not note down beforehand. Of course he would give a general glance at the position of chemistry, the progress of discovery, and the prospects of the institution where the science was cultivated and taught. The above address does nothing of the kind. Italy has been too long under despotic rule to have retained her old courage for scientific inquiry, and now that she has happily reconquered liberty, she is only beginning to see the necessity of cultivating science as one of the most urgent means of cultivating the sources of national wealth. Hence this address is most curious and suggestive; its elementary character, its all but juvenile pretensions, have something of encouragement in them; so that in laying the substance of this discourse before our readers we are sure they will join us in an expression of sympathy for young Italy, and in the hope that her intelligent sons may see the importance of the new institution and encourage it in the only way that can lead to durable success—namely, by earnest study and hard laboratory practice.

The address begins by referring to the backward condition of Italy consequent on political misrule, and the paramount duty of every Italian to acquire political independence for his country. That desirable event having been accomplished, the next struggle was the endeavour to bring up their country to the level of more civilized nations. How was this to be done? Not by relying on the Government, but by their own exertions, in constructing railroads and other means of communication, developing the riches of the soil, encouraging industry; taking, in short, as their model, the Anglo-Saxon race—America and England.

The President goes on to show how false is the system of national prosperity that produces little and consumes much, exports nothing, imports everything; that produces few or no results of public education, while the population is uneasy and miserable. Trade and commerce are depressed, and those who should foster them are discouraged, because the system pursued is a false one. What is wanted for Italy is the development of her internal riches, and the knowledge how to apply science to her everyday wants.

"Banished from my native country by the political events of 1848-9, I have laboured and studied in different parts of Europe, in Asia, and the two Americas, to understand the secret of national prosperity. I returned to Italy, and excited my countrymen to follow the example of the stranger. I was laughed at! By good fortune I have had the privilege of founding this centre of instruction and research, although opposed by the Government and the municipality. The chief object of this Faculty of Chemistry is to instruct Italians in analytical chemistry and its application to industry, and then to distribute the men so educated over the country. Another object is to bring the scientific Italian out of his retirement and make his pulse beat in unison with that of his fellow-citizens—to win him, in fact, to the side of national industry, and make him more esteemed in his native land."

Italy is beginning to feel the importance of the call. Various municipal and other bodies have sent minerals to be examined, and the Faculty is already in a condition to declare that Italy has undeveloped wealth in every kind of industry. Search has been made for coal, that great motive force, and in spite of the opposition of Government and of the municipality of Naples (expressions that occur more than once, and which we confess we do not understand), the province of Naples can supply that great need.

He then goes on to show how to make the exports and imports more nearly balance each other. The imports in Naples and the surrounding districts amounted to 200,000,000 lire per annum,[*] and the exports to only 36,000,000. He proposes to work the metallic ores of the country by means of companies, to extend railroads, &c.

The School of Chemistry, the only one in Italy (!), was founded in 1866, and has already acquired much precise knowledge as to the natural wealth of the country. It is strongly recommended that the youth of Italy be educated in the theory and practice of the sciences; in facts and observations, and not in dry themes. The Faculty has already supplied two Lyceums with the chemical apparatus required for instruction, and the University has also sought its aid. It has sent several chemists to different parts of Italy, and it carries on a wide correspondence with persons who require information on scientific subjects.

The analyses performed by the Faculty had been liberally paid for with one exception, and that was on the part of the Government. One of the departments sent a liquid to the Faculty to know if it were adulterated, and on returning the analysis the fee demanded was refused, on the ground that the Faculty was a public department, although all its documents bear the words "*iniziativa privata.*" It is stated as one of the worst features of modern Italy that it grudges the scientific man his fee, while the number of useless officials that devour the public wealth is legion.

An exhibition of national products would be useless until there is a larger number of producers. It is to be deplored that many Italians are gratified with an honourable mention and a poor medal, while the maccaronis, once especially a native product, are now made of Russian flour, and are eaten with a French fork off an English plate.

It appears that the King had offered the President a house and 400 lire per annum if he would establish the Faculty at Turin, while both house and endowment were refused in Naples. "Such," he says, "is the Government patronage of science in Italy, at a time when the Prussian Government was expending 500,000 lire on the building alone of a laboratory of instruction in Berlin." He calls upon the people to get out of the leading-strings of the Government, and to think and act for themselves, although, somewhat contradictory to this advice, he suggests that a law be passed requiring every commune to set aside an annual sum for the purposes of scientific inquiry.

There are other suggestions, sensible enough in their way, but remarkable to us only for being so obvious. On the whole, we are gratified with the manly spirit of the address, in spite of a little too much self-assertion and recrimination, and think it creditable to the Government that young Italy can enjoy such freedom of speech. The very elementary nature of the address, and of the proposed remedies, show how degraded poor Italy had become under long years of political misrule. That there should be only one establishment in Italy capable of conducting a chemical analysis, and that establishment a private one, from which a University is taking lessons, is indeed a commentary on the oft-repeated proposition that when a nation loses its political freedom it must part also, sooner or later, with its science, its literature, and its art. It cannot become enslaved in body without being also enslaved in mind.

Chemical Notes for the Lecture Room. By Dr. Wood, F.C.S. London: W & H. Warr & Co., Featherstone Buildings, Holborn. 1867.

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[*] The lire is worth 9½d.

For the matriculation examination of the University of London, an examination that has acquired of late years very considerable importance, a knowledge of chemistry is insisted upon quite as fully as a similar knowledge of any other branch of education. If a candidate be rejected for not gaining the minimum number of marks in any one subject, he has to pass in all the others again at a future examination. Last year 641 candidates presented themselves, and of these 334 were rejected; in 1865, 397 out of 616. An ignorance of chemistry is a very fertile cause of these rejections, and candidates generally complain of this subject as being one of "the stiffest." Very few fail, it seems, in mechanical philosophy. This is accounted for by the existence of a book, recognised as being the one necessary to work up thoroughly, and almost universally employed in the latter subject; while in chemistry the beginner, in an absolute sense, confuses himself by a mass of details, without the corresponding impression from seeing lecture experiments. Dr. Wood has endeavoured to prepare a short text-book on the subjects required, to answer the same purpose that Newth's "Mechanical Philosophy" now serves in another subject. We think that he has succeeded in a difficult task, but those who read the book will require to know it well. We need not talk of Dr. Wood's knowledge as a chemist, but we may mention that as a teacher he is widely known, and few chemists have more knowledge of what is required by candidates for the before-mentioned examination, and for whom the book is almost specially framed.

It is a capital guide-book for lecture experiments, although, as the author remarks, it is in no wise intended "to supersede the excellent works of Miller, Roscoe, and others"—we presume that he means for this particular examination. It will form, as he remarks, a book of easy reference. As an additional recommendation, the wording of expressions and symbols are "the very latest out."

Guide to Croft Spa. Third Edition. Darlington: J. & J. Burney, *Telegraph* Office, High Row. 1866.

In the Chemical News, September 14, 1861, a detailed account was given of the chemical properties of the waters of this spa. It will be seen from that notice that three of the springs there contain more sulphuretted hydrogen than any other mineral springs in England. We are glad that the prosperity of Croft Village has called for a third edition of this very complete guide to its spa.

On the Poisons of the Spreading Diseases. By B. W. Richardson, M.A., M.D., F.R.S. London: John Churchill & Sons, New Burlington Street. 1867.

This reprint of the famous lecture delivered at the Leamington Congress last year, will give some satisfaction, and almost as much disappointment. The satisfaction will be derivable from the many forcible truths and collection of facts; the disappointment caused by their admixture with so much that is mere statement, sometimes almost dogmatic.

Every reader of the book will find this to be a difficulty, for it will be necessary for him to winnow, we will not say the grain from the chaff, but the more healthy corn from the other grains mixed with it.

Although opposed from principle to the practice of ladies attending sanitary congresses and interesting themselves in the details of sewage, we find from the present case that fewer evils than we supposed were without an attending good; for the reflection that the Congress would be of a miscellaneous character, "at which perhaps ladies might be present as well as gentlemen," has been the cause of the selection of the subject of this paper—a paper we honestly think of very great value in much of its substance.

The form that we should like to see this valuable matter assume, would be that of detailed experiments given, and after this the author's deductions from them. The one would remain as valuable staple matter of reference; as regards the latter, a verdict might be given by every reader. There is no doubt that Dr. Richardson has found entirely satisfactory evidence of everything that he asserts; but every thoughtful reader must decline to record a similar conviction for himself in the absence of the evidence.

We have fifteen diseases given arbitrarily as those produced by *organic* poisons, organic being used in a sense of its own. Thus strychnine would not be an organic poison; it is not asserted, however, that it is an inorganic one. In this list small-pox appears as produced by organic poison, boil and carbuncle with infections ophthalmia (*Ophthal. Egyptorum?*), but chicken-pox, cow-pox, dysentery are excluded. Typhus appears in the list, and very strong evidence has been tendered of the connexion of dysentery with typhus, and cases, with no apparent fallacy, have been recorded of epidemic dysentery brought from India on board ship causing a typhus outbreak in port by direct infection. Such facts ought not to be ignored in a critical examination of which every step is attended with difficulty. And thus on almost every page the eye is arrested by some very knotty question comfortably disposed of. This renders a detailed criticism a work of almost indefinite length.

To choose a few sentences at random. "Each poison has a specific property always bringing out the same disease through countless ages. All are destroyed by oxidising agents. . . . Exposure to nitrous acid has the same kind of effect; exposure to sulphurous acid likewise produces destruction." "Common sulphur preserves them very well; the poisons of hospital fever I have been able to preserve for months by this means." (Query: What means?) "The poisons will all dry solid. There is no exception to this rule." (Query: Who has examined this?) "Sulphur, creasote, and arsenic hold these organic poisons in perfect steadiness; they preserve their active properties."

All these quotations require support from experiments, original or otherwise, for any judgment to be formed. Again, "There is reason to believe that in the decomposition of sewage water cholera poison may be developed." These quotations are all from two or three consecutive pages, and are accompanied by many others of the kind.

Is our demand for a detail of experiments then unjustifiable? If these are not forthcoming, England must yield the palm to America; for Dr. Richardson quotes some experiments that are convincing.

"During the great American struggle, Dr. Salisbury observed that a large number of men rose one morning with symptoms of measles. . . . The men attributed their illness to the straw upon which they lay. . . . Dr. Salisbury removed from the straw certain portions of fungus; he had the courage to inoculate himself with the fungus, and he thus produced measles. He then inoculated *his own wife*, and then *a mother and four children*, then a mother and two children, and produced in all the same disease." We must leave the reader to judge of the feelings of the ladies present at this great sanitary congress.

Chemical Technology; or, Chemistry in its Application to the Arts and Manufactures. By Thos. Richardson, M.A., Ph.D., F.R.S.; and H. Watts, B.A., F.R.S. Second Edition. Vol. I. Part V. London: H. Baillière, 219 Regent Street. 1867.

This second edition of a well-known volume has given an opportunity for several important additions, with an announcement of the parts necessary for the completion of the series.

The work itself has already acquired imposing size, and the complete set forms in itself a library for the manufacturing chemist, who, indeed, could do without it altogether with difficulty, and would incompletely replace it as his book of reference by a great mass of materials—such a mass, in fact, as must of necessity be consulted by competent chemists for the successful completion of a scheme like this.

The first volume is formed of five parts, each of octavo size, with from 800 to 900 pages. Parts I. and II. contain Fuel and its Applications; Part III., Acids, Alkalies, and Salts; Part IV., Aluminium, Sodium, Phosphorus, Lucifer

Matches, Borax, Artificial Mineral Waters, Gunpowder, Guncotton, Fireworks, &c; and the present volume, Part V., Prussiate of Potash, Oxalic Acid, Tartaric Acid, Tartrates of Potash, and Citric Acid, with appendices presently to be noticed. We have thus four handsome books forming Volume I. Volume II. contains Glass, Alum, Potteries, Cements, Gypsum, &c., &c. Volume III., Food generally, Bread, Cheese, Tea, Coffee, Tobacco, Milk, Sugar.

These three volumes are all that have at present appeared; they form six octavo books of large size, illustrated with woodcuts, plates, engravings, and, in the last volume, coloured plates.

The book itself is rich in passages we should like to quote, were it not for their length. There is, however, one value connected with a work of this kind which ought to be pointed out. Most young chemists, after passing through the routine course of their scientific education, feel somewhat at a loss to know in what direction to turn their experimental energies, so as to obtain a prospect of some return for their trouble. As we stated in these pages a few weeks ago, it often happens that a difficulty is found in carrying out some manufacture which could easily be overcome, by a short investigation, by a competent chemist; and, indeed, in many cases a difficulty which appears insuperable to the manufacturer, would prove a mere *bagatelle* to the chemist. The difficulty, however, is for the young experimentalist to know what are the problems in industrial chemistry which require solving. In this respect the present series of Chemistry Applied to the Arts and Manufactures constitutes a very mine of wealth. As an illustration, we learn that with regard to the determination of the commercial value of tartars, it is a point of the utmost importance that some method should be devised for ascertaining correctly the quantity of tartaric acid present, which is now merely deduced from the quantity of potash found.

That this method is by no means an accurate one may be seen from the three following analyses of the same sample, viz:—

Tartaric acid present as bitartrate of potash,	70·15	69·10	72·25
Tartaric acid present as tartrate of lime,	3·35	3·30	3·75
Total crystallised tartaric acid, per cent.,	73·50	72·40	76·00

This is so urgent a matter, that one of the largest and most distinguished manufacturers of tartaric acid has expressed his willingness to give 100*l*. as a reward to any one who would discover a satisfactory method of determining, directly, the quantity of crystallisable tartaric acid present in tartars, in a sufficiently ready manner to be applicable to commercial analysis. This is only one of the many chemical problems to be met with throughout the work, the solution of which would be attended with considerable profit.

The following are announced as in preparation to complete the work:—Vol. IV. devoted to Liquid Food, Ales, Wines, Spirits, &c.; V. and VI. to Textile Manufactures, Cotton, Wool, Silk, &c.; VII. to the Manufacture of Leather, Gutta Percha, &c.; VIII. to Manufacture of Paper, &c.; IX., X., XI., to Metallurgy and Chemistry of the Metals; XII. to the Manufacture of Colours, Oils, and Varnishes. These will complete an elaborate and exhaustive work, which English chemists will mention with pride, as a proof of the repute in which chemistry is beginning to be held in our country. It seems that no space will be given separately to the consideration of the preparation of medicinal substances in a pure form, such as morphia, chloroform, &c.

As regards this present edition of the volume under notice, no one will doubt the wisdom of the retention of the older notation of the former one, which is still most applicable to the wants of the manufacturer, who cannot be expected to be well acquainted with newer views held in 1867. The next edition will be in time for those of a younger generation now pupils at various laboratories.

Of the 900 pages of this part, upwards of 700 are devoted to the various appendices. Appendix A consists of additions to the various chapters of Vol. I., parts iii., iv., and v., first edition, and brings the report up to the present time; nearly 300 pages are devoted to this part of the work. Appendix B contains abstracts of specifications of patent inventions relating to materials and processes described in those parts; Appendix C (of the greatest value), tables connected with processes described in the former parts; Appendix D, documents relating to the Patent Laws, appearing at a very opportune time. A casual glance will show a great part of the value of all these appendices, but Appendix C is so important, and would be so useful for constant reference, that we would urge a publication of it separately; it would form a volume more easy for constant reference than in its present form. Lastly, we think that the whole of the additions to the first edition, which form the bulk of the book, ought with justice to be prepared also in a separate form. Possessors of the first edition should not be expected to buy the whole volume, part of which they already possess. The same reason should have weight with the publishers, when the time comes for newer editions from time to time. If this were promised, the different parts would have a lasting value even to a greater extent than at present; besides this, the latest information could be more frequently supplied, to supply a more extensive demand. The plates, five in number, are excellent; two of them consist of tables and scales, the other three of apparatus, &c. It would have been better if the paper for these had been of a stronger kind, or mounted on some material to make them more durable for frequent reference. We commend a consideration of this point to Messrs. Richardson and Watts for discussion in the forthcoming volumes relating to textile manufactures and the manufacture of paper.

"Chemical Technology," clearly and accurately written, neatly sent to press, and very moderate in price, may justly be a source of pride to the authors, the publisher, and the profession generally.

CONTEMPORARY SCIENTIFIC PRESS.

[Under this heading it is intended to give the titles of all the chemical papers which are published in the principal scientific periodicals of the Continent. Articles which are merely reprints or abstracts of papers already noticed will be omitted. Abstracts of the more important papers here announced will appear in future numbers of the Chemical News.]

Bulletin de la Société d'Encouragement. January, 1867.

G. de Claubry: "*Report on Bon's Imitations of Precious Stones.*"—"*Blasting of a Cast Iron Roll by Nitro-glycerine at Rothehutte, Upper Hartz*"—Putsch and Ziebarth: "*Improved Furnace for Melting Enamel.*"—"*A Method of Gilding Glass.*"—Weber: "*An Account of an Explosion of Picrate of Soda.*"—"*On the Preparation of Aniline Colours in Powder for Printing Carpets, and for Lithography.*"—"*On Colouring Brass.*"—Knaffl "*On Colouring Zinc and Brass Black.*"—Jünemann: "*A new quick-drying Fire and Waterproof Cement.*"—Kletzinsky: "*Solution for rendering Fabrics Uninflammable.*"

Le Technologiste. March, 1867.

Soames: "*On a Process for Purifying Paraffin.*"—E. Jacobsen "*On the Use of Rosaniline for Detecting the presence of Free Fatty Acids.*"—"*On the Relative Indications of different Optical Saccharometers.*"—V. Kletzinsky: "*Smechochromasy, a Process of Colouring by means of Coloured Soaps.*"

Journal für praktische Chemie. No. 22. 1866.

A. Muller: "*A Memoir on Chromometrical Analysis.*"—H. Vohl "*On the Action of Fuming Nitric Acid on the Chlorinated Derivatives of Benzol.*"

Nos. 23-24, 1866.—O. E. Erdmann "*On the Formation of Aniline Colours from Protein Substances.*"—A. C. Oudemans, Jun.: "*Chemical Researches on certain East Indian Fats.*"—E. Sell and E. Lippmann "*On the Action of Mercury Ethyl *on Monobromoacetate of Ethyl.*"—F. Rochleder: "*Contributions to the Knowledge of Luteoline.*"—H. Ritthausen: "*Researches on some Constituents of Rye;*" "*On a new Compound called by the Discoverer Glutaminic Acid;*" "*On the Constituents of the Gluten obtained from Wheat.*"—A. Claus "*On the Action of Sodium on Oil of Bitter Almonds.*"—W. Körner "*On Brominated Crotonic Acid.*"—O. Hesse "*On Carbo-usnic Acid* (*Carbo-usninsäure*)."—K. von Hauer "*On a Double Salt of Seleniate of Cadmium and Potash.*"

March 1, 1867.—F. Reindel "*On some Basic Salts of Copper;*" "*On some Double Cyanurates.*"—Rammelsberg "*On Phosphorous Acid and its Salts.*"—P. T. Cleve "*On some Brominated and Iodated Ammoniacal Platinum Compounds.*"—L. Rinman "*On the Presence of Nitrogen in Steel and Pig Iron, and on the Condition of Carbon in Hardened and Unhardened Steel.*"—R. Weber "*On the Formation of Protoxide of Nitrogen by the Action of Sulphurous Acid on Hyponitrous and Nitric Acids.*"—A Bauer "*On the Action of Chlorine on Amylene.*"—Baeyer "*On the Reduction of Aromatic Compounds by means of Zinc.*"—F. Rochleder "*On Quercitrine.*"—W. Stein "*On the Elementary Analysis of Hygroscopic Substances.*"—C. W. Paykall "*On the Analysis of some Swedish Minerals.*"

Poggendorff's Annalen. February 11, 1867.

C. G. Jungk: "*Remarks on the Diffusion of Steam through Dry Atmospheric Air and on some other Hygroscopic Phenomena.*"—E. Zettnow: "*Researches on Wolfram and its Compounds.*"—P. Kremers "*On the relative Volume of Compounds of the First Degree.*"—J. Muller "*On the Focal Length of Lenses;*" "*On the Fluorescence Spectrum of the Electric Light.*"—W. Holtz "*On the Production of the Electric Spark in Glass, with especial Reference to the Electrical Machine;*" "*On the Theory of the Construction of Electrical Induction Machines.*"—A. Brezina "*On the Use of the Stauroscope.*"—F. Lindig: "*A Reply to H. Schiff's Paper on Supersaturated Solutions.*"—C. Rammelsberg "*On the Composition of Franklinite.*"—Arndt "*On Nägeli and Schwendener's Method of Calculating the Magnifying Power of Microscopes.*"—W. Schmidt "*On a new Metallic Thermometer.*"

Journal des Fabricants de Papier. February 15, 1867.

E. Bourdilliat "*On Testing the Chemical Products used in Paper-Making* (*Continuation*)."—J. Mauslay "*On the Preparation of Chlorine.*"

Annales de Chimie et de Physique. March, 1867.

F. P. Le Roux "*On the Relative Position of the Planes of Vibration of Incident, Reflected, and Refracted Rays in Isotropic Media.*"—Bontemps: "*Remarks upon J. Pelouze's Memoir on Glass.*"—F. P. Le Roux "*On the Affects of Annealing on the Colour of Glass.*"

Archives des Sciences. February 25. 1867.

G. De Saporta "*On the Temperature of Geological Periods as deduced from an Examination of Fossil Plants.*"—V. Fatio "*On the Presence of Air in the Bodies of Birds.*"—H. Dor "*On Max Schulze's Researches on the Yellow Spot of the Retina, and its Influence on Normal Vision and on Colour Blindness.*"—C. Muller: "*Researches on the Position of the Alkaloids in Cinchona Bark.*"

Comptes Rendus. April 2.

M. E. Chevreul: "*Note on two Works on Alchemy attributed to Artefius and Alphonso X.*"—Daubrée: "*A new Method of Studying the Structure of Meteorites.*"—H. Dufresne: "*A new Method of Gilding and Silvering by Amalgamation, without Danger to the Workmen.*"—J. M. Crafts "*On the Arsenic Ethers.*"—P. Hautefeuille "*On some Inverse Reactions.*"—L. Joulin "*On the Potash and Soda of Stassfurt.*"—Berthelot "*On a Method of Reducing and Saturating Organic Compounds with Hydrogen.*"—A. Civiale "*On the Application of Photography to Physical Geography and Geology.*"—Jolyet "*On the Action of Sulphate of Quinine on Frogs.*"—J. Rambosson "*On the Influence of Food on the Nervous System.*"—C. Hofmann: "*Experiments on Cutaneous Absorption.*"—J. Smyth: "*On the Presence of Ozone in the Atmosphere.*"

Monatsbericht der königlich-Preussischen Akademie. November, 1866.

A. W. Hofmann "*On the Transformation of the Aromatic Monamines into Acids containing a larger Proportion of Carbon.*"—A. Baeyer "*On the Constitution of Mellitic Acid.*"—E. O. Erdmann "*On the Origin of the Blood-red Colour which occasionally appears on articles of Food.*"

Bulletin de l'Académie de Belgique. February 2.

A. Kekulé: "*Report on Montigny's Memoir on the Correlation of the Refractive and Calorific Powers of certain Substances.*"—Plateau: "*Report on the same Memoir.*"

NOTICES OF PATENTS.

Communicated by Mr. Vaughan, F.C.S., Patent Agent, 54, Chancery Lane, W.C.

GRANTS OF PROVISIONAL PROTECTION FOR SIX MONTHS.

218. E. H. C. Monckton, Summer Hill, Birmingham, "Improvements in the manufacture of butter."—Petition recorded January 26, 1867.

952. W. E. Newton, Chancery Lane, "An improved process for manufacturing ice and for other refrigerating purposes." A communication from T. S. C. Lowe, New York, U.S.A.—March 30, 1867.

979. J. Storey, and W. E. Bickerdike, Lancaster, and W. V. Wilson, Jubilee Street, Mile End, Middlesex, "A new method of bronzing metallic and other surfaces."—April 2, 1867.

1044. W. R. Lake, Southampton Buildings, Chancery Lane, "An improved mode of embalming or preserving dead bodies and carcases." A communication from G. W. Scollay, St. Louis, Missouri, U.S.A.—April 6, 1867.

1054. C. F. Claus, Middlesbrough-on-Tees, Yorkshire, "Improvements in the manufacture of chlorine."—April 8, 1867.

1064. J. H. Player, Birmingham, "Improvements in the manufacture of phosphorus and in economising residual products of the said manufacture."—April 10, 1867.

659. W. R. Lake, Southampton Buildings, Chancery Lane, "An improved mode of coating paper and other materials with fluid substances, solutions and compounds for photographic and other purposes." A communication from J. C. Crossman, Boston, Mass., U.S.A.—Petition recorded March 8, 1867.

1042. W. Henderson, Glasgow, "Improvements in oxidising minerals, ores, and metals; in reducing oxides of metals; in separating certain metals from each other; in kilns, furnaces, or other apparatus for these purposes; and in the treatment of the products obtained therefrom."—April 6, 1867.

1087. W. H. Dawes, West Bromwich, Stafford, "An improvement or improvements in the manufacture of iron."

1091. C. Wilmet, Brussels, "An accelerated tanning by means of new processes and apparatus."—April 12, 1867.

1099. J. Aitken, Tottington Higher End, Lancashire, "Certain improvements in apparatus employed in the process of refining sugar."

1107. C. Crockford, Holywell, Flintshire, "Improvement in obtaining useful products from certain materials produced in the process of galvanising or coating iron with zinc."—Petition recorded April 13, 1867.

1118. The Rev. J. Oakden, St. Stephen's Parsonage, Congleton, and J. Picking, Dane Row, Buglawton, Congleton, "A new or improved enamel for enamelling metals and stones, to prevent rust, corrosion, and incrustation." April 15, 1867.

1190. J. H. Johnson, Lincoln's Inn Fields, Middlesex, "Improvements in the treatment of peat, and in the manufacture of peat charcoal, and in the machinery or apparatus employed.

therein." A communication from A. Figge, Hanover.—April 24, 1867.

1198. C. E. Brooman, Fleet Street, "A new or improved process of destroying vegetable matters in wools, noils, woollen waste, and rags." A communication from C. Schaller, Bischweiler, France.

1200. C. E. Brooman, Fleet Street, "A new or improved process of preserving meat, fish, and other substances." A communication from F. Cirio, Turin, Italy.—April 25, 1867.

NOTICES TO PROCEED.

3293. F. W. Reeves, Cambridge Terrace, Notting Hill, Middlesex, and J. B. Muschamp, Pembroke Road, Middlesex, "An improved explosive substance."—Petition recorded December 15, 1866.

3226. L. Schad, Warrington, Lancashire, "Improvements in treating aniline colours for dyeing and printing."—December 18, 1866.

3348. S. Parry, Thackeray Street, Liverpool, "An improved composition for the coating of the bottoms of ships and other vessels."—December 20, 1866.

3433. J. Napier, Salisbury, Wiltshire, "Improvements in the preparation of food of a substance to be employed in the place of malt, and for the medication of food for animals."—December 29, 1866.

211. J. J. Lundy, Leith, Mid Lothian, N. B., "Improvements in the treatment of the residual matters resulting from and obtained in the purification and distillation of mineral or hydrocarbon oils, and also in the treatment of coal-tar and various waste or other alkaline and lime substances, for the purpose of utilising the same."—January 26, 1867.

3307. C. E. Brooman, Fleet Street, "Improvements in the preparation and application of certain fatty bodies." A communication from M. P. Javal and E. P. Javal, Paris.—Petition recorded December 15, 1866.

22. W. Knaggs, Euston Grove, Euston Square, Middlesex, "Improvements in apparatus for evaporating and boiling saccharine liquors."—January 3, 1867.

47. W. Way, M.D., Eliot Place, Blackheath, Kent, "Improvements in preparing phosphatic minerals for use as manure." Partly a communication from C. Henwood, Sombrero, West Indies.—January 7, 1867.

250. E. V. L. Ebersburg, Knightsbridge, Middlesex, "A new or improved article of food for infants and invalids." Partly a communication from Baron J. von Liebig, Munich, Bavaria, Germany.—January 30, 1867.

819. J. Greenshields, Glasgow, N.B., "An improved compound or combination of materials to be used for the production of illuminating gas."—March 21, 1867.

935. J. Bird, Seymour Street West, Connaught Square, Middlesex, and J. Bird, Laurence Pountney Hill, London, "Improvements in the manufacture of artificial fuel."—Petition recorded March 29, 1867.

952. W. E. Newton, Chancery Lane, "An improved process for manufacturing ice, and for other refrigerating purposes." A communication from T. S. C. Lowe, New York, U.S.A.—March 30, 1867.

1042. W. Henderson, Glasgow, "Improvements in oxidising minerals, ores, and metals; in reducing oxides of metals; in separating certain metals from each other; in kilns, furnaces, or other apparatus for these purposes; and in the treatment of the products obtained therefrom."—April 6, 1867.

1153. W. Harrison, Wharton Green, Winsford, Cheshire, "An improved method of consuming smoke in furnaces."—April 20, 1867.

2077. *Making Caustic Soda from Common Salt, by the Action of Lead or its Oxide, with After Recovery of the Lead for further Use.* S. ROWBOTHAM, Penkett, near Warrington. August 14, 1866.

THE sodic chloride is either fused with the lead or mixed with plumbic oxide and water until decomposition takes place. Heat may or may not be necessary; the whole is kept moist by steam. The caustic soda is separated by lixiviation. The plumbic chloride left is then exposed to the action of oxygen with heat, or steamed to recover the lead or its oxide. The lead is further to be purified by the action of charcoal.—Patent abandoned.

2095. *Coating and Recovering Metals from Chlorides and other Solutions of Metals.* J. WEBSTER, Birmingham. August 15, 1866.

CHIEFLY applied for using the zinc chloride in the flux used in the pots for galvanising iron. This is boiled in a cast-iron vat; when the temperature reaches 600° Fahr., scraps of tin or wrought iron from other manufactures are added. The vat has a longitudinal partition, which is a grating or perforated plate to allow of free diffusion: in one half the scrap tin or iron is placed, in the other articles that require a coating of zinc or tin. In this also scrap materials must be placed and pressed on the bottom of the vat by another perforated iron plate for the hydrogen gas to escape. Copper, brass, &c., by such a coating of zinc or tin, are protected from atmospheric action.

CORRESPONDENCE.

Cement Cisterns for Water.

To the Editor of the CHEMICAL NEWS.

SIR,—I find among the answers for correspondents in No. 386 of your valuable paper, an excellent suggestion to J. Y. S. Permit me to call your attention to the use of hot coal tar for preventing the contamination of the water by the cement. It is a cheap and effective means of preventing what is complained of, and is not a fancy. Experiments made by Captain de Bordes, of the Netherlands Royal Engineers, and myself, many years ago, have proved that even from hydraulic cement which sets and hardens in a few hours, for days after lime and other salts are dissolved out by pure distilled water in very appreciable quantity. In the kingdom of the Netherlands, in many parts, and especially in portions of the provinces of Zealand, North Holland, and Friesland, no other water than rain-water is in general use for domestic purposes, because, like as at Amsterdam, Flushing, the Helder, and other places, all the water of canals, rivers, &c., is either brackish or decidedly salt. Large cisterns built under ground, in order to prevent foul infiltrations of surface-water, but which have to be laid entirely in strong cement (hydraulic as it is termed), are used to keep the rain-water for the use of barracks and large establishments. These cisterns are often lined with Dutch glazed tiles, fixed in cement, but it has been found cheaper to simply line the cisterns with cement, and after it is dry to give two coatings of hot coal tar. I suggested lately the same to an engineer, who, having applied it to a cistern of his own in his house, found it to answer perfectly. For a few days the water had a slight tarry taste, which is now entirely gone. I am, &c. DR. ADRIANI.

Drilling Glass.

To the Editor of the CHEMICAL NEWS.

SIR,—In the CHEMICAL NEWS of April 19 there is a description, by Mr. Spencer, of the old and well-known method for drilling glass by means of a file wetted with oil of turpentine. Some years ago I read in a German periodical of another means for the same purpose—viz., dilute sulphuric acid—and I found it, on trial, to answer much better than the first. Not only, it appears, is the efficacy of the cutting tool more increased by sulphuric acid than by oil of turpentine, but also, strange as it seems, the tools (files, drills, &c.) are far less rapidly destroyed by being used with the acid than with the oil. I also found it stated that, in the engineering establishment of Mr. Pintus, at Berlin, glass castings for pump barrels, &c., were drilled, planed, and bored, just like iron ones, and in the same lathes and machines, by the aid of sulphuric acid. As to drilling, I can fully testify to the efficacy of that method.

Whenever I want, say, a hole in the side of a bottle, I send it, along with some dilute (1 : 5) sulphuric acid, to the blacksmith, who drills in it, with a hand-brace, a hole of ¼-inch diameter. This hole is then widened to the required size by means of a triangular or round file, again wetted with the acid. I also find a great help in the latter when making graduations on litre flasks, &c. There is hardly any smell perceptible during the work, which proves how little the acid acts upon the tools, undoubtedly owing to their being tempered; but each time after use I take the precaution to wash and dry the files at once, and I have so far observed no sensible deterioration in them. Hoping this little hint may be useful to some of your readers, as it has been to me,

I am, &c. G. Lunge, Ph.D.

South Shields, April 22.

Department of Science and Art.

To the Editor of the Chemical News.

Sir,—As you have occasionally admitted into your columns strictures upon the operations of the Department of Science and Art, attacking now its supposed procrastination, now its inconsistency, and now its impolicy, allow me to submit to you the accompanying extract from a printed notice, which will show, at least, the generous intentions and liberality of the Council on Education towards its certificated science teachers, and which will, I hope, prove their sincere wish to foster the spread of an intelligent scientific taste among the people. It will also furnish some reply to those who have lately questioned the justice or desirability of retaining a special class of beneficed teachers; for it is obvious that were there no limitation to the number of claimants, such a bounty as that which the Government here offer—viz., of paying the expenses of each certificated master who shall visit the Paris Exhibition—would become impracticable. I believe a score of other good reasons could be given why teachers should conform to rule and submit themselves to examination before benefiting from the national grant; and so far from advocating the abolition of such tests, I, in common with many others, would rejoice to see the already high standard of qualification raised, as it is quite possible that the Department may design—it is time to grumble after we have seen their next May's questions—but I feel that it is absurd to expect the Department to continue to apply their vast machinery to the examination of sometimes a solitary candidate, as was my own case on a certain day last November. I am not surprised that they should meditate making the season of examination of both masters and pupils identical. That the questions proposed to each will be identical is now at least problematical; but were they the same it cannot be supposed but that such an experienced professor as Dr. Hofmann (even with his 1500 candidates) could perfectly discriminate by a series of judicious questions between the proficiency fitting for a teacher and that to be expected from a student; while the grouping together of both classes of men in the same grades, though it may mortify the vanity of incompetent or idle would-be lecturers, is at the same time an additional spur to such as have the pride to excel, from the fear lest distinguished pupils of other schools should be classified as their equals. I beg to subscribe myself as one who, though previously qualified to claim receipt of grants on result, yet preferred to submit to routine and become, after examination, A Certificated Master.

April 26.

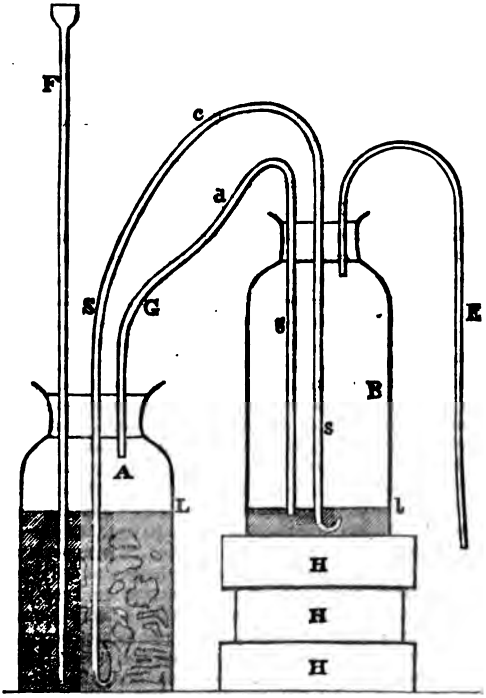
"Their Lordships announce to the certificated masters now engaged in giving instruction in schools of science and art connected with the Department, that they will pay to each such master or mistress visiting the Paris Exhibition, the sum of five pounds in aid of their expenses, and to each an additional sum of two pounds for any report or any useful suggestions which any such teacher may make (in respect to his or her duties or teaching) derived from the study of the Exhibition, such report having first been published in any journal local or otherwise, and afterwards approved by their Lordships. And further, to each of the three best of such reports referring to instruction in science and to each of the three best reports referring to art, my Lords will give respectively the following prizes in addition to the sum above named, namely,—for science, for the best report, twenty pounds; for the second best report, fifteen pounds; and for the third best report, ten pounds, and the same sums respectively to the three best reports for art."

Improved Sulphuretted Hydrogen Apparatus.

To the Editor of the Chemical News.

Sir,—I venture to describe a modification of apparatus for generating sulphuretted hydrogen, which will, I trust, recommend itself to the laboratory on the grounds of compactness and cheapness, combined with effectiveness.

It consists essentially of a generator and a wash-bottle united (in addition to the ordinary connexion) by a siphon, whose legs dip to the bottom of each vessel; it occupies, therefore, the same space as the simplest arrangement, while the production of gas may be rapidly arrested or controlled at pleasure without the escape of noxious effluvia. Its cost of construction need not exceed one shilling, while it affords even greater conveniences than the more costly adaptation described at p. 152 of the *Journal of the Chemical Society*, 1864.



My apparatus consists of two similar wide-mouthed bottles, A and B, whose bungs (or, preferably, whose caoutchouc capsules) are pierced each by three holes. A is the generator, supposed in action; B is the washbottle, which serves also, during inaction, as a reservoir for the exciting acid; F, a tall thistle-headed funnel passing to the bottom of A, by which it is charged, and which serves also as a safety-vent; S, *s*, the siphon limbs, which dip to the bottom of each vessel, and are slightly recurved at the orifices; *c*, *d*, caoutchouc connectors; G, *g*, the transit pipes for the gas as generated, the former terminating at the top, the latter at the bottom of their respective vessels; E, the eduction pipe; H, blocks used to relatively raise or depress each bottle as occasion requires.

Previous to operation, A is filled a quarter full of broken glass; on this are laid lumps of ferric sulphide, which are thus prevented from choking the ends of F and S; acid is poured in through F till A is three-quarters full; the siphon is then charged by nipping the other bend *d*, when the pressure of gas formed in A should be suffered to force the contained liquid over the bend *e* into B, till the mouths of *g* and *s* are covered, and the acid stands at the same level in both bottles, as in the figure L, *l*. B is then raised on the blocks H, and *d* is unnipped, when the gas will commence flowing in its normal direction through G *d g*, and, bubbling through the wash-bottle B, will pass out at the eduction tube E. When it is desired to stop the action, A is raised on the blocks, and B depressed; the former will then be emptied by the siphon into the latter; the reverse will take place, if, with a view of re-exciting the sulphide, the levels be again interchanged.

Your readers' ingenuity will suggest how, by varying the elevations, the flow of gas may be regulated to a nicety, or the charge of acid renewed without opening the bottles or emitting any smell. The eduction tube should, after traversing the solution to be impregnated with H_2S, always finally dip into a bottle of strong liquor ammoniæ, which will absorb any excess of unused gas, thus furnishing a useful laboratory reagent as a by-product.

Should the siphon become inoperative from gas collecting in its bend, which may occur through inadvertently over-emptying either bottle, or from effervescence passing up its limb S, it may be readily recharged as at starting.

Such an arrangement is of course equally applicable to other gases of daily use in the laboratory, for each of which such an inexpensive apparatus might be reserved.

I am, &c., B. W. Gibsone, M.A., B.Sc.

Eaton Square, S.W., April 8.

Extinction of Fires.

To the Editor of the Chemical News.

Sir,—In papers dated Sydney, February 16, I find an account of a patent taken out by Dr. Bland for a method of extinguishing fires in ships' holds and other confined spaces by means of carbonic acid gas. The patentee states that his attention was directed to the subject by the loss of a ship by fire in 1839, and now, after twenty-eight years of, no doubt, laborious experiments, he brings forth the following original process. He places on the keelson of a vessel a number of barrels containing calcareous minerals, and generates carbonic acid gas by the action of dilute acids supplied by tubes from the deck above. The gas finds its exit by numerous holes bored in the upper part of the barrel. The inventor of this brilliant process invited to his establishment at Woolloomooloo, Sydney, a number of gentlemen, members of the legislature, &c., to witness the success of his patent, but the result was in keeping with the former details. His apparatus for confining the combustion to a closed space was so clumsily constructed that sufficient air found access to keep up the combustion in spite of the supply of choke-damp generated from whiting spread on "iron plates," not from a barrel.

It is sincerely to be hoped that we are not to be prohibited by letters patent from employing carbonic acid gas, generated in any off-hand apparatus, to extinguish combustion. This patent seems about as novel as the caustic soda process of Mr. J. Roddy, mentioned in the last volume of the Chemical News.

I am, &c. T. B.

Manchester, April 26.

The Chemical Society.

To the Editor of the Chemical News.

Sir,—Your leading article in the Chemical News of Saturday-last interested me. I have long been expecting to see evil effects arise from the admission of so many persons into the Chemical Society who are manifestly unworthy of that honour. I mean persons who simply pay the fees, and do not promote chemistry either by original communications, or in any other way worthy of the title F.C.S. The following circumstance may perhaps interest you, as it relates to this subject:—A junior partner in a manufacturing firm was admitted into the Chemical Society; on being asked what chemical investigations he had made to entitle him to that honour, he replied that other persons made investigations, besides those who published them, meaning thereby that he had made chemical investigations, but did not publish them. The objects of the Chemical Society are defined to be "the promotion of chemistry, and of those branches of science immediately connected with it, by the reading, discussion, and subsequent publication of original communications." The admission, therefore, into the Society of persons whose practice (as in the above instance) is to monopolise chemical knowledge, and keep it secret for their own personal advantage only, is contrary to the objects for which the Society exists; it is also conferring an honour upon persons who do not adequately deserve it. Further, the admission of persons whose only object is to promote their trades, and the getting of money by them (however worthy those objects are in themselves), is unjust towards those members who, at considerable sacrifice and much self-denial, aid in the extension of chemical knowledge.

I am, &c.,

F.R.S., F.C.S.

Caramel Colours.

To the Editor of the Chemical News.

Sir,—Under the "Notes and Queries" in your valuable paper (No. 387), I happen to find one concerning caramel. Perhaps the following may be of use to your correspondent.

The manufacture of caramel (coffee finings, as it is often termed in London) is kept a secret on this account, that neither coffee-roasters, nor dealers in groceries, nor brewers, may have, or at least are presumed not to have, any in their possession—the Excise prohibiting it. Here in London it is made by roasting sugar of coarse description in cylinders similar to those used for roasting coffee, chicory, and cocoa; this yields a very inferior preparation both for colouring as well as for admixture with coffee. So prepared it contains assamar and other pyrogenetic products which are very bitter. On the Continent apples of inferior description are treated as described, yielding a product superior to that obtained from sugar. Sugar, however, is the only fit material to prepare caramel, and for this purpose the sugar is best heated in capacious roomy vessels made of copper (in Vienna copper lined with silver is preferred), the vessel containing the sugar being placed in an oil bath* containing a thermometer to indicate the temperature. The latter must not be below 410° nor above 428° Fahr. The heating of the sugar is continued as long as aqueous vapours are given off. The crude caramel so obtained is best purified by being placed upon a parchment paper dialyser, which is placed on water. The undecomposed sugar and intermediate compounds are thus got rid of; they dissolve out with facility, and what remains on the filter is, weight for weight, five times as strong in colouring matter as the crude caramel. While the sugar is being exposed to heat it is preferable to stir it with a spatula.

Another mode of obtaining a pure caramel, free from bitter produce (assamar and the like), is to heat the sugar as above, and to treat the powdered caramel with alcohol (pure methylated spirits), to digest it for three to four hours therewith, and repeat this till all bitter taste is gone. An aqueous solution containing 10 per cent. of purified caramel is gummy, and forms a jelly. When a solution of caramel in water is evaporated *in vacuo* (small vacuum pan as used in sugar refineries), it dries up to a black shining mass, freely soluble again in water, hot or cold; but if the solution is evaporated on a waterbath to dryness in contact with air, the whole mass becomes insoluble in water either hot or cold.

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* A mixture of tin and lead is sometimes used, just made so as to remain fluid at from 412° to 430° Fahr.; some bismuth is added.

A very small proportion of caramel gives to a large bulk of water the dark brown tinge known as sepia. An impure but pretty strong solution of crude caramel (*i. e.*, not purified by dialysis or alcohol—hence the term impure for the solution) is sold in London under the name of coffeena in small bottles at 1s. per bottle, to be had in many oil and colour shops in the metropolis; it is used in teaspoonfuls to improve coffee, dispensing with chicory.

I am, &c., A. Adriani.

London, May 7.

P. S.—Treacle is not very manageable to use for the making of caramel. The sugars should be first dried at 212° Fahr. On the Continent dry glucose is sometimes used instead of cane or beetroot sugar for the purpose of making caramel.

Transparency of Red-hot Metals.

To the Editor of the Chemical News.

Sir,—One of the contemporaries, though not a strictly scientific periodical, of your valuable paper, calls attention to a highly curious and startling fact observed and communicated by the reverend and highly eminent *savant*, Father Secchi, of Rome, concerning the transparency of iron while red-hot. The fact that iron, steel, and also platinum and copper, are transparent while at a bright red heat, has been known long since, not only to practical engineers, but, as regards iron, steel, copper and platinum, to workers in these metals. The account given of the manner in which the excellent member of an eminent society found out this property of iron is as follows: The reverend Father had ordered a strong iron tube to be made. As it was intended for an apparatus requiring a vacuum, it was essential that this tube should be perfectly air-tight; and as Father Secchi had some doubts about its soundness in this respect, in order to set these at rest the tube was made red-hot and taken into a dark place, when Father Secchi clearly perceived through the iron, which was half a centimetre thick, a crack inside the tube, and which did not reach to the outer surface. It is rather curious that the fact of the metals above alluded to, to which I have reason to believe that gold may be added, becoming transparent at red heat should have escaped the notice of scientific men. It requires, however, a good bright red heat; but the transparency of the metals is evident thus even in daylight, as I know from my own experience while working in an engineering establishment attached to a large sugar refinery, now many years ago.

I am, &c., A. Adriani.

[The above statements are so much at variance with all previous ideas on the subject, that much stronger evidence will be required before the transparency of red- or white-hot metals can be accepted as proved.—Ed. *C. N.*]

CHEMICAL NOTICES FROM FOREIGN SOURCES.

Alcohols, Synthesis of ($C = 12$).—A. Liben. It appears probable that the action of zincic compounds of alcohol radicals on chlorinetted ether, would be limited to the chlorine atoms, and afford the means of passing from one step in the series of alcohol to another. It was necessary, in the first place, to establish the constitution of chlorinetted ethylic ether. It appears that this is best expressed by

$$\left.\begin{array}{r}C_2H_3Cl_2\\C_2H_5\end{array}\right\}O.$$

By this light the body described by Lieben and Bauer as resulting from the action of zincic ethide on chlorinetted ether has the constitution expressed by

$$\left.\begin{array}{r}C_2H_3ClC_2H_5\\C_2H_5\end{array}\right\}O,$$

and should be called ethylochlorether. This body, under the action of iodhydric acid, gives ethylic iodide, ethylated ethylic chloride, and ethylated ethylic iodide, which last-mentioned body has the composition and boiling-point of Wurtz's butylic iodide. With argentic acetate it gives ethylated ethylic acetate and butylene, or a remarkable similar isomer of butylene. Ethylated ethylic acetate, when boiled with concentrated potash solution, gives ethylated alcohol, which is most probably isomeric with normal butylic alcohol, and identical with butylenic hydrate. If it be so, then Kolbe's conjecture that the ——enic hydrates are secondary alcohols is established. Viewed from the same standpoint, another body which has also been described by Lieben and Bauer as a product of the action of zincic ethide on chlorinetted ether, wherein both chlorine atoms are replaced by ethyl, becomes diethylether.

$$\left.\begin{array}{r}C_2H_3.C_2H_5.C_2H_5\\C_2H_5\end{array}\right\}O.$$

Iodhydric acid produces, with it, ethylic iodide and diethylated ethylic iodide, and from this we may expect to obtain diethylated ethylic alcohol, a secondary or tertiary alcohol isomeric only with hexylic alcohol. It is easy to foresee the synthesis, on those principles, of an almost endless series of different alcohols.—(*Ann. Chem. Pharm.*. cxli. 236.)

Mellitic Acid ($C=12$).—A. Baeyer gives, by letter, some results of researches undertaken in communion with Scheibler. Mellitic acid is hexabasic; it is benzol in which H_6 are placed by $(CO_2H)_6$. Heated with lime, it splits into benzol and carbonic dioxide; sodium amalgam adds H_6 to it, with production of a hexabasic acid $C_6H_6(CO_2H)_6$, which is converted by sulphuric acid into a tetrabasic acid, $C_6H_2(CO_2H)_4$, to which body H_4 may be added, and carbonic dioxide again expelled by sulphuric acid, when benzoic acid is finally obtained. To complete the series beginning with $C_6(CO_2H)_6$, and ending with $C_6H_5(CO_2H)$, three members of which have been studied, and to extend the researches on the group beginning with $C_6H_6(CO_2H)_6$, more material is required than the authors can command; they therefore entreat possessors of the rare honeystone to come to their assistance.—(*Ann. Chem. Pharm.* cxli. 271.)

Dinitronaphthalene and Potassic Cyanide. ($C = 12$).—Mühlhäuser. The reactions of potassic cyanide with nitro-compounds having been chiefly studied in the acid representatives of this group of bodies, it seemed necessary to extend the research in other directions, and dinitronaphthalene was chosen as a substance easy to prepare and purify. Pfaundler and Oppenheim, in 1865, made some experiments on the subject, but did not succeed in obtaining decisive results. An alcoholic solution of dinitronaphthalene is mixed with aqueous potassic cyanide solution, reaction occurs in the cold but is advantageously assisted after a short time by gradually heating to boiling. A fine blue-green colour marks the end of the reaction. On standing, the solution deposits the potassic salt of naphtocyamic acid $C_{22}H_{10}KN_6O_{10}$, easily soluble in hot water and in alcohol with a splendid blue colour. Its tinctorial power is very great; it explodes if heated; the baric salt is insoluble in cold water and in ether, easily soluble in hot alcohol; the argentic salt is almost insoluble in hot alcohol or water, and is very explosive. The free acid is uncrystallisable, insoluble in water, dissolving with brownish-yellow colour in alcohol or in a mixture of alcohol and water; it is bibasic. The acid and its salts are scarcely less sensitive to alkalies and acids than cyanin or irisine, for if water be shaken with magnesia, filtered, and mixed with a solution of the acid, the yellowish colour of the latter is immediately changed to bright blue.—(*Ann. Chem. Pharm.* cxli. 214.)

Ethylic Sulphate, Action of Ethylic Iodide and Zinc on ($Ba = 137$, $C = 12$).—A. Claus. Perfectly dry ethylic sulphate was digested with excess of ethylic iodide and granulated zinc; the reaction was fully accomplished at the ordinary temperature. The product is a solid dark-green, resinoid, semi-fusible mass. Water and ether were very gradually added to the contents of the flask until the violent reaction ceased; the ethereal solution, having been separated from the aqueous one, was distilled until the temperature rose above 100°; the retort then contained an oily liquid, in bulk about two-thirds of the ethylic sulphate employed, which, when further heated, began to evolve sulphuric dioxide. When all liquid had passed over, a loss of a quarter had occurred. This experiment was not, therefore, repeated, but the oil was boiled with successive small portions of baric hydrated oxide and water; the solution was saturated with carbonic acid, filtered, and evaporated; finally, crystals of $Ba.C_4H_{10}S_2O_4$ baric ethylsulphite were obtained. No other product of the principal reaction is described, but the fact that the addition of water in the first instance caused a copious deposit of zincic hydrated oxide suggests the formation of an intermediate zincic compound analogous to those obtained by Frankland and Duppa under corresponding conditions.—(*Ann Chem. Pharm.* cxli. 228.)

Nicotine (C=12).—Dr. C. Huber. When treated with chromic acid, nicotine gives an acid $C_6H_5NO_2$, yielding easily crystallisable salts and azo-compounds. Distillation with lime produces C_5H_5N, an oily base soluble in water. The first reaction also produces another acid richer in carbon, and at least one base. Details will be given at a future time.—(*Ann. Chem. Pharm.* cxli. 271.)

Styrol, Isomeric States of.—M. Berthelot. Styrol from storax possesses a rotatory power on polarised light. That from cinnamates has none. The former is more easily attacked by reagents, and, when mixed with sulphuric acid, disengages more heat than the latter. Polymers of styrol produced by the action of heat or of potassium reproduce at 300° the original styrol, whilst those produced by the action of sulphuric acid distil in part undecomposed, behaving, indeed, as mixtures of distyrol, volatile at about 300°, and more highly condensed and less volatile polymers.—(*Bull. Soc. Chim. Paris*, 1867, 112.)

Coal-tar, Synthetical and Analytical Studies on.—M. Berthelot. The reactions of benzol and ethylene at elevated temperatures give rise to styrol, naphthalene, anthracene, chrysene, and some other constituents of coal-tar, the parent substances being themselves directly derivable from acetylene. Another group of constituents—the benzol series—might be expected to result from the action of formene on benzol; but reaction occurs only at a temperature incompatible with the existence of toluol, and anthracene is produced—a fact not without interest, since toluol is converted by heat into anthracene. Free formene differs therefore from free ethylene in its behaviour with benzol at high temperature. To obtain formene and benzol in presence of each other and nascent, a mixture of sodic acetate and benzoate was heated. A small quantity of toluol and a mixture of probably higher benzol homologues were obtained. The synthesis of toluol involves that of toluidine and of numerous other coloured coal-tar derivatives. The group of constituents which may be represented by aniline was studied in that member. A mixture of benzol and ammonia passed through a red-hot tube produced small, but unmistakable proportions of aniline. The oxygenated bodies—phenol, &c.—will be commented on at a future time. With respect to the analysis of coal-tar, cumol at a red heat gives rise to precisely the same series of hydrocarbons as contained in that complex mixture.—(*Bull. Soc. Chim. Paris* 1867, 113.)

Thermo-chemical Conditions of Pyrogenic Reactions.—M. Berthelot. Ethylene combines readily at a high temperature with hydrogen and benzol, acetylene heated with many hydrocarbons combines with them freely; formene, water, carbonic dioxide, heated with hydrocarbons, either do not combine with them, or do so at a very high temperature only. The reason of this difference appears to be that the formation of ethylene and of acetylene is attended by a very slight evolution or even by an absorption of heat; the compounds when formed retain much energy, resembling in some degree simple bodies, and in their combination with other bodies, positive work is performed with disengagement of heat; the formation of formene, water, carbonic dioxide, and ammonia is effected with great disengagement of heat, and their combination with hydrocarbons with elimination of hydrogen, if occurring at all, will be accomplished with difficulty from the negative work of the direct affinities, and the absorption of heat attending the elimination of hydrogen; therefore in these cases it is necessary to call in the aid of bodies possessed of powerful affinities to supplement the negative work, and to perform the reactions by some indirect methods.—(*Bull. Soc. Chim. Paris*, 1867, 122.)

Periodic Acid, its Basicity.—C. G. Lautsch has examined a number of periodates without being able to give decisive evidence on this point. He is inclined to the opinion that this acid is pentatomic and tribasic, notwithstanding that a difference between atomicity and basicity has not yet been established in the case of any mineral acid. The question might be conclusively answered if it were possible to introduce into the molecule of the acid alcohol and organic acid radicals, but the oxidising power of the acid is so great that it is doubtful whether it is possible to effect this replacement.—(*Journ. prakt. Chem.* c. 65.)

Cobalt and Nickel, Equivalents of.—Dr. E. v. Sommaruga. The determination of the equivalent of cobalt was performed on Gibb's and Genth's purpureo-cobaltic chloride; this salt was dried perfectly by many hours' heating at 110°, weighed, heated till the ammonia, ammonic chloride, and water were wholly expelled. The resulting very hygroscopic cobaltic chloride was then reduced by hydrogen; the metallic cobalt was weighed, and gave a mean of seven determinations 29·965, maximum 30·009, minimum 29·916.

The nickelo-potassic sulphate was used for the determination of the equivalent of nickel. To prepare this salt, commercial nickel was dissolved in dilute sulphuric acid, to which nitric acid was occasionally added; a quantity of potassic sulphate, insufficient to combine with all the nickelic sulphate, was added to the filtered solution; the solution was evaporated to crystallisation, and the crystals, after washing out the nickelic sulphate, were recrystallised several times. The final product worked on still retained traces of cobalt, but no other impurity. The crystals were dried at 100° and weighed; the sulphuric acid was determined as usual, and the equivalent of nickel calculated from the weight of baric sulphate. The mean of six experiments was 29·013; maximum, 29·079; minimum, 28·911. The proximity of these numbers to those obtained by R. Schneider from different experimental bases supports the author in assigning to cobalt the equivalent 30, to nickel 29.—(*Sitzungsber. Akad. Wien*, June, 1866.)

Vapour Density, Determination of.—R. Bunsen. As the omission of one minute detail would, in performing determinations by this method, render the result worthless, an abstract of this wonderful paper would be of little value, and no abstract could give an idea of the astonishing delicacy of the operations described; we therefore refer readers to the original, remarking that with carbonic dioxide, six experiments, in each of which less than ·35 gramme was employed, a mean 1·527, maximum 1·529, minimum 1·525, were found. Regnault, working on 19 grammes, found 1·529.—(*Ann. Chem. Pharm.* cxli. 273.)

Fatty Acids, Chloro-derivatives of (C=12).—Dr. W. Schlebusch. Hypochlorous acid does not unite with fatty acids, but introduces chlorine into their molecule; the acetic acid chloro-derivative is difficult to obtain; valeric acid gives chlorvaleric acid, described by Clark and Fittig. Valerolactic acid and its baric, cupric, and argentic salts are described. Butalanine $C_5H_{11}O_2N$ was obtained by heating the crude chlorvaleric acid with absolute alcohol saturated with ammonia. After a few hours' exposure in sealed tubes to 120°, the solution was evaporated to expel ammonia and alcohol, heated with baric hydrated oxide to decompose ammonic chloride, freed from baric salts by sulphuric acid, and evaporated to crystallisation. Butalanine combines with acids. With chlorhydric acid transparent plates are formed.—(*Ann. Chem. Pharm.* cxli. 322.)

Carminic Acid (C = 12).—H. Hlasiwetz and A. Grabowski. Carminic acid, when boiled with dilute sulphuric acid, splits into carmine red and sugar; the latter reduces Trommer's solution, and gives Pettenkofer's reaction, but neither ferments nor acts on polarised light; traces of it are dissolved by alcohol. Dried at 50°, its formula was $C_6H_{10}O_6$; at 100°, $C_6H_8O_4$.

Carmine red, $C_{11}H_{12}O_7$, is a dark purple amorphous substance, reflecting green light, soluble in water and alcohol, with a fine red color; insoluble in ether. Like carminic acid, it pertinaciously retains traces of phosphates; its alcoholic solution, when treated with alcoholic potash solution, deposits the whole of the carmine red as $C_{11}H_{10}K_2O_7$, from which the corresponding baric and calcic compounds were obtained. Powerful reducing agents perfectly decolorise the solution of carmine red, but the resulting body could not be separated in a pure state. Fused with potash solution of appropriate strength, carmine red gives oxalic and succinic acids, and coccinin—probably $C_{14}H_{12}O_5$—a body resembling chinon. Its crystals polarise light, are insoluble in water, easily soluble in alcohol, difficultly so in ether. Coccinin dissolves very easily in dilute alkaline solutions, and in such solution is one of the most sensitive bodies to the action of oxygen. The solution is at first yellow, then violet, finally a magnificent purple red. Few bodies give rise to so many phenomena of colour as coccinin.

From the splitting of carminic acid into carmine red and sugar, perhaps Schützenberger's is the nearest to the real formula—probably $C_{17}H_{18}O_{10}$—of that much-investigated body.—(*Ann. Chem. Pharm.* cxli. 329.)

Rufigallic Acid, a Derivative of ($C = 12$.)—G. Malin. Rufigallic acid was fused with potash, and gave among other things a straw-coloured body crystallising in very slender needles soluble in boiling water, in alcohol, and in ether, sparingly soluble in cold water, reacting acid, decomposed by heat; its aqueous solution reduced argentic and alkaline cupric solutions; it did not appear to combine very definitely with anything. Its formula is $C_6H_4O_3$, and its name oxychinone.—(*Ann. Chem. Pharm.* cxli. 345.)

Resins rendered Soluble.—H. Violette. Copal and other refractory resins are soluble in oil of turpentine, &c., if they have been heated for 15—20 minutes to 350—400°, of course in closed vessels. The best way of operating is to heat the resin for a few minutes in an open vessel; 5 or 6 per cent. of water are by this means expelled. The vessel is then closed, and the heating continued. The product gives very excellent varnishes. To avoid the necessity of heating the varnishes made as described, in order to brighten them, the oil, &c., which it is intended should be used, may be heated together with the resin. The product then simply needs dilution.—(*Ann. Chim. Phys.* [4] x. 310.)

Phosphorous Acid ($O = 16$).—Rammelsberg. The examination of compounds of phosphorous acid with dyadic metals leads to the conclusion that this acid exists in three states, corresponding to the ortho-, pyro-, and metaphosphoric acids. Thus the (K', Na', Am'), together with the (Pb'', Cu'', Cd'', Mn'', Co'', Ni''?) salts, which may respectively be typified by HR'_2PO_3 and $HR''PO_3$, point to $\left.\begin{matrix}H_2\\H(PO)''\end{matrix}\right\}O_2$, corresponding to metaphosphoric acid. The (Ba'', Sr'', Ca'', Ni''?, Zn''?) salts give $H_5P_2O_7$, otherwise $\left.\begin{matrix}H_6\\2H(PO)''\end{matrix}\right\}O_5$, corresponding to pyrophosphoric acid, and the magnesic salt, though less decisively, indicates $\left.\begin{matrix}H_4\\H(PO)''\end{matrix}\right\}O_3$, corresponding to orthophosphoric acid. An ethylic salt, in which the hydrogen is wholly replaced by ethyl, is represented by $\left.\begin{matrix}2(C_2H_5)\\C_2H_5(PO)''\end{matrix}\right\}O_2$. It is to be observed that whilst the acid radical in phosphoric acid is triadic, that in phosphorous acid, having one bond saturated by H, is only dyadic $H(PO)''$; similarly the acid radical of hypophosphorous acid is only monadic $H_2(PO)'$.—(*Monatsber. Berl. Akad.* Aug. 1866.)

Ethylic Chloracetate, Action of Ammonic Carbonate on.—W. Heintz. The acids of the lactic series possess at once the functions of acids and alcohols, their basicity remaining unaltered by the replacement of hydrogen by acid radicals; and as under the influence of dyadic metals two molecules of acid will unite in order to furnish two replaceable atoms of H basylous, so under the influence of acid radicals should two molecules unite to furnish H_2 chlorous. The preparation of ethylic succinyllactate proves the correctness of this view. Dyadic carbonyl not having been used in a similar reaction, an attempt was made to effect this molecular combination by means of sodic carbonate, but the product was too small to be useful. Ammonic carbonate was therefore employed, it seeming likely that its volatility would assist the reaction, but an examination of the products obtained showed that carbonic acid did not intervene in the reaction which really did occur, the result of the experiments being that ethylic tri-, and probably dis-, glycolamidate were produced.—(*Ann. Chem. Pharm.* cxli. 355.)

Cupric Persulphide ($Cu = 63{\cdot}5, S = 32$).—A. Gescher. Bloxam's body $Cu_2\ (NH_4)_2S_7$ was prepared by making solution of ammonic persulphide of such strength that a sample of it, mixed with an ammoniacal solution of cupric sulphate, deposited on standing red crystals quite free from black cupric sulphide. The right strength being attained, the solutions were mixed in bulk. To the crystals so obtained the formula $2CuS_3 + (NH_4)_2S$ is attributed.—(*Ann. Chem. Pharm.* cxli. 350.)

Aromatic Monamines give rise to Acids richer in Carbon.—A. W. Hofmann. The principal product obtained by distilling one molecule of oxalic acid with two molecules of aniline is phenylformamide, but secondary actions give rise in addition to carbonic dioxide; from the proportions one molecule of oxalic acid and one molecule of aniline, a crude product was distilled containing cyanhydric acid; heated with concentrated chlorhydric acid, and distilled, this crude distillate gave off with the steam an oily body of aromatic odour, which, when boiled with solution of soda, partly dissolved with disengagement of ammonia (resulting from decomposition of diphenylamine); the addition of chlorhydric acid to the alkaline solution produced a precipitate of benzoic acid, the argentic salt analysed, showing that the oily liquid with aromatic odour contained benzonitrile. This body evidently results from splitting of phenylformamide into water and benzonitrile.

The production is an analogous reaction of toluylic acid from toluidine, and a superbly crystalline acid, $C_{11}H_8O_2$, from naphthylamine, proves the generality of the reaction.—(*Comptes R.* lxiv. 387.)

Reactions, General Conditions of.—M. Berthelot considers that a chemical reaction which is capable of setting free a notable quantity of heat will necessarily and directly occur whenever the following conditions exist:—1. The reaction is one which reaches its limit within a very short time from its commencement; this condition is fundamental. 2. The reaction is one which begins without foreign aid at the temperature at the commencement of the experiment; reactions excluded by this condition act in conformity with the principle enunciated, if they are caused to set in, either by raising the temperature or by other means. 3. The parent substances and the products possess similar functions. He is of opinion that the inverse reactions of iodhydric acid and argentic chloride might be foreseen from the basis of this general principle, and that the analogous action of iodhydric acid on potassic chloride, which he has experimentally verified, is a further proof of the correctness of his view.—(*Comptes R.* lxiv. 413.)

Glass.—L. Clemandot. A sample of glass was made from silica and soda, free from lime, the silica being greatly in excess; after fusion at a very high temperature, and while fusing, a portion was taken out; this portion has remained perfectly unaltered; the remainder, after slow cooling in the crucible, became devitrified. Excess in the proportion of any one ingredient is likely to render glass devitrifiable. The most stable glass is that one which is most complex in composition—that is, which contains the greatest number of bases.—(*Comptes R.* lxiv. 415.)

MISCELLANEOUS.

Conversazione at the Pharmaceutical Society.—The annual gathering of the members of this Society and their friends took place on the evening of Tuesday last. The attendance was very numerous, and included several visitors distinguished in various branches of science. During the evening Mr. Baines gave a descriptive lecture on his geographical and ethnological explorations in the interior of Africa, profusely illustrated with photographs and transparencies, which were projected on a screen from a magic lantern. The objects of interest to chemists were very numerous, and amongst other novelties included large masses of fused and forged platinum, magnesium in various forms, and pure hydrate of sodium, by Messrs. Johnson and Matthey; Mr. Beane's valuable ozone generator exhibited at work, as used for the decolorisation of sugar, by Mr. Ladd; Professor Wheatstone's magneto-electric machine; specimens of the new porcelain standard yard and metre, as adopted by the International Association for obtaining a uniform decimal system of measures, weights, and coins, exhibited by Mr. Casella; and a new form of gas-engine, and a new hot-air engine. These last two are sufficiently interesting to deserve a more extended notice. The gas-engine is the invention of M. P. Hugon, and is the first we have seen that requires no electricity. This engine may be worked the whole day without any supervision whatever, and requires nothing but turning on and lighting the gas to set it in full work. When once started it may be locked up and left going day and night without stopping. There is so little danger from its use that its presence does not affect insurance. The expense of the power is said to be about one halfpenny per man per hour, and, used in this form, the employer possesses the advantage of discharging the labour at any moment, and of only paying for it whilst actually required.

The hot-air engine is based upon the fact, long known to scientific engineers, that the most economical mode of obtaining power from heat is by its direct application to the expansion of air, or other permanent gases, rather than by that of steam or any other vapour. The hot-air engine now described differs from the so-called "caloric engines" in several essential particulars as to its construction, so that it is free from those defects which have hitherto prevented the practical carrying out of the caloric theory. In this engine the motive power, instead of being derived from the expansion of air heated in a separate generator, as in former engines, is produced by the expansion of air heated by contact with the fuel itself, and, in addition to this source of the power, by the action of the expansive force of the gaseous products of the combustion of the fuel, which heretofore have been permitted to escape into the chimney without being in any way utilised in the production of power. This result is accomplished by placing the fuel in a grate which can be hermetically closed, and forcing the air required for combustion into it by means of an air-pump worked by the engine itself, so that no part of the heated air of the gases produced by the combustion of the fuel can escape without passing through the cylinder, and there doing duty in the production of force. It is obvious that by such an arrangement the employment of separate iron generators for the purpose of heating the air is dispensed with, and that thereby one of the chief difficulties of the old caloric engine is avoided; for in the hot-air engine the fuel is contained in a fire-clay furnace surrounded by an air-tight iron casing, which in this way is entirely protected from injury. The fuel, which may be anthracite, smokeless coal, or coke, is thus burned under pressure with great regularity, and with the production of a uniform temperature, and at a rate exactly proportionate to the duty the engine is called upon to perform, thus avoiding all waste of fuel—a result which has not been attained with any form of engine yet introduced.

The heated air, together with the gases produced by the combustion of the fuel, pass from the fire-box directly into the cylinder, so that every unit of heat produced is converted into force. The piston consists of a hollow plunger, to which the piston-rod is attached; the packing is placed around its upper circumference, where the heat is so moderate as to permit of efficient packing and lubrication. By means of an air-pump worked by the piston, a supply of air is forced into the grate. It here comes in contact with the fire, and a portion of it, in maintaining combustion, combines with the carbon, producing carbonic acid, &c.; while another portion of the air in excess takes up heat, and is thereby expanded. The mixed heated air and gaseous products of combustion speedily accumulate such an amount of expansive force as to set the engine in motion, by pressing on the piston. At the end of the stroke the expanded gases escape by the waste-pipe, which may be connected by a common stove-pipe with an ordinary chimney. Each upward stroke of the piston produces a downward corresponding stroke of the air-pump, and forces a fresh charge of cold air into the grate to maintain the combustion of the fuel, thus keeping up a continual supply of heated air and gaseous products. The power is increased or diminished by dampers, which pass the air through or over the fire, according to the amount required.

The chief advantages of the hot-air engine will be found in the very important facts, that there is not the most remote danger in its use. The furnace is perfectly insulated, so that all risk of fire is entirely avoided, and the presence of water, whether in large or small quantity, is dispensed with; so that this engine can be employed under circumstances where it would be impossible to use a steam-engine. Either of these engines will be found invaluable in the pharmaceutical laboratory or physical workshop. Each possesses advantages peculiar to itself, but both are free from the drawbacks attending the use of steam-engines, as they require no skilled labour, no water, do not increase insurance, make no dirt, and are entirely free from danger.

Alum Crystallisations over Fresh Flowers.—Make baskets of pliable copper wire, and wrap them with gauze. Into these tie to the bottom violets, ferns, geranium leaves, chrysanthemums—in fact, any flowers except full-blown roses—and sink them in a solution of alum of one pound to the gallon of water, after the solution has cooled, as the colours will then be preserved in their original beauty, and the crystallised alum will hold faster than when from a hot solution. When you have a light covering of distinct crystals that cover completely the articles, remove carefully, and allow them to drain for twelve hours. These baskets make a beautiful parlour ornament, and for a long time preserve the freshness of the flowers.—*W. P. Creecy*, in the *Am. Journ. Pharmacy.*

Death of Walter Crum.—Many of our readers will hear with regret of the decease of Mr. Walter Crum, which took place on the 4th inst., at Thornliebank. Mr. Crum had been for more than twenty years a Fellow of the Royal Society. His papers, chiefly on subjects connected with calico-printing, were numerous, and bore the stamp of great talent and originality; we may especially mention his researches on Indigo, on the Acetates of Alumina, on Mordants in Dyeing, on Cotton-fibre, &c. By his death the Chemical Society loses one of its original Fellows and a respected Vice-President.

Civil List Pensions.—The following pensions on the Civil List have been recently granted:—100*l.* a year to Lady Harris, widow of Sir William Snow Harris, in consideration of her husband's valuable invention of the system of lightning conductors. 100*l.* a year to the Rev. Miles Joseph Berkeley, on account of his eminent services as a botanist, to practical horticulture and agriculture. 95*l.* a year to George Cruikshank, Esq., on account of his great merit as an artist.

Scientific Lecturing pays well in America. Professor Agassiz lately delivered a course of lectures under the auspices of the New York Association for the Advancement of Science, on the Natural History of Brazil, for which he demanded and was paid 500 dollars each, or 3,000 dollars for the course of six lectures. Taking the time he devoted to each lecture—that is to say, an average of one hour and forty minutes—it thus appears that he received five dollars a minute for every minute he spoke. It appears, however, that the association which engaged his services did not lose. —*Pall Mall Gazette.*

Extraction of Indium from the Products of the Roasting of Blende.—The flue dust which condenses in the chimneys of the zinc works of Goslar contain indium. The author has operated on 100 kilogrammes of this dust, which contains about one part of oxide of indium in 1000. To extract this metal, boil the deposit for half an hour with hydrochloric acid, and digest the clear liquid with pieces of zinc for six hours at the ordinary temperature. There is then deposited a black metallic powder, which is washed with water, and which contains copper, arsenic, cadmium, thallium, and indium. By boiling this with a concentrated solution of oxalic acid, a solution of cadmium, thallium, and indium is obtained; the latter is precipitated by ammonia, and the precipitate is then boiled with ammonia and afterwards with water, until the washings contain no more thallium. The oxide of indium is then almost pure, and only contains traces of iron, from which it may be freed by Dr. Winckler's method given in the Chemical News, vol. xiv. p. 157.—*M. Boettger*, in the *Journal für praktische Chemie*, t. xcviii. p. 26, No. 9.

Determination of Iodine by Means of Chloride of Silver.—To determine the iodine contained in organic hydriodates, M. Kraut digests the solution for several minutes with a known quantity of recently precipitated chloride of silver; the increase of the weight of the chloride of silver is in proportion to the amount of iodine. This method has the advantage of not altering the substance beyond the removal of its iodine, which is replaced by chlorine. The process is very useful in many cases.—*Zeitschrift für analytische Chemie*, iv. 167.

Analysis of Earth eaten in Borneo.—Some few years ago the manager of the Orange-Nassau colliery, near Zandjermasin, in the Island of Borneo, found that many of his workpeople (natives) consumed large quantities of a kind of clay; a sample of this material was forwarded to Batavia for analysis, and the following is the result in 100 parts:—

Pitcoal resin (organic matter volatile at red heat)	15·4
Pure carbon " " "	14·9
Silica " " "	38·3
Alumina " " "	27·7
Iron pyrites " " "	3·7
	100·0

We remind our readers that the eating of clay is a custom to which savages—or, at least, human beings of a very low degree of development—are freely given in various parts of the world. No other analysis of any of the substances used as such have been made, or at least, if made, they have not been published. The resident military medical officer at the above-named colliery is strongly inclined to consider it the duty of the manager to eradicate and discountenance this habit of the workmen, as it appears to injure their health.

Obituary.—It is with regret that we announce the death of Mr. Hippolyte Baillière, the well-known scientific publisher, which took place on Saturday last at his residence, 219, Regent Street. Mr. Baillière's illness has been long and painful, and for some time past his sons have taken a very active part in the practical management of the business. The late Mr. Baillière's energy and talents have caused his name to be known and respected in many of the capitals of Europe and America.

Quinine.—The *Homeward Mail* reports a singular affair which has just occurred at Calcutta. Under the Indian Patent Act, every exclusive privilege must cease if the Governor-General of India in Council shall declare that the same is generally prejudicial to the public. This has accordingly been done in the case of a petition filed by W. G. M'Ivor, who wishes for a patent for an alleged new invention for producing and preparing the different species and varieties of cinchona bark for the manufacture of quinine, quinidine, cinchonidine, and other alkaloids.

Estimation of Silver in a Metallic State.—According to M. Classen, silver is wholly precipitated by cadmium; when dealing with a nitric solution of silver, evaporate to dryness in the presence of sulphuric acid, dissolve the sulphate of silver in boiling water, plunge into it a plate of cadmium, and the reduction of the silver takes place at once. The silver is deposited in a compact mass, easily washed with water; as it may contain a little cadmium, boil it in the acid liquid until no hydrogen escapes; wash it until the water contains no sulphuric acid; then dry, and calcine; the silver, at first a black grey, takes the metallic lustre; it may then be weighed:—the results are very exact.

NOTES AND QUERIES.

Transformation of Naphthalin into Benzoic Acid.—Sir,—Can you inform me where I can obtain a formula for this process, mentioned in your number, 385, page 197, in Notes on the Paris Exhibition?—Sydney Hall. Swansea.

Nitro-benzol.—Sir,—Can you tell me how nitro-benzol can be obtained colourless and free from the tarry smell which usually accompanies it?—A Manufacturer.

Depositing Platinum Electrolytically.—Sir,—Will any of your correspondents tell me what is the best solution and battery power to be used for depositing a coherent film of platinum on brass? I should like it to be strong enough to resist liquid acids, but if this is impracticable, a resistance to acid vapours will suffice.

Chinese Blue.—Sir,—A friend of mine is desirous of obtaining a good receipt for making Chinese blue. He is a large consumer, and wishes to manufacture for himself and another house. He has made it, but not satisfactory in comparison with London manufacturers. He would of course pay handsomely for it. Would it be possible to obtain this? and through what channel?—R. H. W., Manchester.

Purified Shellac.—Sir,—In reference to the suggestion in your last number, I am the inventor of a new method of bleaching and purifying shellac, not by chlorine or its homologues, or by any injurious agent; it is in a state of solution in methylated spirits, and is applicable to gilt work, &c., not injuring anything it is applied to. To any one with a small capital, or in the above way of business, my services would be of value. I am not a thorough chemist, but possess a slight knowledge sufficient for the above. I cannot afford to advertise much, and think this would come under the head "Notes and Queries."—E. M. Nash.

Caramel Colours.—Sir,—Can any of your numerous readers tell me the best way to make these? I am a vinegar manufacturer, and use a considerable quantity for giving a dark colour to the vinegar. The manufacture is kept a profound secret by those few houses who make a specialty of it. I have made several attempts, but have hitherto been unsuccessful.—E. A. Glasgow.

Colours from Caramel.—Sir,—Perhaps the following information may be of use to your correspondent:—The secret consists in using glucose and heating it with an alkali. For vinegar, carbonate of ammonia is required, as a fixed alkali produces turbidity. 120 pounds of glucose require 6 pounds of carbonate of ammonia and 6 pounds of water. Heat together in a metal boiler till the glucose is of the desired colour, keeping the mixture well stirred. Then add 30 or 40 pounds of warm water poured in a thin stream.—F. Thompson.

Glass Drilling.—A correspondent, D. F., informs us that the process for working glass with files, drills, or other tools of steel, moistened with dilute sulphuric acid, as given by Dr. Lunge in the last number of the Chemical News, was the subject of a patent taken out by Maudsley some years back.

Nitro-benzol.—Sir,—In reply to "Manufacturer," I beg to inform him that I have prepared nitro-benzol nearly colourless, and with a pure fragrant odour, by distilling it much below its boiling point in a current of steam. The first portions, which contain benzol, &c., are to be rejected.

Light Coal Oil.—Sir,—Have any of your readers tried a plan of mixing the light oil obtained from coal tar with a solution of chloride of lime, and submitting the mixture to distillation? If so, what changes have been noticed, and what was the action on the sulphur compounds? —M.

Calico Printing.—Sir,—I have spent over three years in trying to improve various colours now in use by calico printers and dyers. I shall be glad if you could assist me in obtaining a little ready money for the following important discovery:—I can print on cotton goods a mordant that will produce a perfectly fast mauve, with, or at the same time as, garancine work, more fast and bright than any produced previously. I can dye all aniline colours faster and brighter than they have ever been done, and at a much less expense, on any kind of fabric. I can also produce the same shade of colour on mixed goods at one operation, and which is both cheap and simple. I will render every explanation necessary to any one applying through you or your valuable paper, and give any security and guarantee.—J. K. M.

Calico Printing.—Sir,—Referring to your note upon calico printing in last week's publication, I shall have pleasure in arranging with "J. K. M." a permanent system of remuneration for the advantages he names, which, if reliable, will stimulate the aniline colour trade. If you will let me have the address of "J. K. M.," I will at once communicate with him.—R. E. B., Manchester.

Caromel Colours.—Sir,—I have noted the correspondence respecting caramel colours in the Chemical News, and I should be glad to learn where glucose can be obtained in manufacturing quantities. There was a "Glucose and Caramel Company" in existence some time since, but it is now defunct; and I am therefore at a loss where to obtain it, and cannot spare the time for making it.—J. W.

Map Varnish.—Sir,—I want to prepare a good map varnish which will dry rapidly. If any of your correspondents will kindly instruct me in its preparation through the medium of your valuable section, "Notes and Queries," they will confer a favour upon me.—J. S.

Artificial Teeth.—Sir,—Within the last few years some improvements have been made in the manufacture of artificial teeth, by which they are rendered less brittle. I am anxious to find directions for their preparation, and shall be happy to pay any person who will assist me in this matter.—F. T., Leeds.

Crystallisation of Chromates.—Sir,—During the crystallising of bichromate of potash, a coating of light lemon-coloured crystals formed on the front of the bichromate. These crystals did not, however, appear till the crystals of bichromate had almost done forming. The mother liquor at the time was about six or eight degrees warmer than the surrounding atmosphere. If the liquor was allowed to remain in the crystallising pans until perfectly cold, the crystals referred to would increase to four or six inches in thickness. The form of these crystals was an oblique four-sided prism. Now, this did not appear from want of sulphuric acid; if that was the case, the crystals would have been green. The amount of potash required to decompose the oxide of chromium was calculated correctly and added. Has any of your readers who may be acquainted with the manufacture of bichromate of potash ever noticed the formation of the crystals named, and to what cause can their appearance be traced?—H.

Map Varnish.—Sir,—J. S. will find that he can make a very good varnish for covering over architectural and mechanical drawings, maps, &c., by dissolving one pound of white shellac, a quarter of a pound of camphor, and two ounces of Canada balsam in one gallon of alcohol.

Map Varnish.—Sir,—I beg to place the following recipe at the service of "J. S.," who wishes to know how to prepare a good, quick-drying map varnish:—Thin down with turpentine Canada balsam, and add one-fourth of the bulk of quick-drying pale copal varnish; lay on smoothly with a flat camel-hair brush, and let the map lie flat for a few hours.—Thos. Bournes.

Artificial Teeth.—In reply to the inquiry from F. T., Leeds, in our last number, we have received several letters. They have been forwarded as requested.

Chinese Blue.—Sir,—Will you oblige me by notifying that you wish to receive communications for a subscriber on the subject of Chinese blue from the same or others who replied to R. H. W., of Manchester? Information of value would be paid for.—Wm. Schofield.

Palm Oil.—Sir,—If any of your correspondents will inform me how much chrome is necessary to bleach a cwt. of palm oil, they will confer a favour on yours, &c., Milo.

Santonine.—Sir,—Can any of your readers favour me with some good directions for mounting santonine for the microscope so as to obtain good polarizing crystals?—F. Cooper.

Letters are waiting at our Office for F. T., Leeds; R. E. B., Manchester; H. P. Meaden; J. K. M.; Manufacturer.

ANSWERS TO CORRESPONDENTS.

⁂ All *Editorial Communications* are to be addressed to the Editor, and *Advertisements* and *Business Communications* to the Publisher, at the Office, 1, Wine Office Court, Fleet Street, London, E. C. *Private* letters for the Editor must be so marked.

⁂ In publishing letters from our Correspondents we do not thereby adopt the views of the writers. Our intention to give both sides of a question will frequently oblige us to publish opinions with which we do not agree.

D. H. W.—M. Stas's researches have appeared in the *Proceedings of the Royal Academy of Belgium*. Inquire at Asher's.

G. M. E., Brighton.—The instrument is not yet complete. A full description will appear in the Chemical News as soon as it is in operation.

M. Mond's Process for the Recovery of Sulphur from Alkali Waste.—The inventor has drawn our attention to the description of this process given by our Paris correspondent in our number for April 12, p. 183, as not being sufficiently clear. After saying that the process consists in forcing a current of air into the waste in order to oxidise it, and then lixiviating, the description should continue, "The solution is then drawn off, and air again forced into the waste, the mass again lixiviated, and the same treatment repeated a third time. The vats are so arranged that the liquor is allowed to run from one vat into the other in order to obtain it as concentrated as possible. The whole of these operations can be performed in from sixty to seventy-two hours." In the subsequent description, hyposulphite of lime should have been spoken of, instead of hyposulphite of soda. M. Mond says that the process is being successfully employed at many alkali works in England, amongst others at Messrs. Hutchinson and Co.'s, Widnes.

W.—Crucible is derived from the Latin *crucio*, to torture or twist.

Townsend, Hook and Co.—The address shall be sent. The author is preparing an article on the subject for these pages.

W. Becket.—The syrup of orange and quinine appears likely to be of value in cases where orange wine and quinine would be injurious to the system.

Constant Reader and *Another Person.*—Sunlight shining on a fire will not put it out nor prevent combustion. The explanation of the popular opinion on this point is, that the light of the sun is so immeasurably superior to that of the fire, that the latter appears to lose its brilliancy when sunlight falls on it, and to an ordinary observer would seem dead.

T. S.—We should be pleased to receive the information in a form fit for publication.

M. M. Soo.—The animal matter may be removed from the sections of bone by soaking them in caustic potash. The alkali must be well washed out before mounting the sections. Canada balsam is best for the latter purpose.

M. Jenkins.—What is usually known as queen's metal is an alloy formed of two parts of tin and one part each of lead, antimony, and bismuth.

A Student.—The term "wad" is applied to two distinct minerals. It generally means an ore of manganese mixed with iron; but in Cumberland the term is applied to plumbago.

A. K.—Tetradymite consists of sulphide and telluride of bismuth with impurities. Its name, however, is not derived from the fact of its being composed of four elements, but from its occurrence in quadruple crystals.

Clericus.—Chloroform is, perhaps, the best substance you can use for removing paint stains from oak. You must remember that it will likewise attack the varnish.

James Y.—The term iodide of calomel is retained in American pharmacy for a mixture of iodide and chlorides of mercury, prepared by mixing iodine with calomel. According to the same barbarous nomenclature, there is a biniodide of calomel.

Butter.—You are mistaken in supposing that only rancid butter contains butyric acid. Fresh butter contains a compound of butyric acid with glycerin, which is inodorous; and it is to the decomposition of this compound on standing, by which butyric acid is set free, that the flavour of rancidity is due.

A Student will find full information on the subject of the atomicity of radicals in Wurtz's "Introduction to Chemical Philosophy," page 104, *et seq.*

Kappa.—You will find all you require in Griffin's "Chemical Handicraft."

P. McPherson.—Heavy hydrocarbon oils containing no naphtha are convertible into oils of the naphtha series under the action of heat.

An Old Subscriber.—The lecture referred to is published in the *Journal of the Chemical Society*. Richardson and Watts's "Technology" gives an excellent account of the alkali manufacture.

Communications have been received from Pritchard, Burden, and Co.; S. Hall; Rev. B. W. Gibsone; T. Davies; F. M. Sargeant; J. Pratt; J. Tomlinson; M. Burton; H. Gillman; J. Hargreaves; J. W. Swindells; W. T. Suffolk; W. M. Bywater; Lloyd Smith; George Dutton (with enclosure); H. B. Condy; John Hull; Charles A. Wright; T. B.; Wotherspoon and Co.; G. M. E.; Sir W. Thompson (with enclosure); Geological Society (with enclosure); Dr. H. W.; F. Webb; G. Penny; Dr. S. Muspratt (with enclosure); R. H. W.; Sir H. J. Brownrigg; The Quekett Microscopical Club (with enclosure); J. Glen; A Constant Subscriber; G. A. Keyworth; L. Mond; J. H. Swindells; A. Jaeger; T. Sherlock; J. Kenyon; R. Hulse; J. C. Bell, F.C.S.; G. Thompson; Lewis, Ash, and Co.; A Subscriber; A Constant Reader; James Browne and Son; E. Tate; Messrs. Huskisson and Son; P. Squire; Johnson and Sons; Another Person; Gaskell, Deacon, and Co; Allhusen and Son; May and Baker; J. R. Hass and Co.; W. Beckett; Townsend, Hook, and Co.; S. Mellor; Dr. Adriani; F. Field; T. Sherlock; G. F. Rodwell; W. Webb; Price's Patent Candle Company; J. Sullivan; Lloyd Smith; the Walker Alkali Company; T. Parkins; C. Ogleby and Co.; J. Muspratt and Sons; J. F. MacFarlan and Co.; Davey, Yates, and Routledge; Allen and Hanbury; J. J. Vaughan; Professor B. Silliman (with enclosure); F. C. Calvert; Geological Society (with enclosure); Clericus; Henry Charlton; R. E. B.; J. V.; W. Gossage; Editors of Journal of Mining: William Schofield; E. Frankland (with enclosure); Runcorn Soap and Alkali Company (with enclosure); S. Muspratt, M. D., &c.; W. B.; A. C. Hadland and Co.; R. Wilkinson; Messrs. Denton and Co.; W. Bush; Pullar and Son; D. Forbes; S. Mellor; H. Gillman; C. W. Heaten; Sir B. C. Brodie; G. Foord; Clayton and Co.; F. Barnes; O. Solomon; M. Williams; W. Johnstone; Professor Angelo Pavesi; C. F. Burnard (with enclosure); Robert Bell (with enclosure); J. H. Swindells (with enclosure); William Allen; James Kayen; Thomas Burnes; H. Sugg; W. Perkins; A. H. Church.

Books Received.—"Chemical Notes for the Lecture Room," by D. Wood, F.C.S.; "The Poisons of the Spreading Diseases," by B. W. Richardson, M. A., &c.; "The Calculus of Chemical Operations; being a Method for the Investigation, by means of Symbols, of the Laws of the Distribution of Weight in Chemical Change. Part I. On the Construction of Chemical Symbols." By Sir B. C. Brodie, Bart., F.R.S.—"Dr. Ure's Dictionary of Arts, Manufactures, and Mines." Edited by Robert Hunt. Vols. I., II., and III.—"A Dictionary of Science, Literature, and Art." By W. T. Brande and Rev. G. W. Cox. Vols. I., II., and III.; "On a new process for Preparing Meat for Weak Stomachs," by W. Marcet, M.D., &c.; "Shaw's Medical Remembrancer," by Jonathan Hutchinson, F.R.C.S.

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IDEAL CHEMISTRY.

On Thursday next the Fellows of the Chemical Society will assemble at Burlington House to hear a lecture by Sir Benjamin Brodie on the new Chemical Calculus. The subject is perhaps the most abstruse which has ever been brought before the Society, and it has, therefore, been considered advisable for us to give in the present number a slight outline of the methods of reasoning adopted by the learned author, as set forth in the paper* which has just appeared in the *Philosophical Transactions*.

The memoir, of which the first part is the subject of this article, will mark a new epoch in that branch of chemical science which relates to the symbolic expression of facts. The complete paper is not yet published. The present part, which was read before the Royal Society on May 3, 1866, when it attracted the liveliest attention, and some little discussion from the chemists present, relates to the construction of chemical symbols. In the second part the author proposes to treat of the theory of chemical equations, which is intimately connected with the general processes of chemical reasoning, and especially with the consideration of the nature of that event which is termed a chemical change, of which a new analysis will be given founded on its symbolic expression. In the third part it is intended to consider the principles of symbolic classification, and the light thrown by this method upon the origin and nature of the numercial laws which limit the distribution of weight in chemical change.

The author mainly confines himself at present to the discovery of a system of symbolic expressions by which the composition of the units of weight of chemical substances may be accurately represented, and which may hereafter be employed for the purposes of chemical reasoning. This problem is of a perfectly real nature, admitting, where the experimental data are adequately supplied, of only one solution; and the discussion of this question involves the consideration of the fundamental principles of symbolic expression in chemistry.

In the first section, considerable attention is devoted to the definitions, and the various terms used have definite significations attached to them. "Ponderable matter" and "a chemical substance" require no explanation, but the expression "a weight" is used in a special sense. In this discussion every chemical substance, simple or compound, is exclusively regarded as a *weight* of matter. Its form, condition, or state is disregarded, and the only property with which the chemist has here to do is the transformation of the weights of matter, and the laws of the composition and resolution of these weights. The term *a weight* is used, therefore, in a concrete sense, as when we speak of "a box of weights," and call one of the pieces of metal in the box "*a weight*" of platinum or brass. In just this sense, Sir Benjamin Brodie speaks of *a weight* of water, of oxygen, of sulphur, etc., excluding all other forces but that of gravitation.

A "single weight" is a weight of any portion of matter regarded as one object. The matter making up this weight may be simple or compound, and, if the latter, chemically combined or not. So long as it is considered as one object, it is a single weight.

A "group of weights" is constituted of any number of single weights.

Between two portions of ponderable matter (two "weights") there may be *equality* and *identity* as regards weight. These terms require fuller explanation. Take a *weight* of water—a gramme, for instance—and apply heat to it; it increases in bulk; it becomes a gas; and at a still further elevation of temperature it is resolved into its components, oxygen and hydrogen. But throughout all these profound physical and chemical changes, its action on the balance does not vary. Now, there is the relation of *equality* of weight between a gramme of water and a gramme of oxy-hydrogen gas, as there is between a gramme of water and a gramme of lead. But the gramme of water is connected with the gramme of its component gases by another relation, that of continuity of existence, or *identity*, which does not exist between the gramme of water and the gramme of lead.

A compound weight is here defined as a single weight of which the whole is identical with two or more weights. Such weights are termed the components of the compound weight, which is said to be composed of them. A "simple weight" is a weight which is not compound—that is, which has only one component.

It is necessary to select a "unit of ponderable matter" which may serve as the common measure of those chemical properties which it is desired to investigate, and in this investigation the "unit" is defined as that portion of gaseous ponderable matter which occupies the volume of 1000 c.c. at 0° C., and a pressure of 760 m. m. of mercury. This volume is called the "unit of space," and the weight of a unit of hydrogen is selected as the standard. The weights of other units of matter can therefore be expressed absolutely in grammes, or relatively in reference to the standard unit of hydrogen.

When a compound weight (*e.g.*, a weight of water) is resolved into its component weights (*e.g.*, a weight of oxygen and a weight of hydrogen), the weight is said to be "distributed." The same expression is used in reference to the converse operation of synthesis. The meaning of the term "undistributed weight" follows from the above. A distributed weight is necessarily a compound weight, and an undistributed weight must be regarded, in respect to the events under consideration, as simple, although under other circumstances it might prove to be a compound weight.

A chemical operation is defined as an operation performed on the unit of space of which the result is *a weight*. These chemical operations are represented by symbols x, y.... The symbol $+$ is the symbol of the operation by which one weight is added to another to constitute with it one group. The symbol $-$ represents the removal of a weight from a group of weights. The symbol $=$ is the symbol of identity. The symbol $x + y$ represents a group constituted of the two weights A and B; $x + x$ or $2x$, is the symbol of two weights A; and $x - y$ is the symbol of the weight A without the weight B. The symbol 0 is the symbol of a group in which no weight appears, and which has had its origin in the several performances of the operations x and $-x$; so that $x - x = 0$, and $0 + x = x$. The symbols $+$, $-$, and $=$ are here used in a sense analogous to their arithmetical meaning. No uniform interpretation has hitherto been attached to them in chemistry.

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* "*The Calculus of Chemical Operations; being a Method for the Investigation, by Means of Symbols, of the Laws of the Distribution of Weight in Chemical Change*," by Sir Benjamin C. Brodie, Bart., F.R.S., Professor of Chemistry in the University of Oxford.

But the most important feature of the method is the introduction of the symbol xy as the symbol of a compound weight, of which two portions of matter, say A and B, are the component weights. This symbol indicates that we are to perform the operation y upon the unit of space, and then to perform successively upon that *same* unit the operation x, in which respect it differs from the symbol $x + y$, which indicates that the operations are to be performed upon distinct units, the results being different, according as the operations are performed *successively, jointly, severally*, or *collectively*. The symbol xy is the symbol of the *successive* operations x and y; (xy) represents their *joint* operations; $x + y$ represents them operating *severally*; and $(x + y)$ represents the same operations operating collectively. We can thus express with accuracy the various ways in which we conceive of the existence of the same ponderable matter.

The symbol 1 is necessarily contained as a common factor in every chemical symbol, and is the symbol of the common subject of operation, the unit of space. But the unit of space, as above explained, is space without weight, and the symbol 1 is therefore the symbol of "no weight." It is therefore inferred that $0 = 1$. This equation may at first sight appear paradoxical; it need not, however, be a matter of surprise that in the chemical calculus we should have two symbols for "no weight," since in that system the same ponderable matter may be denoted by xy and $x + y$. These different symbols are necessary, as representing the different ways the "weight" or the "no weight" has been obtained. Similarly, the symbol ∞ is to be interpreted as the symbol of the ponderable universe regarded as a whole, and the symbols 1 and ∞ represent in the calculus of chemistry the limits between which the values of all other symbols are comprised.

Now, according to the definition given of chemical identity, two weights are said to be identical which consist of the same weights; hence the weight (or matter) of which xy is the symbol is identical with the weight (or matter) of which $x + y$ is the symbol; and

$$xy = x + y.$$

This equation is the fundamental equation of the Calculus, and from it the properties of the symbols are derived.

After a discussion of the fundamental chemical equations, the symbols of simple weights (which are termed prime factors), and the construction of chemical equations from the data afforded by experiment, the author proceeds to the symbols of the units of chemical substances. One hypothesis is assumed, and that is that the unit of hydrogen is a simple weight. The symbol of this "weight" is expressed by the letter α. The absolute weight of the portion of ponderable matter thus symbolised—that is to say, of 1000 c.c. of hydrogen at 0° C. and 760 m. m. pressure—is 0·089 grm. This is identical with the "crith" of Dr. Hofmann. It is subsequently shown that the units of the elements mercury, zinc, cadmium, and tin may be symbolised in an equally simple manner. The author employs letters of the Greek alphabet as symbols of simple weights. Speaking of this, he says, "It is a mistake to confuse the objects of a symbolic system with those of a 'memoria technica,' and I am inclined to believe that a purely accidental distribution of letters among the weights to be expressed would be the best. In the selection here made, however, I have not proceeded rigidly upon this principle, a certain reminiscence of the name being retained in the symbol, as for example ξ ὀξύς, the θ of θεὶον, the χ of χλωρός, and the δ of ὑδράργυρος. Facility of writing and reading the symbols is, however, far more important than any aid to memory which can be thus afforded, and these points are to be mainly considered. The unit of hydrogen, which occupies a peculiar position as the 'modulus' of the system, is indicated by a special symbol α."

It is known by experiment that 2 units of water can be decomposed into 2 units of hydrogen and 1 unit of oxygen. Now, let

$$\alpha^m\xi^{m_1} = \text{symbol of the unit of water,}$$
$$\alpha \quad = \text{symbol of the unit of hydrogen,}$$
$$\alpha^n\xi^{n_1} = \text{symbol of the unit of oxygen,}$$

where α and ξ are the symbols of simple weights, and m, m_1, n, n_1 positive integers. Then

$$2\alpha^m\xi^{m_1} = 2\alpha + \alpha^n\xi^{n_1},$$

To this is attached the condition that

$$w(\alpha) = 1,$$
$$m + m_1 w(\xi) = 9;$$

from the fundamental equation

$$(\alpha^m\xi^{m_1})^2 = \alpha^2\alpha^n\xi^{n_1},$$

whence

$$2m = 2 + n \text{ and } 2m_1 = n_1.$$

Selecting from the possible solutions of these equations the minimum solution as both necessary and sufficient to satisfy the condition of the equation, we have

$$m = 1, \quad n = 0,$$
$$m_1 = 1, \quad n_1 = 2,$$

which give

Symbol of water, $\alpha\xi$;
Symbol of oxygen, ξ^2;

the relative weights corresponding to the prime factors α and ξ are

$$w(\alpha) = 1,$$
$$w(\xi) = 8,$$

the equation being thus expressed:—

$$2\alpha\xi = 2\alpha + \xi^2.$$

The following table is given of the combinations of the prime factors α and ξ:—

Name of substance.	Prime Factors.	Absolute weight in grammes.	Relative Weight.
	α	0·089	1
	ξ	0·715	8
	Symbol.		
Hydrogen................	α	0·089	1
Oxygen..................	ξ^2	1·430	16
Water....................	$\alpha\xi$	0·805	9
Peroxide of hydrogen......	$\alpha\xi^2$	1·520	17

By a similar process of reasoning, starting from the ascertained facts that the density of sulphur vapour is 32, that of hydrogen being 1, and that the ponderable matter of 2 units of hydrogen is identical with the ponderable matter of 2 units of hydrogen and 1 unit of sulphur, it is shown that the symbol of sulphur is θ^2, and that of sulphuretted hydrogen $\alpha\theta$. The symbol of sulphuric acid $\alpha\theta\xi^4$, and that of pentathionic acid $\alpha\theta^5\xi^6$. Similarly, selenium is symbolised as λ^2, seleniuretted hydrogen becoming $\alpha\lambda$.

In the case of chlorine, which may be taken as the representative of another group of elements, it is ascertained that 2 volumes of hydrochloric acid can be decomposed into 1 volume of hydrochloric acid and 1 volume of chlorine.

Hence, putting $\alpha^m\chi^{m_1}$ as the symbol of the unit of hydrochloric acid, and $\alpha^n\chi^{n_1}$ as the symbol of the unit of chlorine,

$$2\alpha^m\chi^{m_1}=\alpha+\alpha^n\chi^{n_1},$$

and

$$(\alpha^m\chi^{m_1})^2=\alpha\alpha^n\chi^{n_1};$$

whence

$$2m=1+n$$
$$2m_1=n_1$$

and

$$m=1,\quad m_1=1$$
$$n=1,\quad n_1=2,$$

a minimum.

Since the density of hydrochloric acid is 18·25, we have to determine the absolute weight of the simple weight χ,

$$m+m_1w(\chi)=18{\cdot}25,$$

whence

$$w(\chi)=17{\cdot}25,$$

which gives the

Symbol of hydrochloric acid $\alpha\chi$,
Symbol of chlorine........ $\alpha\chi^2$.

We can only briefly allude to the discussion of the symbols of carbon, silicon, and boron. Owing to the impossibility of asertaining the vapour densities of these elements by direct experiment, their symbols cannot be determined in an analogous manner to those of the preceding elements; but inasmuch as we are able to construct numerous chemical equations which connect the vapour densities, of carbon, silicon, and boron with known vapour densities, we are able to determine from these, within certain limits, the symbol of the elements themselves.

From such data as these it is rendered very probable that the symbols of carbon, silicon, and boron are respectively of the terms

$$\kappa^y,\ \sigma^y,\ (\alpha\beta^2)^y.$$

In the case of the elements antimony, bismuth, tin, zinc, cadmium, and silver, different principles of investigation have to be adopted, and their symbols are given according to two or more hypotheses.

The following table of the symbols of the units of certain well-known substances affords sufficient illustration of the method pursued:—

Symbol of iodine	$\alpha\omega^2$
" " bromine	$\alpha\beta^2$
" " nitrogen	$\alpha\nu^2$
" " phosphorus	$\alpha^2\phi^4$
" " arsenic	$\alpha^2\rho^4$
" " chloric acid	$\alpha\chi\xi^3$
" " ammonia	$\alpha^3\nu$
" " chloride of ammonium	$\alpha^3\nu\chi$
" " phosphide of hydrogen	$\alpha^3\phi$
" " oxychloride of phosphorus	$\alpha^2\phi\chi^3\xi$
" " acetylene	$\alpha\kappa^2$
" " alcohol	$\alpha^3\kappa^2\xi$
" " hydrocyanic acid	$\alpha\nu\kappa$
" " cyanogen	$\alpha\nu^2\kappa^2$

We quote the following from the concluding remarks of the author:

"Our conclusions on this point are so remarkable, and so contrary to anticipation, that doubtless we could never trust them but for the simple and exact process by which they are deduced. Now, the conceptions which we form of the nature of the elemental bodies constitute the fundamental theory of the science, for these conceptions comprise and determine every similar conception. The unit of the element hydrogen is here conceived of as a simple weight, and symbolised by the letter α. That, to say the least, this view may be permitted, is proved by constructing the symbols of chemical substances upon this hypothesis. There are, however, certain exceptions, be they real or apparent, in which this mode of expression is impossible." . .

"The unit of the element mercury, and the units of several other metals, such as zinc, cadmium and tin, so far as our imperfect experience extends, appear to be analogous in this respect to hydrogen. But these are the only elements of this simple composition. The units of a second group of which the element oxygen, symbolised as ξ^2, may be taken as a type, and to which belong sulphur θ^2 and selenium λ^2, are composed of two identical simple weights, and the facts of the science do not permit us to assume these units as otherwise composed. Lastly, another group of elements appears in this system of a different and more complex composition, to which group belong the elements chlorine $\alpha\chi^2$, bromine $\alpha\beta^2$, iodine $\alpha\omega^2$, nitrogen $\alpha\nu^2$ phosphorus $(\alpha\phi^2)^2$, arsenic $(\alpha\rho^2)^2$, and, in all probability, numerous other elements. The simplest view which, consistently with the fundamental hypothesis, can be taken of the composition of these elements, regard being had to the total system of chemical combinations, is that they are severally composed of a unit of hydrogen and of two identical simple weights—as, for example, in the case of chlorine, of the simple weight α and two of the simple weights symbolised by χ, so that the elements of this group are to be considered as combinations of elements of the two previous forms respectively."

The author concludes by saying that from these and other equations "we unavoidably have suggested to us as the ultimate origin of our actual system of combinations . . . a group of elements, $\xi, \theta, \chi, \beta, \omega, \nu,$ . . . of the densities indicated by these symbols, and which, . . . we cannot but surmise, may some day become, or may in the past have been, 'isolated and independent existences.' Examples of these simple monad forms of material being are preserved to us in such elements as hydrogen and mercury, which appear in the chemical system, as records suggestive of a state of things different from that which actually prevails, but which has passed away, and which we are unable to restore."

"Such a hypothesis is not precluded to us, but nevertheless we are not to imagine that it is a necessary inference from the facts. So far as the principles or conclusions of this method are concerned, the "simple weights" $\xi, \theta, \chi, \beta, \omega, \nu, \phi$. . . . may be treated purely as "ideal" existences created and called into being to satisfy the demands of the intellect, to enable us to reason and to think in reference to chemical phenomena, but destined to vanish from the scene when their purpose has been served; and the existence of which, as external realities, we neither assume nor deny."

THE CHEMISTRY OF THE FUTURE.

THE meeting of the Chemical Society on Thursday, the 6th inst., will always be memorable in the history of the Society. The importance of the subject which Sir Benjamin Brodie was to bring forward, the fact that eminent physicists and mathematicians had been specially invited to attend, and the probability that the discussion would vie in interest with the lecture itself, justified us in taking unusual means to secure a full and accurate report of the proceedings. We are sure we need make no apology for devoting to this abstruse subject so large a space of our present issue. The report of the lecture, and the discussion thereon, we are justified in speaking of with pride as a veritable *tour de force* of the shorthand writer. Having to deal with a multitude of technical expressions, enhancing the difficulties of his wonderful art, he has given to our readers the very words as they fell from the lips of the speakers; the editorial right of omission or condensation having been very sparingly exercised.

In order to give unabridged and undivided this remarkable report, occupying almost the entire space of an ordinary number, we have been compelled to omit several original articles, and nearly the whole of our foreign and home correspondence; but so that we should not disappoint that numerous class among our readers, who care less for ideal than for positive chemistry, and who value the Chemical News in proportion as it gives them solid facts and useful hints, we have enlarged this number to twenty pages by the issue of a supplement.

SCIENTIFIC AND ANALYTICAL CHEMISTRY.

Remarks on some Recent Contributions to the History of the Benzole Series,[*] *by* A. H. Church, *M.A., Professor of Chemistry, Royal Agricultural College, Cirencester.*

The more elaborate and exact study of the homologues and derivatives of benzole has revealed many most important facts. Admirable digests of these recent researches will be found in Will's *Jahresbericht* for 1865 (pp. 514 *et seq.*), and in the second volume of Kekulé's *Lehrbuch der organischen Chemie* (pp. 528 *et seq.*). My present object in writing on this subject is twofold—I wish to reassert my own discovery of several new substances and new reactions, and I wish to vindicate the accuracy of some of my experiments against the attacks which they have suffered from one or two foreign chemists.

Twelve years ago my first paper on the benzole series was published. Since that time not only have there been made many additions to our knowledge of hydro-carbons and of the nature of isomerism, but the process of fractional distillation has also been materially improved. Thus it has come to pass that some of my former results I now interpret differently, some I have modified or further developed, some I have corrected in later papers, while some are discoveries now generally acknowledged, and which, in not a few cases, have served as starting-points for the subsequent researches of other chemists. I am compelled, therefore, to attribute, in some measure at least, the hostile criticism of Mr. C. M. Warren and Professor Beilstein to their imperfect acquaintance with the whole series of my published notes and papers. I do not wish to enter into any personal controversy with my critics, and I will not speak of the manner in which my reputation as a chemist has been assailed, but I do feel bound to restate the broad facts concerning the matters in dispute, drawing my data entirely from published sources.

These matters in dispute may be arranged under the three heads of—1. Boiling points; 2. The parabenzole series; 3. Reactions.

1. Boiling Points.—The boiling points I assigned[†] in 1855 to benzole and to the cumole from cuminic acid do not differ widely from those given by Mr. Warren,[*] and even the slight difference that exists is probably due to the methods adopted for correcting the observed temperatures—

	Warren.	Church.
Benzole	80·1	80·8
Bumole	151·1	148·4

With toluole, xylole, and cymole the case is different. Warren places the boiling point of toluole at 110·3°, my determination gave 103·7°,[†] a result not far from that of Glénard and Boudault (106°) and that of Max Dürre. Coal naphtha, there is no doubt, contains a large quantity of a hydrocarbon boiling between 109° and 113°, and seemingly having the composition and properties of the toluole from toluic acid; but this fact is not incompatible with the co-existence of other hydrocarbons of the same formula. I may have placed the boiling point of toluole 6° too low; all I can say as to this matter is that the liquid boiling at 104° gave me every proof of its being toluole.

Xylole was discovered by Cahours in the light oils of wood naphtha. It was ascertained by its discoverer to boil between 128° and 130°. My experiments led me to fix its boiling boint a little lower, at 126·28; and as I obtained from coal naphtha an oil having the same composition and properties, and yielding apparently the same derivatives, I concluded the two hydrocarbons to be identical. Mr. Warren fails to discover in his sample of coal naphtha this hydrocarbon boiling at 126°—he concludes at once that it does not exist in any coal naphtha, and that my experiments are untrustworthy. As, however, Mr. Warren makes out Cahours to have been wrong as to his xylole, and tells us that Mansfield mistook cumole for cymole, I cannot think myself very badly off in sharing with those eminent chemists the censure of Mr. Warren, for, "if gold rust, what should iron do?" The liquid which Mr. Warren calls xylole is the petrole of Bussenius and Eisenstuck;[‡] the pseudocumole[§] of W. De la Rue and H. Müller; my paraxylole,[‖] and has also been redescribed by Béchamp[¶] and by Naquet.[**] But that the true xylole discovered by Cahours really does exist I cannot doubt. I would here merely add that the boiling point of my xylidine,[††] 213°—214°, differs by little more than 1° from the number assigned by Deumelandt[‡‡] to xylidine obtained from a coal naphtha fraction boiling 13° above that which yielded my product.

The case of cymole still remains. Warren does not find this hydrocarbon in coal naphtha, while he affirms the boiling point of the cymole from oil of cumin to be 179·6°. Dr. Noad, who prepared large quantities of this liquid, gives 171·5° as its boiling point, while my determination was 170·7° for the oil as distilled from sodium, and 175° to 176° after treatment with oil of vitriol. This number 175° is that now generally confirmed by other chemists as the true boiling point of cymole.

The discrepancies between some of my boiling points and those more recently determined depend in great measure upon the different corrections applied to the thermometric indications, and also upon the existence in coal naphthas of two series of isomers, which seem to me to have been jumbled together by some observers.

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[*] Communicated by the author.

[†] *Phil. Mag.* 1855.

[*] *Sill. Amer. Journ.* (2) xl. pp. 89, 216, 384.

[†] *Phil. Mag.* 1855. [‡] *Ann. Chem. Pharm.* cxiii. 151.

[§] *Phil. Trans.* 1863, pp. 331, 342. [‖] Chem. News, xii, 292.

[¶] *Compt. Rend.* 1864, lix. 47, 305. [**] *Ibid.* lix. 199.

[††] *Phil. Mag.* 1855. [‡‡] *Zeitschrift für Chemie*, 1866, p. 21.

On this second point a word or two will at once explain my meaning.

Warren accepts 80·1° as the boiling point of benzole; the identity of benzole from various sources is generally allowed. Now cumole obtained from cuminic acid boils, according to Warren, at 151·1°. I do not suppose that any one has good grounds for controverting the statement that benzole from benzoic acid, and boiling at 80·1°, and cumole from cuminic acid, and boiling at 151·2°, are members of the same series. Both are derived by the same process from homologous acids.* If we compare, as we may fairly do, their boiling points, to what difference for C_3H_6 does it point? Not to Mr. Warren's 30°, but to the smaller difference of 23° or 24°.

		Difference for $3CH_2$.
Benzole	69·1°	71·1°—23·7° × 3.
Cumole...............	151·2°	

Nearly all the difficulties suggested by the divergent results of different experimenters will be explicable if the existence in various coal naphthas of two or more series of benzolic homologues be granted. Warren, while readily admitting the occurrence of several similar isomeric series in different kinds of natural and artificial paraffin oils, seems to deny the occurrence of analogous variations in different samples of coal naphtha.

II. The Parabenzole Series.—In treating of the boiling points of the benzole series, I have necessarily referred to the existence of an isomeric series of hydrocarbons. Warren makes the very obvious suggestion that parabenzole is a mere mixture of benzole and toluole. I originally entertained the same notion myself, till a close examination of the physical and chemical properties of this liquid convinced me that it was untenable. I have sufficiently combated this idea in my notes on the parabenzole series published in the volume of the Chemical News before cited.

The existence of an isomeric benzole has received strong confirmation in some experiments by Fittig, the occasional collaborateur of Beilstein. He finds a hydrocarbon, in some points closely resembling parabenzole, in the products of the distillation of camphor with zinc chloride.

I will add one fact about the occurrence of parabenzole in coal naphtha which may serve to account for its non-detection by some observers. My experiments were made with fractions boiling at about 100°, and which had been collected during some years in numerous distillations of large quantities of the naphtha; yet a few ounces only of parabenzole was the ultimate product.

III. Reactions, and New Compounds and Processes.—The following are the chief points under this head to which I would wish to call attention. I claim as my own the following processes, methods, &c., most of which have been republished of late years as new discoveries or adopted without acknowledgment:—

1. The purification of hydrocarbons by distillation from sodium. (*Phil. Mag.* 1855.)
2. The production of the so-called "nitro-sulpho" acids by dissolving nitrobenzole, &c., in oil of vitriol or Nordhausen acid. (*Ibid.* 1855.)
3. The oxidation, by chromic acid, of nitrotoluole and its homologues into β nitrobenzoic acid, etc. (*Ibid.* 1861.) In 1862 I showed in the International Exhibition a specimen of this β nitrobenzoic acid thus labelled and distinguished from the ordinary α acid.
4. The oxidation of sulpho-toluenylic acid into benzoic acid by the action of the chromic acid.
5. The probability of the existence of methyl-benzole, &c. (*Phil. Mag.* 1859.)*

I trust that none of the foregoing remarks will lead any one to suppose that I underrate Mr. Warren's own researches. While demurring to much of his criticism, I look upon some parts of his experimental investigation as monuments of patient labour and as most valuable additions to our accurate knowledge of a complex subject. And, with reference of Professor Beilstein, I am far from denying that in some particulars his criticism, though rough, is just. For example, in my earlier papers I rested my convictions of the identity of some of the benzolic homologues from various sources upon experimental grounds, which have since been shown to be inadequate. Then, again, I stated in 1855 (*loc. cit.*) that coal-tar toluole, by digestion with sodium, yields two hydrocarbons differing in boiling point—an asssertion which is erroneous if pure toluole be operated upon, but which I myself corrected in 1857, explaining, at the same time, the real facts of the case. In the other charges which Professor Beilstein brings against my statements, I believe him to be unjust, and that he is so in some instances may be proved very readily. He states† that I assert xylole to be changed into benzoic acid by oxidation with chromic acid. The fact is that, on the contrary, I offered no experiments of my own on the subject, but simply stated‡—"I have *not* yet experimented with the *xylole* series!" The last matter in which my reputation is assailed is as to the identity of the α and β nitrobenzoic (nitrobenzoic and nitrodracylic) acids; as already stated, I affirmed them to be different as early as 1862. But Professor Beilstein is not content with charging me with the terrible mistake of confounding two bodies which every one knows are actually *isomeric*, for he does me the further kindness of recommending *my* process as the best method of preparing the β nitrobenzoic acid! He does this without acknowledgment in his attack on another chemist.§.

On the Separation of Tin and Arsenic,
by Professor Wöhler.

This method is based upon the solubility of sulphide of arsenic in bisulphite of potash, which does not dissolve sulphide of tin. The mass, oxidised by nitric acid, is allowed to digest with sulphur and caustic potash till solution is complete (or till the formation of a metallic oxysulphide, which is separated by filtration). The liquid, treated by excess of sulphurous acid, is allowed to rest for some time, and is then evaporated till two-thirds of the water and all the sulphurous acid have gone off. Filter off the sulphide of tin, and wash it, not with water, which must not be used here, but with a concentrated solution of chloride of sodium. This may be removed from the precipitate by means of a slightly acid solution of acetate of ammonia, but the liquor so obtained must not be added to the washing waters charged with salt. The sulphide of tin, when dried, may be converted into oxide of tin by roasting in contact with air. The arsenic which the liquid contains in the state of arsenious acid may be precipitated by a current of sulphuretted hydrogen.

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* The fusing points of the acids of the benzoic series demand further investigation.

* *Vide* Fittig and Tollens, *Ann. Chem. Pharm.* cxxxi. 304.

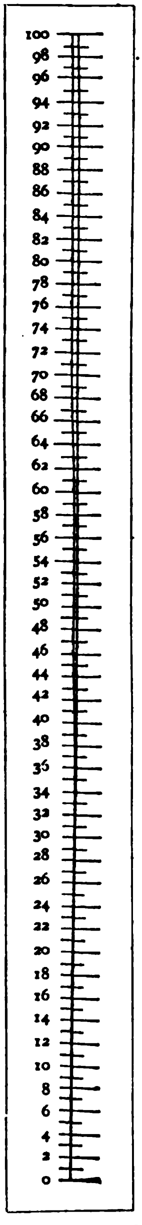
† *Zeitschrift für Chemie*, 1865, p. 670.

‡ *Phil. Mag.* March, 1861.

§ Chem. News, 1866, p. 61.

On the Application of the Blowpipe to the Quantitative Determination or Assay of certain Metals, by DAVID FORBES, *F.R.S., etc.**

Determination of the Weight of the Silver Globule obtained on Cupellation.—As the amount of lead which can, by the method before described, be conveniently cupelled before the blowpipe, is necessarily limited, the silver globule which remains upon the bone-ash surface of the cupel at the end of the operation is, when substances poor in silver have been examined, frequently so very minute that its weight could not be determined with correctness by the most delicate balances in general use.



The blowpipe balance employed by the author turns readily with one-thousandth of a grain, but could not be used for determining weights below that amount.

Globules of silver of far less weight than one-thousandth are distinctly visible to the naked eye—a circumstance which induced Harkort to invent a volumetrical scale based upon the measurement of the diameters of the globules, which scale in practice has been found of very great utility in the blowpipe assay of silver.

The scale for this purpose which is employed by the author is shown in full size in the annexed woodcut.

This figure represents a small strip of highly polished ivory about 6¼ inches long, ¾ inch broad, and ⅛ inch in thickness, on which are drawn, by an extremely fine point, two very fine and distinct lines emanating from the lower or zero point, and diverging upwards until, at the distance of exactly six English standard inches, they are precisely four-hundredth parts of an inch apart. This distance (six inches) is, as shown in woodcut, divided into 100 equal parts by cross lines numbered in accordance from zero upwards. It is now evident, if a small globule of silver be placed in the space between these two lines, using a magnifying glass to assist the eye in moving it up or down until the diameter of globule is exactly contained within the lines themselves, that we have at once a means of estimating the diameter of the globule itself, and therefrom are enabled to calculate its weight.

As the silver globules which cool upon the surface of the bone-ash cupel are not true spheres, but are considerably flattened on the lower surface, where they touch and rest upon the cupel, it follows that the weight of globules corresponding in diameter to the extent of divergence at the different degrees of the scale cannot be calculated directly from their diameters as spheres, but require to have their actual weight experimentally determined in the same manner as employed by Plattner.

The table here appended has been calculated by the author, and in one column shows the diameter in English inches corresponding to each number or degree of the scale itself, and in the two next columns the respective weights of the flattened spheres which correspond to each degree or diameter; for convenience these weights are given in the different columns in decimals, both of English grains and of French grammes.

These weights are calculated from the following data found as the average result of several very careful and closely approximating assays which showed that globules of silver exactly corresponding to No. 95 on this scale, or 0·038 inch in diameter, possessed a weight of 0·0475573 grains or 0·003079 grammes. From this the respective weights of all the other numbers or degrees on this scale were calculated, on the principle that solids were to one another in the ratio of the cubes of their diameters. This mode of calculation is not, however, absolutely correct in principle, for the amount of flattening of the under surface of the globule diminishes in reality with the decreasing volume of the globule. In actual practice, however, this difference may be assumed to be so small that it may be neglected without injury to the correctness of the results.

The smaller the diameter of the globule, the less will be the difference or variation in weight in descending the degrees of this scale, since the globules themselves vary in weight with the cubes of their diameters; for this reason, also, all such globules as come within the scope of the balance employed should be weighed in preference to being measured, and this scale should be regarded as more specially applicable to the smaller globules beyond the reach of the balance.

No. on scale.	Greatest diameter in inches.	Weight of globule in grains.	Weight of globule in grammes.
1	0·0004	0·00000005	0·000000003
2	0.0008	0·00000044	0·000000028
3	0·0012	0·00000149	0·000000096
4	0·0016	0·00000355	0·000000229
5	0 0020	0·0000069	0·00000044
6	0·0024	0·0000119	0·00000077
7	0·0028	0·0000190	0·00000120
8	0·0032	0·0000284	0·00000184
9	0·0036	0·0000403	0·00000262
10	0·0040	0·0000554	0·00000359
11	0·0044	0·0000736	0·00000478
12	0·0048	0·0000958	0·00000620
13	0·0052	0·0001218	0·00000789
14	0·0056	0·0001522	0·00000985
15	0·0060	0·0001872	0·00001203
16	0·0064	0·0002272	0·00001471
17	0·0068	0·0002725	0·00001764
18	0·0072	0·0003234	0 00002094
19	0·0076	0·0003804	0·00002463
20	0·0080	0·0004437	0·00002872
21	0·0084	0·0005137	0·00003327
22	0·0088	0·0005906	0·00003823
23	0·0092	0·0006748	0·00004367
24	0·0096	0·0007668	0·00004964
25	0·0100	0·0008667	0·00005611
26	0·0104	0·0009749	0·00006311
27	0·0108	0·0010918	0·00007068
28	0·0112	0·0012176	0·00007883
29	0·0116	0·0013528	0·00008758
30	0·0120	0 0014976	0·00009696
31	0·0124	0·0016524	0·00010698
32	0·0128	0·0018176	0·00011677
33	0·0132	0·0019934	0·00012817
34	0·0136	0·0021801	0·00014114
35	0·0140	0·0023786	0·00015397
36	0 0144	0·0025879	0·00016755

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* Communicated by the author.

No. on scale.	Greatest diameter in inches.	Weight of globule in grains.	Weight of globule in grammes.
37	0·0148	0·0028097	0·00018190
38	0·0152	0·0030437	0·00019705
39	0·0156	0·0032903	0·00021302
40	0·0160	0·0035550	0·00022983
41	0·0164	0·0038230	0·00024751
42	0·0168	0·0041096	0·00026606
43	0·0172	0·0044111	0·00028553
44	0·0176	0·0047250	0·00030589
45	0·0180	0·0050546	0·00032725
46	0·0184	0·0053991	0·00034955
47	0·0188	0·0057590	0·00037285
48	0·0192	0·0061344	0·00039716
49	0·0196	0·0065258	0·00042250
50	0·0200	0·0069335	0·00044890
51	0·0204	0·0073581	0·00047638
52	0·0208	0·0077799	0·00050495
53	0·0212	0·0082580	0·00053464
54	0·0216	0·0087343[8]	0·00056549
55	0·0220	0·00922854	0·00059748
56	0·0224	0·0097412	0·00063067
57	0·0228	0·0102725	0·00066506
58	0·0232	0·0108228	0·00070021
59	0·0236	0·0113922	0·00073753
60	0·0240	0·0119815	0·00077570
61	0·0244	0·0125901	0·00081513
62	0·0248	0·0132119	0·00085588
63	0·0252	0·0138901	0·00089797
64	0·0256	0·0145440	0·00094141
65	0·0260	0·0152311	0·00098623
66	0·0264	0·0159472	0·00103245
67	0·0268	0·0166828	0·00108010
68	0·0272	0·0174414	0·00112918
69	0·0276	0·0182220	0·00117974
70	0·0280	0·0190256	0·00123177
71	0·0284	0·0198529	0·00128535
72	0·0288	0·0207035	0·00134041
73	0·0292	0·0215782	0·00139704
74	0·0296	0·0224469	0·00145525
75	0·0300	0·0234010	0·00151504
76	0·0304	0·0243496	0·00157645
77	0·0308	0·0253224	0·00163950
78	0·0312	0·0263228	0·00170422
79	0·0316	0·0223484	0·00177060
80	0·0320	0·0284000	0·00183869
81	0·0324	0·0294789	0·00190852
82	0·0328	0·0305838	0·00198008
83	0·0332	0·0317162	0·00205340
84	0·0336	0·0328768	0·00212851
85	0·0340	0·0340649	0·00220549
86	0·0344	0·0349739	0·00228400
87	0·0348	0·0364422	0·00235938
88	0·0352	0·0378008	0·00244730
89	0·0356	0·0390138	0·00253168
90	0·0360	0·0404368	0·00261797
91	0·0364	0·0417943	0·00270790
92	0·0368	0·0431930	0·00279642
93	0·0372	0·0446162	0·00288860
94	0·0376	0·0460718	0·00298276
95	0·0380	0·0475573	0·00307900
96	0·0384	0·0465239	0·00317728
97	0·0388	0·0506249	0·00327759
98	0·0392	0·0522069	0·00338020
99	0·0396	0 0538215	0·00348452
100	0·0400	0·0554688	0·00359138

Chemical Composition of the Mud from the Streets of the City of London, by Dr. Letheby.*

During the last twelve months many analyses have been made in my laboratory of the mud from the City thoroughfares, with the view of ascertaining the relative proportions of horsedung to the matter from the abraded stones and iron of wheels and horseshoes; and the results show that the former material averages about 57 per cent. of the dried mud.

It was first ascertained that the amount of moisture in the street mud varies to a considerable extent, according to the state of the weather, but it is rarely less than 35·3 per cent. of the weight of the mud in the driest weather—the average of ordinary weather being 48·5 per cent.—and in wet weather it ranges from 70 to 90 per cent.

After all moisture has been driven off from the mud by exposing it for many hours to a temperature of from 266° to 300° of Fahrenheit, the relative proportions of organic and mineral matters were as follows; and for comparison the composition of well-dried fresh horsedung and common farmyard dung has also been determined:—

Composition of Mud from the Stone-paved Streets of the City, compared with fresh Horsedung and Farmyard Dung dried at 300° Fahr.

Constituents.	Fresh Horse-Dung.	Farm-yard Dung.	Mud from Stone paved Streets. Maximum Organic.	 Minimum Organic.	 Average.
Organic matter .	82·7	69·9	58·2	20·5	47·2
Mineral matter .	17·3	30·1	41·8	79·5	52·8
	100·0	100·0	100·0	100·0	100·0

The largest amount of mineral matter is always found in the mud in wet weather, when the abrasion of the stone and iron is greatest. At that time it may amount to 79 per cent. of the weight of the dry mud; whereas, in dry weather, it does not exceed 42 per cent. Taking the average of all weathers, the amount of horsedung to abraded matters is about 57 per cent.

The exact proportions of stony and ferruginous matters in the mineral constituents of the mud have not been determined; but from the deep red colour of the ash obtaining by incinerating the mud, there can be no doubt that the proportion of iron in the mud is very large; and it must have been derived from the wheels and horseshoes abraded by the stones.

In the case of the wood pavement, the amount of organic matter in the dried mud was larger than in the case of the stone pavement. It amounted, in fact, to about 60 per cent., and the ash was highly ferruginous.

Very probably the average proportions of horsedung, abraded stones, and abraded iron in the mud from the stone-paved thoroughfares is about as follows: —

Horsedung	57
Abraded stone	30
Abraded iron	13
	100

The mud was in every case so finely comminuted that it floated freely away in a stream of water, and the inference is that it would not subside to any great extent in a sewer with a moderate flow of water.

On the Invariableness between the Ratios of the Weights of the Elements forming Chemical Compounds, by J. S. Stas.

I have already stated that the laws of chemical proportions are not mathematically proved. Indeed, the attentive examination of all the facts in the science

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* Communicated by the author.

bearing upon this subject has convinced me that chemists rely rather upon the constancy of the composition of compounds, than upon a rigorous demonstration of Wenzel's law, and of Dalton's hypothesis, known as the *law of multiple proportions.*

I shall not here examine Gay-Lussac's celebrated article "*On the Mutual Combinations of Gases*,"[*] nor Wollaston's article "*Upon the Carbonates and the Oxalates*,"[†] which, since the commencement of this century, have served as the experimental basis of Dalton's hypothesis. It is now allowable to affirm *a priori* that Gay-Lussac has not succeeded in proving his *law of volume* as a mathematical law; for, in fact, it can only be correct within certain limits, since the law of the compressibility of elastic fluids and the law of the expansion of gases by heat are themselves only approximately correct. The experiments of Wollaston on the relations of oxalic acid and potash in the neutral and acid oxalates were performed upon such a small scale that it is impossible to tell from them if the law of multiple proportions is a mathematical or an approximate law. Even admitting the quantities to have been sufficient, the principle relied on by the famous English chemist—viz., *neutrality measured by colouring matter*—is but an hypothesis whose basis requires *à priori* proof.

All the analyses and the syntheses yet performed are quite powerless to prove the law of definite proportions to be a mathematical law. For, whatever skill a chemist may possess, it is impossible for him to perform an analysis or synthesis without committing an error of observation. Now, hitherto, nothing has proved that the differences found in certain analyses between experiment and calculation must be *wholly* owing to error in the operation; a certain part may be due to the inexactitude of the law of definite proportions. Then, again, if the existing analyses and syntheses could give an exact solution of this problem, all chemists would agree as to the atomic weights of a large number of bodies, and Prout's hypothesis would be definitely decided. The diversity which has long existed as to certain atomic weights proves better than any reasoning that an absolute proof of the law of definite proportions is still wanting.

The constancy of the composition of stable compounds being admitted, what is required to resolve this problem? It must be proved that in binary and ternary bodies, for example, having each *two elements in common*, the common elements exist with *invariably the same ratios as to weight.* Thus in two bodies, AB and ABC, the ratios of the weights of A to B should be just the same in AB as in ABC.

It may be seen that the solution of this problem may be made independently of analysis, for to resolve the problem it is merely required to discover if the ternary bodies may be reduced to binary bodies without any fraction, however small, of either of the common elements becoming free; or, inversely, if the binary may be transformed into ternary substances without any fraction of one of the elements of the binary compound remaining unincluded in the ternary compound.

Among all the facts composing the science of chemistry, there is not one which entirely satisfies these conditions. The transformation of the chlorate and bromate of potassium into the chloride and bromide under the influence of heat is the nearest approach of any. Chemists who have closely studied this decomposition of the chlorate have observed traces of chlorine only in the disengaged oxygen. In the analysis that M. Marignac made of this salt, he attempted to measure these traces of chlorine.[*]

For my part I have tried every means for fixing the chlorine on red-hot silver.[†]

In the hope of finding in the transformation of chlorate and bromate of potassium into chloride and bromide a solution of the problem in question, I made new and lengthy trials, but they were all fruitless. I always found traces of chlorine or bromine, although I had taken every conceivable precaution to deprive the chlorate and bromate of the infinitesimal quantities of silica or of foreign metals which they retain with the most persistent tenacity. I was not more fortunate with the perchlorate of potassium; however slowly I decomposed it by heat, and whatever pains I took to purify it, the oxygen it gave off by the action of heat was always, towards the last, contaminated by traces of chlorine.

Having failed in these attempts, I directed my researches in another direction. It is well known that sulphurous anhydride transforms into iodide of silver the iodate of that metal suspended in water, at the same time that it becomes sulphuric acid. I have shown that, under the same influence, bromate becomes bromide, and chlorate chloride, of silver. The absolute insolubility of iodide, bromide, and chloride of silver in water acidulated with sulphuric acid, and the possibility of recognising in a liquid a ten-millionth part of silver, of iodine, of bromine, or of chlorine, are exceptionally favourable conditions for submitting the law of definite proportions to a decisive test. For this purpose I undertook the researches I am about to describe.

These experiments were very difficult of execution There were two obstacles I had to contend with; the first, which is readily foreseen, consists in the great difficulty of procuring salts of silver sufficiently pure to be submitted to so rigorous a test; the other, which was quite unforeseen, is due to the property possessed by sulphurous acid of changing under the influence of still obscure, or rather completely unknown, causes, and of having, in its changed condition, peculiarities opposed to those which it possessed before undergoing this change.

It is of course impossible for me to give here any idea of the difficulties I met with in the preparation of *pure iodate, bromate, and chlorate of silver*; they were naturally different for each of their salts. As the different steps which I took will prove very instructive to those who would care to repeat these researches, I will relate very fully the means to which I had recourse to obtain these salts, and to ascertain their degree of purity.

On the Microscopical Examination of Coal Ash or Dust from the Flue of a Furnace, by J. B. Dancer, *F.R.A.S.*[‡]

When coal is burnt in a furnace to which atmospheric air has free access, a portion is converted into gaseous and volatile matter; and the incombustible substance which remains is the ash. The amount of ash in coals from different localities is very variable; it is said to range from 1 to 35 per cent. The ash or dust which is

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[*] *Mémoires et de Physique de Chimie de la Société d'Arcueil*, vol. ii. p. 207.

[†] *Philosophical Transactions of the Royal Society.* 1808, 1st part, p. 96.

[*] *Bibliothèque Universelle de Genève*, vol xl. p. 148.

[†] *Recherches sur les Rapports Réciproques des Poids Atomiques: "Analysis of Chlorate of Potash."*

[‡] Read before the Manchester Literary and Philosophical Society.

the subject of this paper was collected from the flue of my steam boiler furnace, in which common engine coal is used as fuel. This coal leaves a considerable amount of incombustible matter. A specimen of the dust is now before you; it is of a reddish-brown colour, and free from soot or carbonaceous particles.* When this dust is examined under the microscope with a power of 40 or 50 diameters, it is found to consist of ferruginous matter and crystallised substances, some particles transparent, others white and red. It contains also a number of curious-looking objects, which vary considerably in size and colour; the majority of these bodies are spherical, and when separated from the irregularly shaped particles forming the bulk of the dust they become interesting objects for the microscope. I shall confine my remarks more especially to these globular bodies. Some of these are as perfect in form as the most carefully turned billiard balls, and have a brilliant polish. The various colours which these globules exhibit give additional interest to their examination. Some are transparant crystal spheres, others are opaque white, many are yellow and brown, and variegated like polished agates or carnelian of different shades. The most abundant of the highly polished balls are black; there are others which look like rusty cannon balls—some of these have an aperture in them like a bombshell, and many are perforated in all directions. To obtain these objects the dust should be washed in a bowl and all the lightest particles allowed to float away; the remainder consists of fragmentary crystalline and ferruginous substances; mixed with these are the polished balls described, which, under the microscope, by a brilliant reflected light, look like little gems. To separate the spherical bodies from the irregular ones, it is only necessary to sprinkle some of this material on an inclined glass plate, and by gentle vibration the balls roll down, and can thus be collected. Having satisfied ourselves with the examination under the microscope, it is natural that we should desire to know more about these novel objects. What is their elementary constitution? Why are they spherical? How do they get into the flue? I have not attempted a chemical analysis of these minute bodies, many of which are less than the 100th part of an inch in diameter. I can only therefore offer an opinion as to their probable constitution, judging from what is known of the chemical analysis of coal ash, and from the appearance they present under the microscope. Referring to the chemical analysis of coal ash, we find that it sometimes contains silica, magnesia, alumina, sesquioxide of iron, lime, soda, potash, sulphate of calcium, anhydrous sulphuric acid, anhydrous phosphoric acid, sulphur, and sometimes traces of copper and lead. The vegetable origin of coal is now generally admitted, and doubtless some of the substances I have just named have been taken up by the coal plants, whilst other portions may have collected in the locality where the coal was formed. As this is not immediately connected with our present inquiry, I proceed to speculate as to the constitution of these globular bodies. The transparent spheres I imagine to be silicates of soda or potash; the opaque white are most likely silicate of soda or potash combined with lime and alumina; the yellow and brown are silicates coloured by iron in different proportions. The black globes are not all alike in composition; some of these are silicates coloured by carbon, others are iron balls coated externally with a silicate. Many of these rusty cannon balls are probably ferrous oxide formed by the action of heat on the iron pyrites in the coal. There are also balls of black magnetic oxide; the perforated shells are probably ferrous sulphides. The globular form of these bodies suggests that they have been thrown off in scintillations, such as are seen during the combustion of iron in oxygen gas, and whilst in a fluid state they assume a spheroidal form. They are carried by the draught into the flue, and being of greater specific gravity than the carbonaceous matter forming the smoke, they fall before the current of air has reached the chimney. Some of the dust has been a considerable time in the flue, exposed to the intensely heated circulating flame; the reducing action of this would probably convert some of the oxide into metallic iron. Many of these balls have the appearance of reduced oxides. The flue dust contains a larger amount of ferruginous matter than can be accounted for by the analysis of coal ash. I think the surplus may be regarded as representing the wear and tear of the iron work about the furnace, such as fire bars, boiler plates, &c. The brick work and cement about the boiler and flues may also supply some of the silica, alumina, and iron for these balls, numbers of which are merely thin shells. The movements of these objects, caused by the approach of a magnet under the stage of the microscope, are somewhat amusing, and it is at times startling to see the crystalline objects, both spherical and irregular, exhibit magnetic attraction; probably they contain particles of iron imbedded in them; if they do not, may we not imagine that there is some magnetic compound in which the crystalline matter predominates? When we consider the accidental condition under which this matter has combined, it is just possible that some new molecular arrangement or combination of elements may have taken place. It is very probable that many of these polished balls are much more complex in their elementary constitution than I have stated. They are, in fact, a kind of glass, and many of them merely bulbs. Pelouze states that glass is probably an indefinite mixture of definite silicates. Glass, containing small quantities of ferrous oxides and sodic sulphates, when exposed to sunlight, becomes yellow, and possibly some of these balls may have changed in colour since they came from the flue. Hydrochloric and nitric acid exert very little action on the ferruginous globes; this may be due in some measure to the high temperature at which the oxides have been formed; in other cases they are no doubt protected by an external coating of some silicate. It would require much time and patience to collect a sufficient number of each kind of these minute objects for a chemical analysis; but the spectroscope might probably assist in revealing their constitution. When time permits I hope to resume the subject.

On the Supposed Nature of Air prior to the Discovery of Oxygen, by George Farrer Rodwell, *F.C.S.*

XVI. J. C. Sturm.— John Christopher Sturm,* although not very notable in the science of pneumatics as an original investigator, did much to propagate the discoveries of others, and to induce pneumatical research. He has been called "*le restaurateur des sciences physiques en Allemagne*," but we cannot admit this designation, both on account of the inaccuracy of the expression, and because it behoves us to remember that Otto von Guericke, Gaspar Schottus, and Atha-

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* My attention was drawn to this subject by Mr. Johnson, of Wigan, in November, 1866.

* Born at Rippolstein in Bavaria in 1635, died at Altorf in Franconia in 1703.

nasius Kircher were among his contemporaries, and with him assisted in the introduction of a taste for physical science into Germany. In regard to the inaccuracy of the above expression, we may remark that the term "*restaurateur* des sciences physiques" is incapable of being applied to any individual of the period in which Sturm lived. A *restorer* is one who brings back, or induces the return of, that which existed at some former time, but the physical sciences had no pre-existence, and therefore could not be restored. We may speak of "the restoration of learning," and of the "Renaissance period of literature," because we know that among the Greeks and Romans a noble and polite literature had arisen at a long prior period, and had been sustained by some of the most sovereign intellects which the world has beheld. We may speak of Petrarch as the restorer of *belles-lettres*, and of Cosmo de' Medici and Marsilio Ficino as the restorers of a taste for the Platonic philosophy, because an elegant and refined erudition and a love for the Platonic philosophy had pre-existed, and had for a time been extinguished. But this was not the case with physical science; it had no Renaissance period. In the age of Sturm it was new born into the world; hence he cannot be called its *restaurateur* in Germany, but may rather be regarded as an indefatigable and assiduous nurse, who helped to rear the young child, and to give it a good start in life.

We have no wish to depreciate Sturm, but rather to show that he is not entitled to the extravagant praise which one of his biographers (who probably committed this error of judgment "quoniam dilexit multum") awards to him. We do not think that the services of Sturm have ever been sufficiently recognised by the world at large, although they were undoubtedly well recognised in his own country during his lifetime, and in the succeeding period. Apinus speaks of him as "hic μαθηματικοτατος et per omnem Europam longe celeberrimus naturalis scientiæ Doctor;"* while Brucker at a somewhat later period designates him as "vir longe doctissimus et tum veteris philosophiæ, tum recentioris callentissimus, quique experientiam naturali philosophiæ optimo consilio junxerat, ut ex ejus Collegio Curioso constat."†

The "Collegium Curiosum" alluded to in the last sentence was a scientific society founded by Sturm in 1672 on the plan of the Accademia del Cimento. It originally consisted of twenty members, and it continued to flourish long after the death of its founder. The early labours of the society were devoted to the repetition (and often modification) of the most notable experiments of the day, and to the discussion of the results. Two volumes of proceedings were published by Sturm, the first in 1676, and the second in 1685; in them we find a collection of the principal pneumatic experiments of the Florentine academicians, and of Pascal, Boyle, Hooke, Otto von Guericke, and Huygens. The volume, which appeared in 1676, is entitled, "*Collegium Experimentale sive Curiosum. In quo primaria hujus seculi inventa et experimenta physico-mathematica . . . . . per ultimum quadrimestre anni* 1672. *Viginti naturæ scrutatoribus*, &c." The "*Programma Invitatorium*" is dated June 3,1672; and Sturm therein urges that inasmuch as the age of disputatious philosophy had given way to that of experimental philosophy, and as, moreover, scientific societies had been founded in Florence, London, Paris, Rome, and Venice, it would seem to be desirable to found one in Germany, for the attainment of which end he requests the collaboration of the learned.

The dedication of the work (to Antonio Magliabecchio, Duke of Tuscany) is a good example of the turgid and florid Latinity which prevailed in the dedications both of that and of a much later period. Then follows a list of the members, and a preface by Sturm describing the object and aims of the society, and the first chapter commences with an account of the diving-bell "recens inventa,"* and described by George Sinclair† in his "*Ars Magna et Nova Gravitatis et Levitatis.*" Next follow chapters relating to the camera obscura, the Torricellian experiment, the gravity of air, the water barometer, the mechanical powers, the thermometer, the air-pump, the microscope, the telescope, &c. In the 1685 volume (which is a continuation of the above), Sturm describes a modified form of Boyle's air-pump, which he had invented; but although by its means a partial vacuum was rapidly obtained, a good vacuum was impossible, because a spring was employed to close the valve, which allowed air to pass from the receiver to the pump barrel; but when the air within the receiver was much rarefied, the force exercised by its expansion was incompetent to overcome the pressure of the spring, and the receiver ceased to be further exhausted.

Altogether these two volumes would seem to constitute a nearer approach to a text-book of the very sparse (but rapidly increasing) physics of the period, than any preceding work. In addition to the above Sturm was the author of a large number of works, chiefly mathematical and physical.

Sturm was Professor of Mathematics and Natural Philosophy in the University of Altorff‡ for thirty-four years, and in that capacity possessed, and exercised, considerable influence as a propagandist of the new philosophy. His own studies had been pursued in the Universities of Jena and Leyden, in the former of which the Aristotelian philosophy prevailed, and in the latter the philosophy of Descartes. He commenced his career as a rigid Aristotelian, and afterwards became a syncretist,§ and endeavoured to unite the philosophy of

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* "Vitæ Professorum Philosophiæ qui a condita Academia Altorfina." Norimbergæ, 1728.

† "Institutiones Historiæ Philosophicæ." Lipsiæ, 1756.

* The invention of the diving-bell, is however, wrongly attributed to Sinclair, for it is mentioned by Bacon in the "Novum Organum," and by the Italian mathematician Nicolas Tartaglia.

† At that time Professor of Natural Philosophy in the University of Glasgow.

‡ This must not be confounded with its homonyme, the capital of the Swiss canton of Uri, which is a town of greater size, and in the present day of greater importance. The Altorff alluded to above is in Franconia, a few miles distant from, and under the jurisdiction of, Nuremberg. It is mentioned as early as the year 912, and in 1823 contained 1800 inhabitants. The University was founded in 1579 according to Lloyd ("Dictionarium Historicum, Geographicum, et Poeticum," 1686) and Hoffmann ("Lexicon Universale," 1698); and in 1575, according to Seally ("Geographical Dictionary," 1787) and Worcester ("Geogr. Dict.," 1823; but we must accept the latter date, because Apinus ("Vitæ Proff. Phil., etc.") gives the life of a professor who is described as holding office in the University in 1575. Seally in 1787 writes as follows:—"The fine University structure consists of a building three stories high, and contains in it a valuable library, an anatomical theatre, and a chemical elaboratory. In the main body of it is an observatory. . . It now contains about 200 students." In 1809 it was incorporated with the University of Erlangen, and it thus happens that one of the earliest homes of experimental philosophy in Germany has ceased to be recognised or noticed.

§ From συγκρινω to conjoin, cement together, applied to those who endeavour to unite the opinions of diverse sects into one system, and thus to produce a comprehensive coalition system, and a consequent coalition of sects. Brucker has well observed—"Parum utilitatis attulisse philosophiæ hoc syncretismi studium, res ipsa et eventus loquitur. Frustra enim philosophiæ veræ auctoritas hominis aut sectæ præfigitur, quæ non, quis dixerit, vel quid dixerint philosophorum principes, quærit, nec curat, utrum eorum effata salvari queant, sed quid verum sit meditatione duce inquirit."

Aristotle with that of Descartes, so as to produce a system participating in the dominant qualities of each. In this, however, he failed, and he then abandoned syncretism, and became essentially and completely a Baconian philosopher. In his thirty-seventh year we find him asserting that for a right investigation of nature the philosophy of Aristotle is not to be followed, neither that of Descartes, but the principles of truth and reason founded upon experimental proof.

Sturm was one of the first professors in Europe—"physicam scientiam juxta cum mathematicis publice docere." What a mighty revolution had occurred in this century in regard to the tolerance of individuality of opinion and the dissemination of free philosophical thought! Here we have the Baconian philosophy introduced into the very heart of a centre of learning without opposition, and with a simultaneous and necessary subversion of the Aristotelian philosophy. How well this contrasts with the intolerance exhibited by the Universities of Pisa and Padua (always noted for their strong adherence to Aristotle)—an intolerance which drove from the former University its greatest ornament and glory, Galileo!

It is not in the capacity of a great discoverer in science that we have here spoken of Sturm; it is not because he exercised a markedly direct influence either upon pneumatics or pneumatic chemistry; but because, by indirect means, he did much for the furtherance of those branches of science; because he helped to induce a permanent and wide-spreading taste for physical science; finally, because he laboured long and lovingly, and in the true spirit of the Baconian philosophy.

The University which he adorned—"ubi mathesin et physicam summa cum laude et applausu docuit"—has passed away, and with it the memory of the man. It would seem to be forgotten that he established in the heart of Europe a centre for the propagation of experimental philosophy; it would seem to be forgotten how deeply he stamped his name upon the times in which he lived, and how great has been the after-influence:—the letters are all but obliterated, and it has become necessary to grave them more deeply before they quite disappear.

TECHNICAL CHEMISTRY.

*On the Manufacture of Caramel Brown,** *by* Thos. Sherlock.

Caramel brown may be prepared in a variety of ways from glucose, molasses, or cane sugar. The following process gives a uniform and perfectly satisfactory article, and after having manufactured large quantities of the colour and tried several other processes, I have come to the conclusion that this is the best.

Provide an iron pan capable of holding twenty imperial gallons. Provide also an iron paddle or stirrer, flattened out broad at the end, about four feet long, and made light enough to be handled easily. Have also close at hand three or four gallons of clean boiling water. Set the pan on a ring over a fireplace, and put in half a hundred weight (56 lbs.) of good ordinary raw sugar. It is mistaken economy to use the very commonest brown sugar. Light a fire under the pan, and as it burns up stir the sugar about with the paddle. The sugar gradually melts, giving out puffs of vapour, and finally becomes a viscid liquid of a light brown colour. This is the first stage in the process. Only a moderate heat is required, and the melting should not be hurried. Now increase the heat gradually, stirring briskly and constantly. The liquid will become thinner and darker in colour, and at length begin to boil vigorously and rise up in the pan. The whole secret consists in the management of this part of the process, and minute attention should be paid to the following simple directions. Allow the melted mass to rise up till the pan is half full; then open the fire-door, throw water on the fire, and pull it out quickly. This should be done by a second person, the actual operator stirring sharply with the paddle to keep the mass in the pan. If the fire be drawn without first throwing water on it, the contents of the pan will inevitably boil over, and there will be a corresponding loss of product. Continue the stirring till the boiling subsides, and the dark brown mass lies quiet at the bottom of the pan. If a little be now dropped on to a cold plate or piece of metal, it will solidify to a brittle lump, of a clear rich brown colour, showing that the operation has succeeded. All that now remains is to add sufficient water to bring the mass to the desired consistence. The water must be boiling when added, and in very small quantities at a time. There is a considerable rush of steam as the first portions of water are stirred in, and care must be taken in using the paddle to stand clear of the hot particles projected from the pan; but after a few additions of water all this subsides, and the water may be added more freely.

The finished colour is usually sent out, either as a stiff paste-like extract, in which condition it is used by saddlers, curriers, &c., for browning certain kinds of leather, or as a syrup more or less thick. In this last form it is used for colouring vinegar, spirits, gravies, and many other liquids, and is well known in the drug trade as "color fuscus." If the stiff form be required, about a gallon of water will be sufficient, and in this case the product should be got out while hot, and put into stone-ware jars, previously heated, and standing on a piece of wood.

Fifty-six pounds of raw sugar should yield at least 60 lbs. of the stiff colour, and proportionately more of the thinner kind, and when cold should dissolve readily in water, giving a clear brown solution, without deposit or turbidity.

The causes of failure in the manufacture may be either a deficiency or an excess of heat.

If the heat used be insufficient, some of the sugar remains imperfectly converted, and a muddy dirty-looking product is the result. On the other hand, if the heat used be excessive (strong heat is not required in any part of the process), the mass becomes black, granular, and insoluble in water—in fact, burnt and useless.

*Note on the Bituminous Schists of Vagnas (Ardèche).** *by* M. L. Simonin.

This bed of schist, now worked, merits notice. It is more a sort of tertiary boghead than a true schist. Its texture is compact and massive, like that of carbonised and compressed peat. Its origin from peat is further revealed by the numerous very delicate vegetable filaments apparent to the naked eye in the rock.

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* Communicated by the author.

* Communicated by the author.

The schist, distilled in a revolving retort, gives about 10 per cent. by volume of raw paraffin oil. This oil is decarburetted in a fixed retort, and gives, with a lighter oil, a very pure coke as residue. The tar is separated from the decarburetted oil by means of sulphuric acid and soda, and it furnishes an oil that is purified by a second distillation and a new treatment with acid and alkali. The result is a white opalescent light oil, of a specific gravity of 0·825°, and an agreeable ethereal odour. The illuminating power is that of nine ordinary wax candles, and the point of combustion is 158° F., whereas the American oil inflames at 113° F.

The yield of light oil is 5 per cent. of distilled schist, and the secondary products are the coke above mentioned, the acid tars, paraffin, &c. The distilled schist serves as a combustible for all the operations of the manufactory; also, for the same purpose, the lignites, too poor to be distilled into mineral oil, are made use of.

PHYSICAL SCIENCE.

*On the Occlusion of Hydrogen Gas by Meteoric Iron,** *by* Thomas Graham, *F.R.S.*

Some light may possibly be thrown upon the history of such metals found in nature as are of a soft colloid description, particularly native iron, platinum, and gold, by an investigation of the gases which they hold occluded, such gases being borrowed from the atmosphere in which the metallic masses last found themselves in a state of ignition. The meteoric iron of Lenarto appeared to be well adapted for a trial. This well-known iron is free from any stony admixture, and is remarkably pure and malleable. It was found by Wehrle to be of specific gravity 7.79, and to consist of—

Iron	90·5883
Nickel	8·450
Cobalt	0·665
Copper	0·002

From a larger mass a strip of the Lenarto iron 50 millimetres by 13 and 10 millimetres, was cut by a clean chisel. It weighed 45·2 grammes, and had the bulk of 5.78 cubic centimetres. The strip was well washed by hot solution of potassa, and then repeatedly by hot distilled water, and dried. Such treatment of iron, it had been previously found, conduces in no way to the evolution of hydrogen gas when the metal is subsequently heated. The Lenarto iron was enclosed in a new porcelain tube, and the latter being attached to a Sprengel aspirator, a good vacuum was obtained in the cold. The tube, being placed in a trough combustion furnace, was heated to redness by ignited charcoal. Gas came off rather freely, namely—

In 35 minutes	5·38	cub. centims.
In 100 minutes	9·52	"
In 20 minutes	1·63	"
In 2 hours 35 minutes	16·53	"

The first portion of gas collected had a slight odour, but much less than that of the natural gases occluded from a fire by ordinary malleable iron. The gas burned like hydrogen. It did not contain a trace of carbonic acid, nor any hydrocarbon vapour absorbable by sulphuric acid. The second portion of gas collected, consisting of 9·52 cub. centims., gave by analysis—

Hydrogen	8·26	cub. centims.	85·68
Carbonic oxide	0·43	"	4·46
Nitrogen	0·95	"	98 6
	9·64		100·00

The Lenarto iron appears, therefore, to yield 2·85 times its volume of gas, of which 86 per cent. nearly is hydrogen. The proportion of carbonic oxide is so low as 4½ per cent.

The gas occluded by iron, from a carbonaceous fire, is very different, the prevailing gas then being carbonic oxide. For comparison a quantity of clean horseshoe nails was submitted to a similar distillation. The gas collected from 23·5 grammes of metal (3·01 cub. centims.) was—

In 150 minutes	5·40	cub. centims.
In 120 minutes	2·58	"
In 4 hours 30 minutes	7·98	"

The metal has given 2·66 times its volume of gas. The first portion collected appeared to contain of hydrogen 35 per cent., of carbonic oxide 50·3, of carbonic acid 7·7, and of nitrogen 7 per cent. The latter portion collected gave more carbonic oxide (58 per cent.) with less hydrogen (21 per cent.), no carbonic acid, the remainder nitrogen. The predominance of carbonic oxide in its occluded gases appears to attest the telluric origin of iron.

Hydrogen has been recognised in the spectrum analysis of the light of the fixed stars, by Messrs. Huggins and Miller. The same gas constitutes, according to the wide researches of Father Secchi, the principal element of a numerous class of stars, of which α Lyræ is the type. The iron of Lenarto has no doubt come from such an atmosphere, in which hydrogen greatly prevailed. This meteorite may be looked upon as holding imprisoned within it, and bearing to us, hydrogen of the stars.

It has been found difficult, on trial, to impregnate malleable iron with more than an equal volume of hydrogen, under the pressure of our atmosphere. Now, the meteoric iron gave up about three times that amount, without being fully exhausted. The inference is that the meteorite has been extruded from a dense atmosphere of hydrogen gas, for which we must look beyond the light cometary matter floating about within the limits of the solar system.

FOREIGN SCIENCE.

PARIS EXHIBITION OF 1867.

(From our Special Correspondent.)

Group V.—Class 44: Chemical and Pharmaceutical Products.

Felix Dehaynin, of Paris, has founded at Aubervilliers, near Paris, some large chemical works, for the treatment, on a large scale, of the products of the distillation of coal tar. One would scarcely believe that the quantity of coal tar annually distilled in the manufactory of Gosselies, where the dust and small coal are agglomerated in the form of small bricks, of immense utility for railway locomotives, amounts to eight or ten thousand tons. It is then treated by another operation, and transformed into benzol, nitrobenzol, and aniline, substances emlyoyed for the preparation of the new colours, the solution of caoutchouc, removal of grease from stuffs, leather varnishing, etc. The benzol,

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* Read before the Royal Society, May 16, 1867.

nitrobenzol, and the aniline of Aubervilliers are much esteemed. The eye cannot contemplate without astonishment the array of twenty-five bottles, which only form a part of the products of the distillation of coal.

The glass case of M. Robinet, of Paris, 3, Rue de l'Abbaye Saint-Germain, formerly President of the Academy of Medicine, is distinguished from his neighbours' by the fact that he sells nothing, that he gives away all the products he obtains—that is to say, his numerous analyses of waters —and he limits himself to the task of soliciting new ones, which he analyses in succession, and gives gratuitously the results of his analyses to those who send the samples. He even goes so far as to give bottles of certain waters to those who, for particular reasons, would be interested in knowing the nature of those in their neighbourhood; and he bears the expense of carriage, bottles, &c., even if they are sent to the other end of the world. What motive has induced M. Robinet to make so many great sacrifices, and to give himself so much labour? Nothing is more simple to answer. He has undertaken to fill up a void in the physical history of our country; he has given to science, industry, agriculture, and public salubrity, a Hydrographic Dictionary of France. There exist, undoubtedly, already, numerous documents from which such a work might be compiled; but they are scattered about and incomplete. The "Geographical Dictionary" no longer exists. The work of 221 pages, published under this title in 1787, by M. Mothey, geographer to the king, did not fulfil the required result. M. Robinet set courageously to this task after the work he had performed as reporter to the commission of inquiry for the diversion of the waters of the Dhuys. He proposes to treat of soft or potable waters in a statistical, geographical, geological, chemical, economical, hygienic, and agricultural point of view. A first essay, already published, devoted to the study of the basin of the Marne, amply proves that the author will complete his programme.

In his glass case we find a hundred specimens of water, and one of the labels bears the number 2082. In fact, M. Robinet has analysed more than 2000 waters! His exhibition is only intended to invite remittances of water from the four quarters of the globe. We would say that his appeal has already been responded to, for we find in the galleries waters from London, the Danube, &c., and there is every reason to hope that the geographical dictionary especially devoted to France will also interest, in a greater or less degree, most of the foreign nations. We sincerely wish that, among the numerous pilgrims to this great *fête* of nations, some will not object to encourage his work and send specimens of waters. The notation for the representation of watercourses of M. Robinet is well worthy the attention of hydrographic and other engineers, his new system gives immediately a very exact idea of the direction, extent, inclination, and other essential characters of watercourses. In order that the remittance of water to be examined may be complete, it should comprise the waters of the rivers, brooks, and wells, with the nature of the soil in which they rise, drainage water, rain water collected in an earthenware or porcelain vessel, and drinking water from the public fountains.

We find in the glass case of M. Joly, of La Rochelle and Paris, No. 13, Rue d'Antin, a very original and new product —viz., marine silk. M. Joly discovered in the eggs of fishes of the family of Sebacians (the ray) that their exterior envelope is formed of a very close tissue, composed of an infinite number of delicate filaments which are easily removed and separated. Once drawn out they possess the appearance, colour, and finish of cocoon silk, serving without trouble for tissues of ordinary silk or silk wad. The interior of the eggs contains an albuminous white substance which can serve usefully in competition with the white of hens' eggs for printing on tissues; they contain a considerable quantity, as each roe weighs on an average 240 grammes (about ½ lb) The manufacturing industry of tissues will certainly make good use of these new products.

We also remark in the collection of M. Joly:—1. His cream of cod-liver oil, much more agreeable to the taste, and more digestible, than the best and purest ordinary cod-liver oil. 2. His Squalus-liver oil, which M. Joly was the first to think of extracting from these fishes. 3. Fish-liver oil for leather manufacture. 4. French guano, a manure formed of inedible fish and the debris of fisheries, very much sought after by agriculturists, and which is likely to improve the condition of the fishermen, as they can sell with profit what was formerly thrown into the sea.

The pharmaceutical establishment of M. Ch. Genevoix, 48, Rue Bonaparte, was the first that, thirty years ago, manufactured in quantities of a ton a day, ferruginous pills and lozenges. His syrups of iodide of iron and his purgative lemonade are remarkable for their indefinite preservation. His gaseous powder replaces, weight for weight, and with much economy, tartaric acid in the preparation of aerated waters in the gazogene apparatus with two compartments. The Paris hospitals employ every year, on an average, 10,000 packets. The quality, the attractive form, the low price, and excellence of manufacture combine to render the establishment of M. Ch. Genevoix a model pharmacy.

M. Emile Genevoix, 14, Rue des Beaux Arts, exhibits feculous seeds and fruits containing from 1 to 10 per cent. of different oils which play an important part in alimentation. Disseminated between the grains of starch, these oils can be extracted by means of sulphuret of carbon, chloroform, benzol, ether, &c. M. Genevoix substituted for these solvents, firstly, carbonisation by sulphuric acid, which, in spite of the high temperature produced, set the fatty matters at liberty, without destroying them, in a state easily soluble in menstrua, or able to be obtained in a greater quantity by distillation. But this method was too costly when it was used on the large scale, and it has given place in the factory of M. Genevoix to an industrial process which allows the production by tons weight of the oil of some fruits, very abundant and without value—the horse-chestnut, for example. Bought at 40 or 50 fr. the ton, the finest chestnuts are rasped, submitted to a full fermentation, boiled in ten times their weight of water, and transformed into glucose by the addition of 2 per cent. of sulphuric acid. The liquor, freed from insoluble portions, is submitted to a slow ebullition, which allows the oil to agglomerate at the surface particle by particle. Drawn off and filtered, this oil is sold to the public without any addition. For ten years past the average quantity of oil manufactured in the chemical works at Romainville has been 600 kilogrammes, extracted from fifty or sixty tons of horse-chestnuts which were bought from agriculturists for 2500 to 3000 fr. The wholesale price of the oil is 20 fr. Vegetable wax is separated from it under the form of stearin, margarin, &c. The water on which the oil floats, when neutralised, gives "syrup of glucose," and "horse-chestnut alcohol," prepared for trade on a large scale. The fabrication of starch had to be abandoned, as the supply of horse-chestnuts was uncertain and insufficient.

This fatty substance, very fluid, absorbable by the skin, has a place marked out in the therapeutics of the gout and rheumatism—in fact, the chestnut oil of M. Genevoix has been found very efficacious, and enjoys a great success.

M. Genevoix, struck with the happy effects obtained by the valerianate of ammonia, has combined valerianic acid with other new base. His valerianate of triamyline contains four equivalents of valerianic acid. If the medicinal action depends upon the acid, and not upon the base, the new compound cannot fail to be successful.

Poultices are attended with great inconvenience in consequence of their weight, their cooling, and their more or less disagreeable odour. For the application on the skin of liquid medicaments, laudanum, tincture of iodine, or fatty matters, M. Genevoix proposes an impermeable tissue enclosing a double layer of swanskin, which is wetted with a decoction of marshmallows, linseed, or poppyheads, and which preserves its temperature for more than twelve hours at 70° C.

M. Boboeuf, No. 9, Rue Buffault, Paris, the inventor of sodiac phenol, is an old acquaintance of ours. We have often enough spoken of him to permit us to say only a few words to-day. Along with phenol he exhibits magnificent specimens of phenic acid and picric acid manufactured by his processes. Thanks to him, the price of phenic acid has descended from 100 to 5 fr. the kilogramme; that of picric acid from 60 to 14 fr. His perfumed phenol, of which the odour is really agreeable, and the hygienic and preservative qualities are incontestable, will vie with the most popular toilet water and dentifrices. His phenol soap will certainly be useful in the therapeutics of skin diseases. It suffices to comb the head a few times with a comb dipped in a small quantity of phenate of soda, to cause all pimples, greasy or dry scurf, &c., to disappear. He obtained the Monthyon Prize, and a most favourable report was made by M. Kuhlmann in September, 1866, at the Mulhouse Industrial Society. These honours, joined to his commercial success, recommend M. Boboeuf to the attention of the jury.

M. Scipion-Dumoulin, No. 5, Rue St. Claude, commenced, in 1820, by the preparation of unalterable ink, rendering forgery impossible. This ink was favourably reported upon at the Academy, by Vauquelin and Deyeux. In 1842 he took out a patent for employing essential oils in a lamp, called the *gazomètre*, which was put into activity or stopped by means of a cock; it was a prelude to the Mille gazo-lamp, which was not possible in those days, as the lighter oils did not exist. In 1852 he devised a carburator, a vase or reservoir containing a spongy matter soaked in volatile essence for extra-carburising the gas passing through it. The means of procuring the gas without machinery is a secret discovered by M. Mille, by which the air or gas is charged spontaneously by passing over the volatile matters so as to be rendered more inflammable. In 1851 he communicated to the Academy of Sciences a new process for preparing picric acid by the reaction of nitric acid on the quasi-resin of canauba palms. This forms picric acid, and another part of the resin is transformed into wax like beeswax. The process is carried on with success in several manufactories of Lyons; the picric acid obtained is very pure, and is not greasy like the acid extracted from coal tar; it is very detonating, and explodes at the least shock. M. Payen repeats every year this experiment in his course of lectures.

In 1862 M. Dumoulin proposed to replace the oxide of lead glaze used for culinary purposes, at the potteries, by a glaze completely unalterable by alkalies and acids. Aided by a skilful potter, he thought at first he had realized the wished-for improvement, but it was otherwise; after a short struggle he gained the ground. He exhibited first in 1855 his liquid glue first discovered in 1850, which Francis Arago presented to the Academy on September 27, 1852, as solving a very difficult problem. The process was given voluntarily to the world by M. Dumoulin—it consists in pouring a small quantity of nitric acid into a solution of common glue or gelatin in its own weight of water. This glue, of two sorts, brown and white, is very strong, and is employed in private houses, workshops, and by jewellers, clockmakers, &c. Thousands of phials are sold abroad. To this liquid M. Dumoulin has added a cement insoluble in boiling water, and which rendered great service in the restorations at the Campana Museum. We may mention his cobalt green and blue, which were the first in France which really opposed the blues and greens of Saxony; his process of gilding and silvering without the battery, very easily and promptly effected by means of precipitated gold and silver, which render so much service in the jewellery art; his new ink, double black, prepared since 1853, without salts of iron, and incapable, therefore, of corroding the pens or paper, possessing from the first moment, and preserving indefinitely, its deep colour.

In 1860 M. Jean-Henri Chaudet, of Rouen, was the first to propose the use of bisulphite of soda or *leucogene* for wool bleaching, which is now universally employed in France, Belgium, Italy, Prussia, and Russia. In a tub filled with cold water, the first operation is to pour 18 litres of leucogene at 25° for every 100 kilogrammes of wool to be bleached. The wool, washed and scoured as well as possible, is steeped for at least three hours in this bath, and left to drain over the tub so as to save all the liquid. It is then dried in the open air. The same bath serves for any length of time, provided at each operation there are added nine fresh litres of leucogene per 100 kilogrammes of wool, with a sufficient quantity of water necessary to replace that used up by the preceding operation. The wools thus bleached have a whiteness which is permanent and more lasting than the bleaching obtained by sulphurous acid They can enter into the manufacture of tissues and other goods without the slightest danger of injuring the most delicate colours. Leucogene is an excellent decolorising matter if used in the bleaching of vegetable textile matters, such as cotton, linen, hemp, jute, or phormium. It gives a silky white colour to threads and tissues, that cannot be obtained with the hypochlorites. M. Chaudet manufactures annually 140 tons of leucogene, representing 2000 tons of white wool.

In 1866 he conceived the idea of applying bisulphite of soda and indigo to the blueing of wools. He has rendered great service to manufacturing arts by introducing an agent for permanently dyeing white wools blue by sulphurous acid. The process is extremely simple. All that is necessary is to add to the ordinary leucogene bath from three to five parts by weight of blue dye for every 100 kilogrammes of wool to be bleached. The operations of dyeing and bleaching take place simultaneously.

In the course of 1865–66, M. Chaudet effected another improvement in the industrial arts. Up to that time chromium was only employed as a mordant in the state of chromate, in which it acted the part of an acid. The colouring matter was often burnt by the oxygen of this acid, and the tints obtained changed rapidly when exposed to the air. By substituting for the chromate other salts in which chromium acted as base, new and permanent effects have been obtained. The salts used by M. Chaudet are the sulphate, the nitrate, and the oxalate of chromium. They are employed in the same manner as the salts of alumina. In copper vessels 4 or 5 by weight of sulphate of chromium, at 62° Beaumé, are dissolved as mordant for 100 of wool. The liquid is raised to a boiling point and left to simmer for two hours. The stuff is then washed and rinsed as usual. For vegetable substances such as flax, cotton, or hemp, the bath is composed of a solution of nitrate or oxalate of chromium, marking 1½° to 3° Beaumé. It is stove-dried, rinsed, and dyed. For printing, the ordinary method of operation is not changed, except that the acetate of alumina is replaced by the nitrate or oxalate of chromium, or the sulphate of alumina by the sulphate of chromium. With the salts of chromium as mordants, new shades of colour are obtained of a solidity before unknown.

(From our own Correspondent.)

Paris, May 28, 1867.

At the meeting of the Society of Encouragement on May 17, M. Tresca, who replaces M. Combe in the secretary's chair, paid a high compliment to the new steam engines of M. Duvergier, the constructor of the boats now plying on the Saone and Loire; he pointed out improvements in the machinery and in the employment of steam.

M. Julien Caudron, ropemaker at Malaunay (Seine Inférieure), submitted to the Society (Rope Section) ropes, splices, and knots of cotton, which, owing to their tested strength and cheapness, are preferred, for naval purposes, in some cases to hemp ropes. Experiments made at the several seaports amply confirm the hopes of the inventor.

M. Payen resumed, in a rather long but learned and interesting discourse, the progress accomplished in the fabrication of paper, parchment, and the employment of parchment paper for the separation, by endosmose, of the salts which hinder the extraction and crystallisation of sugar contained in juices, syrups, and molasses. After having called to mind the physical and chemical composition of herbaceous or woody fibres, he enumerated—1. The papers of MM. Bachet and Marehad, who treat the sprigs of fir first by hydrochloric acid, and afterwards by chloride of lime to bleach them. 2. The papers made from seaweed and other marine plants obtained nearly in the same manner by MM. Poinsot, Breton, &c. 3. The lucern papers of M. Caminade, the wild-thistle papers, the straw and sparta papers, &c. 4. Chinese rice papers, or those made from the pith of *Laruca poperia*; 5. The parchment papers of M. Neumann, at St. Denis. We shall not return to the subject of the osmose of sugars, as we think our readers are already sufficiently initiated into it.

M. Isambart had at work two magnesium lamps, the first being exactly similar to that of M. Solomon. A movement by clockwork rapidly unrolls the magnesium wire, which is coiled on a bobbin. It has the inconvenience of disengaging too much white vapours of magnesia, which for a moment obscure the light. The second is only a modified Larkin's lamp; and M. Isambart should not have omitted to mention his name. An ordinary spirit-lamp occupies the bottom; a glass tube forms a chimney; a small box or recipient containing magnesium in powder mixed with 50 or 80 per cent. of fine sand. By turning a small button a cock is opened; the mixture is inflamed, and burns with a very brilliant light. We prefer the first light to the second, of which the intermittent and unsteady light is intolerable to the eye. M. Isambart said that the expense per hour of the second lamp was about 3 fr., and that the price of magnesium may still be lowered. At 3 fr. this intense light is not too dear.

M. Durand presented, and set at work, a small model of a brick-making machine, representing one of the novelties of the Exhibition, of which we shall speak shortly.

M. Julien presented a pamphlet on iron and steel. We do not wish to enter into controversy here on this subject, only we notice that iron and carbon do not combine; they only mix together as water and oil.

M. Emile Petit sent specimens of artificial lithographic stones. We wish him success, as the lithographic presses of MM. Kocher and Houstiaux require for continuous impression cylindrical stones of a size difficult to be found in nature.

M. Lavollée, in the name of the Committee of Commerce, read a report on the exportation of the habitable constructions by M. Benis. Two portable private houses were sent out to the Island of St. Thomas. The walls were formed of two vertical and parallel sheets of scagliola or imitation of marble, the interior being filled up with wood shavings, alga marina, &c. The ceilings are partly of iron and partly of wood subjected to the process of injection of sulphate of copper and tar afterwards. The floors are of imitation marble, &c. The cost of the two houses was 45,000 fr., including 1100 fr. for the carriage. Their aspect is exactly that of houses built of marble.

M. Tresca made a communication on the mechanical applications in the interior of mines. Works in mines require a motive power, either sudden and discontinued or continuous and slow. In the first case, the best motor is, compressed air; in the second, water under pressure. As in tunnel work there can be no admission of fire, combustion, or the engendering of steam. Compressed air has been happily employed by M. Sommelier for the engines used by him in the tunnel of the Alps. Water under pressure was employed by M. Perret in the works of the South of France railways, to give a rotatory movement to the rings of circles of M. Deschaux, armed with diamond points for cutting the hardest rocks. The great problem of the day is the mechanical getting of coal. He explained and put before the eyes of the members two models; one of Carrett, Marshall, and Co.'s coal-cutting machine, the other of that of Jones, Levick, and Co., of Newport, Mon.

M. Chalmel, of Paris, exhibits a preservative varnish, used by all the great silversmiths and goldsmiths of Paris. This varnish is also very serviceable for copperplate engraving. He also exhibits an excellent green for water-colour drawings, miniatures, &c.; and oil of turpentine and lavender for the vitrification and incorporation of colouring matters on marble, porcelain, crystal glass, &c.

M. Bourgeois-Rocques, of Ivry (Seine), has adopted a singular method of manufacturing essences of wine. He sells it in small phials to vendors of natural and artificial liquids for the purpose of giving the colour, aroma, and "bouquet" of first-class wine crops, and of the best growers. For example he gives to the most ordinary white wines the perfume and taste of Sauterne.

The widow Madame Audouin, of Paris, exhibits a species of marine glue, which is susceptible of being put to an immense variety of uses. The forms are very variable—black glue, resisting the action of salt water; red, replacing minium; yellow, for staining wood; and soldering glue for sticking together wood, metals, glass, or porcelain.

CHEMICAL AND PHARMACEUTICAL PRODUCTS IN THE BRITISH SECTION.

(FROM ANOTHER CORRESPONDENT.)

In giving a short account of the chemical and pharmaceutical products of British manufacture in the French Exhibition, our object will, in the first place, be to state what the substances exhibited are, and in the next, whenever possible, to glance rapidly at the chemical reactions and principles involved in their production.

We need scarcely say that in a vast number of instances the Exhibition has simply been used as a means of advertising goods of no scientific merit whatever. In an almost equal number of cases the goods shown are devoid not only of all claim to the interest of scientific men, but are not even remarkable as representing any advancement in either manufacturing skill or inventive resource. In addition to these there are many instances where there are no means whatever of forming a judgment as to the quality of the substances exhibited. We allude to compounds and mixtures in liquid or powder, the true composition of which is a secret known only to the manufacturer. The chemical and pharmaceutical products form Class 44 of the Exhibition Catalogue; they are contained in Gallery V. of the building, and have been thus classified:—

Acids, alkalies, salts of all kinds, sea-salt, and products extracted from mother waters.

Various products of chemistry; wax and fatty substances, soaps and candles, raw materials used in perfumery; resin, tar, and the products derived therefrom; essences and varnishes; various coating substances, blacking, india-rubber and gutta percha and their products; dyes and colours.

Mineral and sparkling waters, natural or artificial.

Raw materials used in pharmacy, simple and compound drugs.

We shall take them as nearly as possible in the order in which they occur in the Catalogue.

The first exhibitors whose case we shall notice are the well-known firm of Allen and Hanburys, of Plough Court, London. They exhibit only two articles—viz., cod-liver oil and Liebig's extract of meat. Of the first of these two, there are samples manufactured in London and in Norway, both with the stearine, and also with so much removed as will crystallise out at ordinary temperatures.

Messrs. Allen and Hanburys inform us that they commenced the manufacture of cod-liver oil as early as the year 1843, and according to the directions then recently published by Professor Donovan. They have continued the manufacture regularly since that date, both in London and abroad, with only slight modifications of the original process. The oil is of a pale straw colour, and, when first made, has a sea-weed like odour, which, however, cannot long be preserved, no matter how carefully it has been prepared. Donovan's process, according to Cooley, is as follows:—"The perfectly fresh livers are placed in a metallic vessel and heated with constant stirring to 180°F., by which treatment they break down into a uniform pulpy liquid mass. This mass is immediately transferred to calico bags, whence the oil drains out; after filtration, while still warm, this oil is sufficiently pure for use."

Cod-liver oil is now so much used in medicine, that it has become a highly important article of commerce.

In this country we are inclined to look with more or less of suspicion upon any name that is connected with a largely advertised medicine. It is impossible to deny that the lustre of one of the most brilliant names in chemical science has been somewhat dimmed, at least in the eyes of the world, by being continually seen attached to jars of a highly nutritive but certainly uninviting-looking preparation in great favour at the present moment with the debilitated and dyspeptic.

This circumstance, although certainly unfair, is absolutely inevitable. There are few, therefore, who read the advertisements of "Dr. De Jongh's light brown cod-liver oil," who are aware that that gentleman (who, from the pertinacious way in which he has been decorated, appears to be a favourite with kingly amateurs of cod-liver oil) published in 1843 a most laborious research on the substance alluded to.

That cod liver oil is a valuable remedial agent in numerous diseases of a scrofulous type is now so generally conceded, that it would be a waste of time to argue the point. To which of the numerous ingredients detected by De Jongh we ought to attribute the active properties of the oil, is another matter. We need hardly say that the wildest views are entertained on the subject, especially by medical men, whose chemistry, as a general rule, is disgracefully defective. The subject is so important that we shall make no apology for quoting the analyses of the authority we have named. It is true that they show weak points, especially as regards the bile ingredients; the defects, however, are more easy to point out than to remedy, and, at the time the analyses were made, were not so obvious.

	Brown.	Light Brown.	Pale.
Oleic acid (with gaduine and two other substances,)	69·78500	71·75700	74·03300
Margaric acid	16·44500	15·42100	11·75700
Glycerine	9·71100	9·07300	10·17700
Butyric acid	0·15875	—	0·07436
Acetic acid	0·12506	—	0·04571
Fellinic acid and cholinic acid, with some margarine, oleine, and bilifulvine	0·29900	0·06200	0·04300
Bilifulvine, bilifellinic acid, and two peculiar substances	0·87600	0·44500	0·26800
A peculiar substance insoluble in alcohol of 0·968 sp. gr.	0·03800	0·01300	0·00600
A peculiar substance insoluble in water, alcohol, and ether	0.00500	0·00200	0·00100
Iodine	0·02950	0·04060	0·03740
Chlorine and traces of bromine	0·08400	0·15880	0·14880
Phosphoric acid	0·05365	0·07890	0·09135
Sulphuric acid	0·01010	0·08595	0·07100
Phosphorus	0·00754	0·01136	0·02125
Lime	0·08170	0·16780	0·15150
Magnesia	0·00380	0.01230	0·00886
Soda	0·01790	0.06810	0·05540
Iron	traces	—	—
Loss	2·56900	2·60319	3.00943
	100·00000	100·00000	100·00000

It is not remarkable that so many persons should entertain unsound views regarding the causes of the efficacy of cod-liver oil. Dr. De Jongh considers the value of the oil to be derived from the iodine and the elements of the bile. Others have imagined its curative properties to reside in the bromine, others in the phosphorus. It appears to us that these views are sufficiently refuted by the fact that none of the attempts to administer the ingredients of the oil in a separate state have succeeded. It is true that it has been attempted to evade this difficulty by saying that in cod-liver oil the substances alluded to are in a peculiar condition in which they are especially prone to assimilation. There is not the slightest evidence that this is the case. The free phosphorus has had the credit of being the really valuable ingredient. But is it absolutely certain that the phosphorus in cod-liver oil is free? Unless we are mistaken, the evidence for the existence of free phosphorus rests upon the fact that more phosphoric acid is obtained after oxidation of the oil with nitric acid than is obtained by precipitation from the liquid separated from the fatty acids after saponification. We think the evidence for the existence of free phosphorus requires to be greater than this. But, even assuming the fact, we think it in the highest degree doubtful if cod-liver oil, even if of the most absolutely correct "light brown" tint, possesses any remedial virtues save what are due to the fact of its being a highly digestible fat oil; and we consequently contend that the coloured and fœtid oils possess no curative properties that are not found in the carefully prepared and consequently nearly colourless oils—nay, more, that the disgusting flavour of the foul fish-oil of commerce, by rendering it more loathsome, in the same ratio renders it more difficult to assimilate.

(From our own Correspondent.)

Paris, June 18, 1867.

The Society of Mechanical Engineers of England held its annual meeting in Paris a few days ago, presided over by Mr. Penn, the celebrated constructor of marine engines. The meetings took place in the amphitheatre of the Conservatoire des Arts et Métiers, and many papers were read and communications made by the members. Amongst others were—"*Puddling by Machinery*," by M. Menelaus; "*Ventilation of Buildings*," by General Morrin; "*The Flowing of Solid Bodies through Orifices*," by M. Tresca, etc. Without presenting any novelty, the subjects brought forward were listened to with much interest. The scientific congress terminated with a magnificent banquet, at which 200 persons sat down. The meeting took place in the magnificent saloon of the Restaurant des Frères Provinciaux. Among the French celebrities invited we may mention General A. Morrin, M. Leverrier, M. Combe, M. Solacroup, director of the Orleans railways, M. Tresca, &c. Mr. Penn proposed, in the handsomest terms, the health of General Morrin, Messrs. Tresca, Fairbairn, and Stewart, of Mancester, and dwelt on the cordiality existing between the mechanical engineers of France and England.

M. Pasteur continues actively his researches on silkworms. He is now able to state that—1. Not a single silkworm, chrysalis, or moth derived from the eggs exempt from corpuscles presented any of these microscopic organisms; out of 16 sets of eggs laid, not infected with corpuscles, 15 succeeded. 2. The silkworms, chrysalides, and moths from the eggs obtained from corpusculous moths gave, in a more or less degree, the worms, chrysalides, or moths affected with corpuscles. M. Pasteur has also discovered in his researches another terrible malady, which he carefully describes, and for which the only remedy seems to be the renewing of the air by a shaft. M. Le Ricque de Monchy has found from his observations that creasote preserves healthy silkworms from parasitic diseases, and cures sick ones.

In submitting to the action of the poles of an electromagnet, bubbles of the glycerine liquid of M. Pasteur, filled with oxygen, M. Chantard, of Nancy, has succeeded in obtaining energetic attractions, and considerable oscillatory movements. He produces a sort of magnetic pendulum, which can be rendered visible to all by a ray of Drummond light.

The lectures at the great hall in the park of the Champ de

Mars commenced last week. They will be continued daily—at midday, 2 p. m., 4 p. m., and 8 p. m.—on different subjects, at the same time exhibiting and explaining various objects in the palace. For this purpose special authority has been granted by the Imperial Commission for the temporary removal of objects from the stalls to the lecture hall in the park. Excursions will also be made, accompanied by the audience, to visit and explain important objects in the main building, park, and reserved garden. The lectures at noon embrace subjects only indirectly connected with the Exhibition, such as lectures on the universal language of music, invented by M. Sudre; studies of India, by M. Giguel, etc. From 2 to 5.30 p m. the attention of the audience will be directed to various objects, and at 4 p. m. the subject is confined to those in direct relation with the Exhibition. Among the scientific lectures will be one by Dr. Crace Calvert on phenic acid and other analogous products. The new building is situated at the right of the Pont de Jena, next to the International Club building on the banks of the Seine. It holds 500 persons, and the decorations are carried out in the style and with the well-known taste of Parisian ornamentation. The electric light, the Carlevaris, Drummond, oxyhydro lamp (magnesia), and the millegazo lamp will alternately light up the interior of the hall.

F. MOIGNO.

(FROM OUR SPECIAL CORRESPONDENT.)

THE Glass Company of St. Gobain, Chauny, and Cirez, have four plate glassworks and two manufactures of chemical products.

The factories at Saint Gobain and Chauny are the oldest of those belonging to the Company. Their creation dates as far back as 1693. They comprise all the workshops, furnaces, and apparatus necessary for the manufacture of mirror and plate glass, thin white or coloured glass for dwellings, and thick slabs for lighting cellars. They also furnish moulded glass of a special quality for lenticular lighthouses, the cutting and polishing of which is executed in the workshops of MM. Henri Lepaute, Sautter, Barbier, and Fenestre.

The great lenses, which serve for enlarging photographic views in the apparatus of Worthly and others, are made at St. Gobain, as was also the great piece of glass 3 ft 11¼ in. in diameter for the great silvered mirror of M. Leon Foucault's telescope.

The glass plates, etc., cast at St. Gobain are sent to Chauny, nine miles off, to be polished, silvered, etc. At these immense works the motive power for grinding and polishing is 600 horse-power, partly furnished by a fall of water, and partly by steam. The polishing powders, such as emery, English red, etc., are there prepared. The manufacture of tinfoil for silvering glass is also carried on at Chauny, whence are supplied all the mirror factories of the Company. They make sheets of tinfoil of dimensions almost without limit, either by hammering and rolling, or by casting on cloth.

The establishment at Cires-sur-Vezouze replaced in 1740 the factory of Saint Quirin, and includes the workshops and melting furnaces, a portion being situated near falls of water, which are utilised as motive power. The plate glass factory at Stolberg was founded in 1853 by the Aix-la-Chapelle Company. An establishment was founded by the Company at Manheim in 1854, at the confluence of the river Neckar with the Rhone.

Amongst the objects exhibited by the Company are—

1. Plate glass, unsilvered, measuring 5·93 m. by 3·64 m., or 21·58 square metres in superficies, and 5·88 m. by 3·60 m., or 21·17 square metres.

2. Silvered mirrors, 5·90 m. by 3·68 m., surface 21·71 square metres; 5·01 m. by 3·60 m. — 18·04 square metres.

3. Moulded rough glass.

4. Coloured and aventurine glass, the latter being very beautiful.

5. Thin glass for small mirrors and photographic plates.

6. Different products, including a leaf of tinfoil, 6 m. by 4 m., for silvering the largest glasses.

There are also eight fine specimens of different sorts from the other works mentioned above, owned by the Company. The glass for the immense plates described above, was melted in one single pot, capable of containing a ton of matters in fusion, 700 to 800 kilogrammes being utilised.

The silvering of these gigantic glass plates is one of the most delicate operations. For those of twenty square metres, the sheet tin weighs two pounds per square metre; if it is any thinner, it dissolves the mercury before the operation is finished.

For a long time past I have abstained from discussing the chemical and pharmaceutical products of England, for my heart always failed me, as it was with pain that I viewed the inferiority of the English display in this class. I cannot help regretting their abstinence from our galleries, either from indifference or from other causes. This inferiority, perhaps, may be explained by the bad light in which the objects are placed, so that the daylight hardly penetrates except in a triste and gloomy ray. Several English chemists, well informed of all that has been accomplished latterly, accompanied us to this department, and found almost nothing of novelty.

THE CHEMICAL NEWS has announced that one of its oldest and most qualified collaborateurs is coming to Paris to examine the English section, and bring to light the hidden articles. I warmly applaud this step.

I am now able to give you the following list of those to whom the great prizes of the Exhibition have been awarded:—

2nd Group.—MM. *Maine*, printer and publisher at Tours; *Garnier*, photographic engraving, Paris; *Sax*, wind instruments, Paris; *Mathieu*, surgical instruments, Italy; the Reverend Father *Secchi*, meteorologic apparatus, Rome; *Eichens*, astronomical instruments, Paris; *Jacobi*, galvanoplastic work, Russia.

3rd Group—*Fourdinois*, art furniture, Paris; *The Baccarat Company*, for crystal glass, France; *Klagmann*, sculptor, Paris.

4th Group—*The City of Lyons*, tissues and silk goods.

5th Group—*Petin, Gaudet, and Co.*, steel, France; *Bessemer*, manufacture of steel, England; *Hofmann*, colours obtained from coal-tar, Berlin (Prussia); *Krupp*, steel works, Essen (Prussia); *Brazil*, *Algeria*, *British India*, *Egypt*, *Italy*, and the *Ottoman Empire*, for cotton since 1861.

6th Group—*Creusot Company*, different machines, steam engines, etc., France; *Siemens*, gas furnaces, Berlin (Prussia); *Whitworth*, mechanical tools, England; *Viguier*, railway signals, France; *Cyrus W. Field*, Transatlantic cable, New York (United States); *Hughes*, telegraphic apparatus; *Isthmus of Suez Company*; *Kindt* and *Chaudron*, apparatus for tubbing mine shafts and sounding apparatus, Belgium; *Hofmann*, brick ovens; *Napier*, marine steam engines, Glasgow; *John Penn*, marine engines, Greenwich; *Hirn*, transmission of motive power, France; *Farcot*, steam engines, Saint Omer (France).

7th Group—*M. Pasteur*, member of the Institute, preservation of wines, France; *Henri Mares*, application of sulphur to vines; *The Emperor of Russia*, thorough-bred horses.

10th Group—*The Emperor of the French*, workmen's dwellings; *Dufresne*, mercury gilding, France.

F. MOIGNO.

(FROM OUR OWN CORRESPONDENT.)

PARIS, June 25, 1867.

WE have seen petroleum oil recommended in an English paper for the destruction of insects. The method is very efficacious, but Dr. Sacc, of Neufchâtel, in Switzerland, remarks to us that petroleum oil has a more powerful effect upon plants than upon insects; it kills them as if by fire. He saw a magnificent Norfolk Island pine (*Araucaria excelsa*) killed by being doctored with petroleum oil, applied in order to kill the insects. In this case the remedy is worse than the disease.

A young chemist, M. Bèraud, relates, in a letter to M. Dumas, how his grandfather, Etienne Bèraud, was the first, at the works of La Paille, along with Chaptal, to conceive and put into practice the continuous combustion of sulphur in leaden chambers. "One evening in the year 1795, my grandfather submitted to Chaptal the following project:—A brick furnace is to be constructed close beside the chamber. The fumes of sulphur are to pass into it by means of a leaden pipe three lines thick and a foot diameter, and to prevent the heat from melting the pipe it is to be surrounded by another lead pipe full of water, to be renewed when necessary. Chaptal made a thousand objections to this process; the draught produced in the chamber would waste a great quantity of the acid vapours, and take away all the profits of the enterprise. After a long discussion, Chaptal returned home very late; on going to bed he could not sleep, but was haunted with the idea put forward by his pupil and partner. Thinking over his objections, Chaptal found them all disappear one by one, and at last so well did he approve of the proposal that he roused his servant and sent him off to La Paille, a quarter of a league from the town. Of course he found the place shut up and silent, but, by throwing stones at the window-shutters, the man roused my grandfather, who put his head out of the window. The man then cried out—'M. Chaptal has found your idea excellent, and he begs of you to put it into execution the first thing in the morning.'" The new apparatus had such success that three years afterwards the two partners divided 265,000 fr. between them as profits. A portion of this sum was spent in the construction of new works at the Ternes, Paris.

The director of the Dieuze Salt Works, M. Paul Bouquet, and the director of the laboratory, M. W. Hoffman, nephew of the celebrated professor at Berlin, have placed at our disposal a complete memoir on the regeneration of the sulphur from soda waste, which we propose to analyse in sufficient detail to give an exact and clear idea of the whole process. Soda waste, oxidised in the air, is transformed, after a certain time, into two series of compounds; one, insoluble, consists of sulphate of lime, carbonate of lime, silicate of lime, silicate of alumina, silicate of magnesia, and sulphur.

The other is soluble, and consists of polysulphide and hydrosulphate of sulphide of calcium, polysulphide of sodium, hyposulphite of lime, hyposulphite of soda, sulphate of soda, and chloride of sodium. Left to itself, and to the action of rain, the alkaline sulphide varies greatly in strength.

On the other hand, the acid chloride of manganese contains, besides chlorides of iron and barium, free chlorine, hydrochloric acid, water, chlorides of magnesium, aluminium, cobalt, and nickel. If, in order to regenerate the sulphur, we allow the acid chloride of manganese to react upon the soda waste, or on the sulphurous vapours which proceed from it, an abundant escape takes place of sulphuretted hydrogen gas. The presence in the atmosphere of sulphuretted hydrogen caused serious ophthalmia, even when present in a small quantity, which made it necessary for the workmen to stop work for some days occasionally; also, when the quantity of gas was more considerable, the air was infected to such an extent that birds passing over the vessels in which the reaction took place, fell completely suffocated. It was, therefore, necessary to produce a combination which, whilst it separated from the soda waste all the sulphur it contained, would avoid the disengagement of sulphuretted hydrogen, or reduce it to such a degree that its presence would be no longer noxious. It has been ascertained that if the soda waste, on being removed from the lixiviating apparatus, is mixed directly with a certain proportion of sulphates of iron or manganese, these salts are transformed into sulphides. The mixture is then heaped up and left exposed to the air, and is stirred from time to time and kept wetted by a thin stream of water until the metallic sulphides, absorbing the oxygen of the air, are transformed into free sulphur and metallic peroxides. These last, in presence of an excess of sulphide of calcium, are reduced afresh into sulphides of iron and manganese, which, at the end of a short time, are again oxidised in their turn by contact with the air, and so on. The oxygen of the oxides combining with the sulphide of calcium gives rise either to hyposulphite of soda or soluble oxysulphides, the composition of which approaches nearly $CaOS$. Lastly, the sulphur, being set at liberty by successive oxidations of the metallic sulphides, combines with the sulphide of calcium to form polysulphide of calcium soluble in water.

The process is now carried on at Dieuze, and is based upon the above observation. The experience of several months has practically proved that from the 20,000 litres of chloride of manganese and the 30,000 kilos. of soda waste produced every day at the Dieuze works, 14,000 kilos. of pure sulphur can be economically obtained, together with 2,200 kilos. of sulphur in the state of sulphides, 770 kilos. of binoxide of manganese at 60 per cent., 20 kilos. of hyposulphite of lime, and 600 kilos. of sulphate of lime, which can be employed instead of kaolin in the manufacture of paper.

A very interesting lecture was given on June 22, in the great hall in the Exhibition grounds at the Champ de Mars, by Captain Craufurd, R.N., F.R.G.S., etc., on the subject of the depolarisation of iron ships, by the method invented by Mr. E. Hopkins, C E. Captain Craufurd was introduced to the assembly by the writer of this article, under whose auspices the hall was constructed. This instructive lecture was admirably delivered by Captain Craufurd—not in English, but in excellent French; and the practical experiments were conducted by the Rev. E. Hopkins, son of the inventor, and illustrated by a model of H.M. ironclad frigate *Northumberland.* Among the *savants* assembled to hear the lecture of the gallant captain we remarked Admiral Labrousse and M. Gossin, both of geographical fame. F. MOIGNO.

(FROM OUR SPECIAL CORRESPONDENT.)

YOUR correspondent, when he promised to go to Paris and communicate his views upon the Exposition, little knew the task he had undertaken!

He arrived at the moment of the advent of the sovereigns of Russia and Prussia, and, for the time, nothing was thought of in Paris but the "Grand Prix," the review, and the preparations for the entertainment at the Hôtel de Ville.

It would be out of place here to detail the grand sights at which your correspondent "assisted," and you would not care to know how near he was to the assassin when a *coup de pistolet* nearly plunged France into a state of anarchy. Let us, then, leave these interesting but unscientific details, and hire a conveyance to take us to the Exposition. But this is more easily said than done. To get a conveyance now in Paris is a feat not to be undertaken too rashly. In one of the so-called comic journals with which Paris is infested (and at whose jokes we would weep instead of laugh, if the fluids of the body were not dried up by the heat), there is a picture of a family on their knees in the street imploring a coachman to take pity on them and drive them to the Exhibition. The coachman passes on, nose in air; then wildly exclaims the father (as a last resource), "Take the hand of our daughter." From the difficulty we had in Paris to get a conveyance, we believe even that bribe would be insufficient.

The appearance of the Exhibition building from the outside is, as every one has heard, most unprepossessing. At the principal entrance you walk under an awning of dark green, powdered with the Napoleonic bees. On the left is the exquisitely decorated *salon* of the Emperor, on the right the department occupied by the Whitworth and Armstrong guns.

To those who remember the two Exhibitions in this country, especially that of 1851, the general aspect of the French building—whether we regard the interior or exterior—cannot fail to be disappointing.

The arrangement in concentric rings, large as the rings are, effectually prevents the possibility of the existence of great lines, and consequently there is no part of the building or grounds where the view is really grand or imposing. Now we contend that from the great reputation which the French have not unjustly acquired, and the experience which they have had to help them, we have a right to expect something better.

To say that the French Exhibition as a building, or the arrangements as a whole, can in any way compare with the previous English ones, is simply and obviously untrue. That there is much in the present collection superior to objects of the same class which were in the previous Exhibitions, no unprejudiced person will attempt to deny; but the advance is not so great as might have been anticipated, and of the discovery of new principles, or even really new applications of principles previously recognised, there is scarcely a trace.

To return to our remarks on the general arrangements. We all know the unmerciful manner in which almost all the details of the English Exhibitions were criticised, both at home and on the Continent. But even poor Colonel Sibthorpe never (at least, in our hearing) accused the Commissioners of sacrificing the dignity of a national undertaking with the view of finding amusement for those who were too stupid to feel an interest in the contents of the building. What would have been said if within our grounds we had allowed a music hall after the type of the Alhambra of the Oxford? and yet the present Exhibition contains a *café chantant*, where four or five young ladies sing very French songs, while their audience drink beer and smoke their cigars. What would have been said if within the grounds of either of our Exhibitions we had admitted Richardson's show? and yet in the new Exhibition gardens is a so-called *Théâtre Chinois*, where conjurors swallow swords and toss cups and balls; but we pledge our word that the entertainment of the much-lamented Richardson was decidedly more attractive than that of his French descendant.

The straining after effect evinced in sending Arabs, mounted on camels, to promenade the grounds, is upon a par in taste with the plan adopted of dressing the barmaids in the costume of the country represented by the refreshment rooms in which they display themselves. It is some comfort to our insular vanity, however, to know that the Englishwomen in the ordinary dress of the period immeasurably surpass in appearance their fantastically attired sisters of other nations.

Your correspondent (who has been repeatedly accused of an undue leaning towards French things) has no hesitation in saying that in the Paris Exhibition the dignity of a great national undertaking has been sacrificed to glitter, theatrical effect, and, above all, to the great principle of "making the war pay its expenses."

After the building is closed, the theatre and refreshment bars remain open, and the aspect of the place so much resembles that of a *Mabille* or *Maison Rouge*, that really one would scarcely be astonished at lighting upon groups illustrating the dances of all nations with a *soupçon* of *cancan*.

No unbiassed person of good taste will, we are certain, deny that in grandeur, repose, and dignity, the present French Exhibition, as regards building and arrangements, is inferior to both its English predecessors.

If we were asked what we thought the visitors generally admired most, we should unhesitatingly say the pictures. This, however, is always the case, it being obvious enough that there are more people capable of appreciating pictures than there are of comprehending machines, specimens, and objects illustrating the present state of practical science.

It was, however, decidedly an unexpected—shall we say undesired?—pleasure to meet so many old, very old friends among the pictures. It must, nevertheless, be admitted that infinitely more tact has been exercised by the judges who admitted the pictures (whatever the rejected artists may say) than has been shown by those whose province it was to select the objects considered worthy of representing the present state of chemical manufactures.

We contend that no object should be admitted into an exhibition unless it has to a greater or less extent a characteristic appearance or property. A platinum still *is* a proper object, because, although the platinum might possibly be impure, its appearance in some degree enables the spectator to form a judgment upon it, and moreover its value and beauty render it an object of interest; but cakes, bottles, and tins of blacking, no matter how good in quality, can represent no important advance in excellence, no development of an idea, and serve no purpose whatever save to show the vanity and bad taste of the exhibitor.

When we think how many beautiful works of science and art have been refused space, and how many more have never been sent, owing to the modesty or timidity of their inventors or makers, we cannot refrain from expressing our disgust at seeing cases filled with soda water, baking powders, and blacking.

In our next article we shall return to the description and study of the chemicals in the English department.

Paris, June 15.

PROCEEDINGS OF SOCIETIES.

ROYAL INSTITUTION.

Tuesday, May 21, 1867.

A Course of Four Lectures on Spectrum Analysis with its Applications to Astronomy,* *by* William Allen Miller, *M.D., LL.D., Treasurer and V.P.R.S., Professor of Chemistry, King's College, London.*

Lecture II.

Spectra of Simple Bodies.—Spectra of Compounds.—Effect of Temperature upon each of these classes of Spectra.—Mode of Comparing Spectra with each other.—Analysis of Artificial Flames by the Spectrum.—Nature of Information thus obtained.—Discovery of New Metals by the Spectrum.

In the last lecture I brought before you three different classes of spectra—1. The spectra produced by the ignition of solid and liquid bodies, which are continuous. 2. The spectra of ignited gases, which are discontinuous, or interrupted. 3. Composite spectra, the result of the action of the spectrum of an ignited gas or vapour upon a continuous spectrum at a higher temperature which is transmitted through the gaseous spectrum. We have, therefore, 1, spectra exhibiting one continuous shaded band of varying colour, from red, on the one hand, to violet on the other; 2, spectra in which certain colours only are present, each band of colour being perfectly definite, and referable to the particular substance by which it is produced; and 3, we have absorption spectra produced by the superposition of this second class of spectra upon the first.

I purpose to-day to confine our attention chiefly to those spectra which are furnished by the incandescence of gaseous matter under different conditions—different especially as regards temperature; for the study of differences of this kind is particularly necessary to enable us correctly to interpret the changes going on in objects at a distance.

It is a remarkable, but, at the same time, not, perhaps, on consideration, a very surprising fact, that the same substance in the liquid or solid condition should give out a very different spectrum from that which it exhibits in the gaseous state. The particles of which bodies consist in the solid or liquid form are fettered, so to speak, by association with each other. Consequently, the vibrations to which they give rise are, of necessity, of a more composite order than those which would be produced by the same substance if it were converted into the gaseous condition, where its particles not only have no mutual attraction, but are powerfully self-repulsive. These ultimate particles or atoms, in the case of simple bodies—molecules in the case of compound bodies—by the fact of their being raised in temperature, acquire motions of their own, which they are able to communicate to the other; and by these motions they occasion in us the sensation of light.

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* Reported specially for this paper, and revised by the author.

I shall, for the purpose of fixing our ideas, project upon the screen before you, in the first place, two or three gaseous spectra of elementary metallic bodies which appear to exist in the atmosphere of the sun. These metals will be, successively, iron, nickel, and chromium. The proportion of iron in the atmosphere of the sun, as we shall see on a future occasion, is very considerable. It is also probable, but not absolutely certain, that nickel is there; and there appears to be no doubt that chromium is present. Iron, at first, will give a comparatively feeble spectrum. As the temperature rises, the particles of the iron will become more intensely incandescent, and the spectrum of the metal will proportionately increase in brilliancy. Iron is a metal which requires an extremely high temperature for its volatilisation; and therefore, until the charcoal points have become fully ignited, the bands are apt to appear more fitfully than is the case with more volatile metals, such as zinc and thallium, and some others which we have had occasion to examine already. The bright bands which are here prominent will be some of those to which I shall have to call your attention hereafter when we consider them in relation to the sun's spectrum. As the iron has now disappeared, we will introduce another magnetic metal—nickel. The spectrum of nickel is one in which we have certain bands in the green particularly prominent, and, like iron, nickel is a metal which requires a very high temperature for its volatilisation. Each of these metals is an elementary substance, so far as we at present know, and, as you see, is capable, in the intense heat of the voltaic arc, of becoming converted into vapour. We will next examine the spectrum of chromium. The chromium bands are also very characteristic; among them you will observe a number of brilliant lines in the blue. These three metals cannot be volatilised in the heat of such a flame as the Bunsen gas flame, which is produced by burning a mixture of atmospheric air with coal gas, although it is a very hot flame, and may be used to volatilise a large number of substances. Amongst others it volatilises the metals of the earths and of the alkalies and most of their compounds; but it does not volatilise the metals which belong to the class with which we are at present engaged. The spectra produced by the ignition of the elementary bodies are different from those furnished by the compounds of the same bodies. Many of these compound substances, if heated to a temperature not sufficient to decompose them, exhibit peculiar spectra. The number of compound bodies, however, which can be so converted into vapour in a common flame without undergoing decomposition, is comparatively small. Metallic sodium has its own peculiar spectrum, which I have already shown you; there is also a spectrum of potassium equally characteristic, and these appear when the compounds of sodium and potassium are decomposed by heat without special precautions. But if you volatilise the chlorides of these substances in an atmosphere of chlorine, the characteristic spectra of these metals will disappear altogether. This is an experiment that I do not venture to make here on account of the extremely irritating nature of the fumes. Indeed, I am not able to project upon the screen directly the spectra of compound bodies, and for a reason which will be at once understood. The temperature which is required for a sufficient ignition to enable us to project any spectrum upon the screen is exceedingly high, far beyond any that we can attain in our furnaces, so that the very act of producing a sufficient heat to render them luminous enough to be seen by a large audience would be attended with the actual separation of their constituents, and the destruction of the compounds themselves as compounds.

I shall, therefore, in order to give you some idea of the difference between the spectra of the elements and their compounds, ask you to look at a photograph representing the spectra of certain compound substances. I have selected the spectra produced by the compounds of copper, because they resist a higher temperature than most others without undergoing decomposition, and are consequently amongst those most easily observed. On this photograph you will see a representation of three spectra, placed one over the other. The uppermost is the spectrum of metallic copper; the second is the spectrum of cupric chloride, or the compound of copper with chlorine; and the third is that of cupric iodide, or the compound of copper with iodine. You will observe certain lines in the yellow and the green, which are pretty constant in them all. In the second spectrum, characterised by lines in the blue, we see the effect produced by the combination of chlorine with the copper; and this is again different from the spectrum of the iodide. When Bunsen and Kirchhoff first made their experiments upon the volatilisation of bodies in the flames upon which they experimented, they concluded that, whatever the temperature employed, the same spectrum was always produced by the same substance. That statement has since, however, been ascertained not to be absolutely correct. An elementary body may be heated through a very wide range of temperature without experiencing any change in its spectrum, but at a very high temperature new lines not previously observed often make their appearance. I am indebted to my friend, Mr. Crookes, the discoverer of thallium, for the loan of this diagram representing the spectrum of the metal at an intense heat, and also for some specimens of thallium itself. Here is a bar of thallium, the body of which I am now speaking, of between two and three pounds in weight. It is one of the rare metallic elementary bodies recently discovered by the application of this method of spectrum analysis. The ordinary spectrum of thallium exhibits a single strong band in the green; but when the electric spark is sent between two thallium wires, supported on a suitable insulating stand, if the light of this spark be examined by the spectroscope—an instrument the plan of which I shall presently have to explain—not only does the brilliant green band come out, but a series of others are produced, especially in the more refrangible part of the spectrum, in consequence of the intense heat of the electric spark. Several other bodies show the same phenomenon, and exhibit additional lines in the more refrangible portion of the spectrum when the temperature is sufficiently raised.

Lithium offers an example of this kind. This metal presents a singular characteristic line—viz., a brilliant crimson band—which is brought out at moderate temperatures; but there is another, a fainter line, in the orange, which requires a higher temperature for its development; and, lastly, a very brilliant band in the blue, which requires a still higher heat, such as that of the voltaic arc. I believe the first time this blue band was seen was in the theatre of this Institution at a lecture by Dr. Tyndall. I have here in one of these diagrams a representation of the spectrum of lithium indicating the blue line, which we shall presently see, in addition to the two lines in the red and the orange usually seen in the lithium spectrum. The orange line is, indeed, a little too bright in comparison with the other when seen by the gas flame. [The green line of thallium was produced.] That is the beautiful green line of thallium when heated in the voltaic arc; and if I could produce a sufficiently intense heat, by sending an electric spark between wires of this metal, to project the light on the screen, a large number of other lines, which are not now visible, would be developed in the still higher temperature thus obtained. Although in the voltaic arc we have a most intense heat, the temperature is not sufficient to cause the thallium to vibrate in such a way as to produce more than a single line. The lithium at the same temperature will acquire the power of producing an additional number of vibrations of increased frequency, so that we shall have at least three distinct bands—a band in the red, a band in the orange, and another in the blue. [Lithium spectrum shown.]

Hitherto I have taken the substances in their metallic state, but I wish to show you, in the next place, that if we take certain bodies in their compound condition we may separate their constituents from each other in the flame of the voltaic arc. For instance, if, instead of taking metallic barium, I take a compound of barium with chlorine, and heat that strongly in the lamp, the chlorine and the barium at that very elevated temperature will be separated from each other; the barium will be converted into vapour, and we shall obtain from the metal its characteristic series of bands, the chlorine spectrum being so faint as to elude our observation under these circumstances. [Barium spectrum from chloride of barium shown.] I must request you to bear in mind the position of these bands, as I am about to take another compound of barium in order to demonstrate the fact that we are here really dealing with barium itself. We before had the chloride. Now I take the carbonate of the same substance, and this carbonate will at the same temperature be resolved into barium on the one hand, which is the glowing gas from which that spectrum was produced, and into other bodies which give out comparatively little light, and which, therefore, occasion no interference with the results. [Spectrum of carbonate of barium shown.] You see the spectrum is not quite so bright as the last, for the reason that the carbonate is not a body which is so completely and easily volatilisable as the chloride. The chloride is a substance which, like most chlorides, is readily volatilised by a moderate heat. The carbonate is a body which, at a high temperature, becomes decomposed. It gives off its carbonic acid and produces baryta, and that baryta, in the focus of heat which we have here, is undergoing decomposition. Its oxygen is being separated from it just as, when oxide of mercury is moderately heated in a spirit of flame, we can separate that into its components—mercury on the one hand, and oxygen on the other. So it is here with baryta; in the intense heat of the voltaic arc, the barium and the oxygen are becoming separated from each other.

I want you, then, to draw a distinction—for it is a very important one—between the spectra of compounds and the spectra of simple bodies. It has been observed that the spectrum of a compound body generally exhibits a series of broad bands, whilst in the case of elementary bodies the characteristic spectra consist of sharp, narrow, bright lines.

I stated just now that when compound bodies were decomposed in the voltaic arc, their gaseous constituents, as they disappeared, produced little or no effect upon the spectrum; but you are not from that to conclude that a gaseous body—I mean a permanently gaseous body, such as oxygen or nitrogen—is not capable of producing a spectrum. It does give a spectrum, but one of very much less brilliancy and intensity of light than metallic bodies such as I have been showing you. I wish to make this manifest, but I cannot throw the spectra of these gases on the screen. They are so very faint that, if I were to attempt to produce them, they would be invisible at a distance. But I must show you an ingenious way in which this difficulty has been overcome by Plücker, who has succeeded in obtaining such spectra in a form in which he could analyse their light by the prism; and thus he has not only been able to distinguish one gaseous body from another, but even to measure the distances between the different lines of each and make maps of the spectra of these various bodies. Here I have four glass tubes, each containing a different elementary gas. These tubes have been made in a particular manner, in order that we may be able sufficiently to concentrate the heat of the electric spark in its passage through them to render the gas luminous. Each of these tubes consists of two somewhat wide portions separated by a narrow thermometer-like tube in the middle. At each end is a platinum wire melted into the glass, and there is a narrow tube at the side for the purpose of introducing gas, and then removing more or less of it, as may be necessary, by connecting this tube with an air-pump. After the glass tube has been exhausted sufficiently it is melted off, and the vessel remains permanently charged with a quantity of gas for examination. When the experiment is to be made, the platinum wires are connected with the terminals of the secondary wire of a Ruhmkorff's coil, from which the induction spark is transmitted through the tube. You will see how different the appearance of the spark is when it passes through the wide part of the tube from that which it exhibits in passing through the narrow portion. In the latter it is much more brilliant, and the temperature is higher. It was by an arrangement of this kind that Plücker was enabled to increase the brilliancy of these spectra sufficiently to analyse their light by means of a prism. Here are hydrogen, nitrogen, chlorine, and iodine. You will observe that I am showing you the entire light caused by passing the spark through these gases. Every one of these has a special light of its own. Here is the red light of hydrogen; the violet light of nitrogen; the third tube shows the peculiar light of chlorine, while the light of iodine in the fourth is quite different from any of the others. Each of these luminous lines, when viewed through a prism, is seen to consist of bright lines, as in the case of the metals. I cannot show you their spectra; if I could, I should be very glad to do so, because they are exceedingly beautiful.

Here let me say that, beautiful and brilliant as are the spectra which I throw upon the screen, they are totally unfitted in this form for examination from a philosophical point of view. The object of these experiments at the present time is to show distinctly and in a broad way the differences which exist in the spectra of different elements; but the philosopher, in examining these in his closet, has not merely to see that they are different, but he has to measure with great precision the intervals between each of these bars of light; for you will see that here, as in the notes of music, each one of these lines has its own special position in the scale, and we must know exactly what that position is in order to be able to recognise it again, so as to connect it with the substance by which it is occasioned. The eye has no means of comparing these differences as the ear compares sounds, but we are obliged by angular measures to determine the position of these lines with regard to certain fixed points. More of that, however, by-and-by.

Before I quit this part of our subject, I wish to show you that in particular cases the same gas may give two different spectra. If you send an electric spark through one of these exhausted tubes of nitrogen at a low temperature, you will obtain a spectrum of a particular kind, but the same gas will exhibit at a higher temperature a spectrum which is quite different. This is one of the most important discoveries with reference to gaseous spectra which Plücker has made. Here is a nitrogen tube arranged on a whirling table for imparting to it a rapid rotary motion. Nitrogen is a substance which, at a low temperature, emits a golden yellow light, and this, when examined by the prism, is seen to consist of a series of bands in the less refrangible part of the spectrum. If the temperature is raised—as by sending a spark from a large Leyden jar through the tube—the light becomes of a bluish or violet colour, and then the character of the spectrum is seen to be entirely altered. I can only show the alteration in the colour and duration of the light at the two different temperatures. But as I cannot show you the spectra of the gases upon the screen, I have endeavoured to obtain a substitute by employing the aid of the photographer; and if this photograph be placed before the lamp we shall be able to project upon the screen a representation of the different spectra which Plücker has figured. I shall give you first the series of bands which are produced at low temperatures, directing your attention first to nitrogen, a material which is of special interest to us, inasmuch as it is the most abundant constituent in our atmosphere. You will observe particularly the spectrum of nitrogen, and notice the particular way in which the bands are distributed. It is found that these bands are made up of fine lines closely aggregated together. The spectrum becomes more feeble towards the refrangible end, and at this point in the violet it suddenly ceases. This is the nitrogen spectrum at the lowest temperature. Plücker and Hittorf (who made their experiments together) call this the nitrogen spectrum of the first order. I now wish to show you, in contrast to these lines, the effect which is produced by sending through the same gas a spark at a high temperature; here we have a series of brilliant bands so produced, forming a spectrum quite different from that of the same gas at a lower temperature. So also in the case of sulphur the spectrum at a high temperature is of a very different nature from that which the same body exhibits at a lower temperature. This is true also of selenium, though I have not the spectrum of selenium at the lower temperature to exhibit to you. These cases, in which the character of the spectrum changes with the temperature, are the exceptions. In the case of oxygen there is no such change, neither is there in the case of phosphorus, chlorine, iodine, bromine, and arsenicum.

These differences are very important when viewed theoretically, though as yet we have no satisfactory explanation of them. It has been supposed that where two spectra occur the bands formed at a low temperature are produced by a substance which is really different from that which gives rise to the bands which are given out by what appears to be the same body at an intense heat. It has been conjectured, though it is by no means proved, that substances which, like nitrogen, sulphur, and selenium, give two different spectra, are not elementrary bodies, but that at the high temperature they are actually separated into their components, just as we recently separated chlorine from barium, and carbonic acid and oxygen from the same metal. But this conclusion is by no means well established, because it is seen that the moment we cease to pass the electric spark, the whole thing is as it was before; and if the substance has been decomposed by the intense heat, it has been as instantly recomposed on the diminution of temperature. Professor Stokes has suggested that the degree of rapidity in the vibration of the particles of the substance is connected with the different duration of the electric discharge in the two cases, and that a higher intensity of the electric spark gives to the nitrogen, for instance, the power of producing a higher series of vibrations than it can furnish when heated for a longer period to a lower degree. At present, however, that is a point for further investigation. We do not know why it should be that certain elements, if they be elements, should be thus altered, and certain others should not experience a like change under circumstances apparently similar. It is, however, an extremely curious fact, and has an important bearing upon the application of observations of this nature to the interpretation of astronomical and other phenomena.

There is another fact with regard to the spectra of gases which, as I am now upon that subject, I may mention here. Plücker observes that, although in the case of solid bodies you may gradually distinguish the presence of small quantities in admixture with others, it is not easy to do so in the case of gases. You may have a notable quantity of one gas added to another, and yet the spectrum will be only that of the predominant gas. It is not so in the case of solids. For instance, if you heat an alloy of gold and silver by the electric spark, the gold gives a spectrum in which you have also the characteristic lines of silver superadded, although the proportion of silver may not exceed one part in a hundred.

I wish to show you, if I can, the effect that is produced by examining two spectra at the same time. My object now is to give an idea of the principle upon which two different spectra are compared with each other. You must, if you please, be indulgent to me if I should not succeed, because I believe this is the first time this has been attempted before a public audience. I have here two lanterns, and I wish so to arrange them as to throw upon the screen at the same instant two spectra passing through the same lens and the same prisms. [The spectra were produced as desired.] These are the spectra of lithium and of strontium; and I have selected these two, because in each case the colour they communicate to flame is so similar that they are not capable of being distinguished from each other, if viewed without the aid of the prism. Both bodies tinge the flame of a brilliant red colour. But by means of the apparatus which we have here, we see at once that the position of the lines in the two cases is quite different. We have the crimson line of the lithium on the edge of the screen. Then we get the orange line and the blue band as the temperature rises, and you will observe that there is a blue band given by the strontium, very near the same position as that of the lithium line. It is so near that when it was observed for the first time in this theatre the remark was made, "Oh! that must be a mistake: you have taken strontium instead of lithium!" But you see when we compare the two, there is no doubt whatever about the difference. Mr. Fox Talbot was the first to point out the fact, more than thirty years ago, that lithium and strontium compounds could be immediately distinguished by the aid of the prism.

The apparatus which we use for the purpose of comparing spectra for philosophical purposes is not liable to any of the uncertainty which an extempore arrangement for projection on the screen is liable to entail from the overlapping of the two spectra. In the spectroscope, when properly arranged, the two spectra are presented to each other edge to edge with the greatest possible accuracy, and I must now endeavour to show to you how it is that this measuring apparatus is used.

We want, for example, to make an examination of the residue of a water which has been boiled down, and we wish to ascertain if, among other things, there is any strontium present in the salts. For this purpose we make an experiment precisely similar in principle to that which I made just now upon the screen. We take a salt of strontium, and we arrange it so that, by means of the apparatus at our disposal, we can transmit the light from the strontium flame through the instrument. It enters the tube at a narrow slit, and passes along it until it falls upon a lens. Placed at just such a distance from the slit as to render the beam of light parallel, the sheaf of parallel rays immediately falls upon the prism behind the lens; and when the light comes out on the other side of the prism, it is separated into the coloured bands of strontium. The observer does not throw these upon a screen like the one we have been using, but upon a far more sensitive screen at the back of his eye—the retina. To bring these lines to a focus, there is a small telescope arranged behind the prism, fitted with a sliding tube for adjusting the focal distance. The light from the other flame also passes through the same slit. If I introduce a little salt from some of the Bath waters, for example, into this flame, I immediately colour it. We place the second flame opposite to a small right-angled prism, which is so small that at a distance you do not see it in the instrument; but it is so arranged that it shall cover just half the slit. In this case the prism is used simply as a reflector, which directs the beam down the axis of the tube. We have, then, two beams passing through the same prism and the same telescope, and falling upon the eye at the same moment. They are so adjusted, however, that their spectra shall meet edge to edge. If we do this, then we have a means of making the spectrum of the strontium compare itself with that of the substance we place in the second flame.

If I take, as I will do now, a little of the residue from the Bath water, and make its spectrum fall upon the screen, we shall have a very complex image. There is the yellow line of sodium; there are green bands; I believe there is a little strontium; and there are the bands of calcium. Here is a line which is very like the lithium line. We will put some lithium into the other lamp, and see whether that gives the same line. We can compare spectra in this way one with the other; and if the bodies we introduced into the second flame are present in the original substance which is being examined, certain lines of its spectrum will run into each of the lines of the spectrum with which it is being compared, thus proving that the body compared is present in the original substance. In other words, we take a substance the composition of which is known to us, and we compare its spectrum with the spectrum of a body the composition of which is unknown to us.

Each of the earths has a spectrum of its own, which I would show you if time allowed, but as that is not the case, I must make the best approach to it that I can by exhibiting a photograph of the spectra of those bodies.

This is the characteristic line of potassium—a double line in the red. Then there is a diffused light in the middle, and here we have a blue or violet line near the most refrangible extremity, indicating potassium. Potassium is not distinguishable in such small quantities as some other metals. Here is the spectrum of rubidium, a metal which was discovered by means of this method of analysis. Bunsen was examining the water of the Durkheim spring, and he found he had a double red line which he had not seen before, and some lines in the blue which he had also not seen before. These were the lines of substances which he had not previously met with; and although in the original water only between three and four grains of the substances were present in a ton of water, yet, relying upon the accuracy of the indications which he had obtained in the spectrum, he procured a large quantity of the water, boiled it down, and succeeded in isolating these two bodies. One of them he called rubidium, from the occurrence of these red lines, and the other cæsium, so named from the occurrence of two bright lines in blue. In addition to these, two other metals have also been discovered by the aid of the prism; one of them—thallium—I have already mentioned, and you have seen the green line by the occurrence of which the metal was discovered by Mr. Crookes as he was examining a particular substance, of which he possessed but a small quantity. The difficulty then was, first, to find out in what minerals it existed, and next to devise chemical means of obtaining it. These difficulties have been overcome, and it can now be procured in considerable quantities. I have seen masses of it weighing thirty pounds. I have quite recently heard that carbonate of thallium has been introduced by M. Lamy into the preparation of glass, which is said to be superior to any hitherto used for optical purposes.

The short remainder of the hour shall be devoted to the examination of the spectra of these metals, which were originally discovered by the aid of the spectra themselves. We will first project upon the screen that of cæsium. You will probably see several lines, but the characteristic lines are those two blue lines now visible, one of them considerably brighter than the other. These are the lines more particularly distinguishable at low temperatures. There are also a number of lines in the red. In showing you these bodies in the voltaic arc, I am working under considerable disadvantage, because, at so high a temperature, the spectra are much less simple than they are at lower temperatures. At high temperatures many of these bodies, as I have said, acquire the power of vibrating with different degrees of velocity, in consequence of which additional bands corresponding to these new velocities of vibration are developed at a high temperature, but they do not lose the power of vibrating with the definite velocities which they acquired at a lower temperature; they therefore preserve the bands originally seen, as well as those produced by the intense heat applied.

We will now throw upon the screen the rubidium spectrum, using the chloride of the metal. In this case the chlorine is separated from the rubidium. If the spectrum is a good one, we shall have a double red line upon the less refrangible edge of the spectrum. Here is the rubidium line, accompanied by the paler bands in the blue. The double red line is the characteristic portion to which the body owes its name, from *rubidus*, dark red. This is very different from the spectrum of cæsium, where the blue lines were particularly prominent. The brightness of the blue lines in rubidium is not to be compared with that of the line in the red.

I shall conclude by showing you one of the latest fruits of spectrum analysis—a substance characterised by two remarkable lines in the blue. The name of "indium" has been given to it, because it gives a light of an indigo-blue colour. These bands are at the violet end. Here is the spectrum of this body. I am indebted to an old friend, Professor Varrentrapp, for this sample of indium, who has sacrificed half his specimen in order that I might be enabled to show you these bands. It is a substance scarcely to be obtained, although in the Paris Exhibition there is a mass of upwards of a pound weight of it, but it is there at present in prison as an exhibit.

ROYAL DUBLIN SOCIETY.*

At the last evening meeting of the Royal Dublin Society, Dr. Emerson Reynolds read a paper upon "*An Isomer of Sulphocyanogen.*" If the new body which he has discovered proves, on investigation, to be really an isomer of the theoretical radical, Scy, the communication will be one of the most important contributions received for some time in connexion with organic chemistry. The author, having referred to the fact of sulphocyanogen never having been isolated, dwelt at some length upon the different views that had been taken by workers with the subject, particularly as regards the composition of the sulphocyanides. These views may be enumerated in a few words. 1. We have the radical theory. In this theory, the existence of the salt-radical, CyS, is admitted, and we may look upon the ammonium salt as NH_4,C_2NS_2. This molecular arrangement is the one that is generally accepted. 2. The composition may be viewed as a sulphide of ammonium combined with sulphide of cyanogen, thus—NH_4S,C_2NS. 3. The body may be viewed as sulpheretted urea, or as a sulphocarbamide. The relation to urea is represented in the following formulæ:—

$C_2N_2H_4S_2$	$C_2N_2H_4O_2$
Hydrosulphocyanic acid.	Urea.

Gladstone (*vide* Watts's Dictionary, vol. v. page 505) has also noticed that sulphocyanic acid, together with urea (carbamide), is formed by the action of sulphuretted hydrogen on ammonio-cupric fulminate. Dr. Reynolds referred to all these views, but stated that the first was the accepted one.

Now, when sulphocyanide of ammonium is submitted to destructive distillation, it is split up, according to Liebig, into bisulphide of carbon, sulphuretted hydrogen, ammonia, and a residual substance, which that chemist has named melam. This latter substance is afterwards converted into hydromellone—

$$8(NH_4,C_2NS_2)=4CS_2+8HS+5NH_3+\underset{\text{Melam.}}{C_{12}N_{11}H_9}.$$

$$C_{12}N_{11}H_9={}_2NH_3+\underset{\text{Hydromellone.}}{C_{12}N_9H_3}.$$

When, in the presence of water, sulphocyanide of ammonium is submitted to the action of heat, Dr. Reynolds says that it gives

$$NH_4,C_2NS_2+HO=\underbrace{C_2NS_2H}_{\text{Hydrosulphocyanic acid.}}+NH_4O.$$

But he finds that heat, in the absence of water, produces

$$NH_4,C_2NS_2=NH_3+H+\underset{\text{Isomer.}}{C_2NS_2}.$$

The latter is purified by recrystallisation. It is a very stable compound, which will bear a considerable temperature without decomposition. The analysis gave figures that corresponded to C_2NS_2.

The following compounds were described:—

Bichloride of platinum gives a red crystalline precipitate, having the following composition—$C_2NS_2,PtCl_2$. It will be

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* Specially reported for the Chemical News, by Charles R. C. Tichborne, F. C. S., etc.

seen at once that this compound does not follow the type of the ammonium salt, but accords with the composition of urea.

Nitrate of silver forms a compound, a direct combination of the salt with the isomer.

There is also a mercurial salt, $C_2NS_2,HgCl_2$.

The author said, in conclusion, that we might naturally expect to find an isomer of sulphocyanogen, as such changes seem to be a natural property of the cyanogen compounds. Thus, he gave as instances the three chlorides of cyanogen, and the paracyanogen procured on submitting cyanide of mercury to destructive distillation.

As the author used the old notation, we do so in our report.

QUEKETT MICROSCOPICAL CLUB.

Friday, May 24, 1867.

Mr. Ernest Hart, *President, in the Chair.*

The ordinary monthly meeting was held at University College.

A paper was read by Mr. M. C. Cooke "*On Binocular Vision.*"

The **President** read a paper "*On the Minute Structure of the Iris and Ciliary Muscle,*" in the course of which he demonstrated the structure and direction of the ciliary or accommodative muscle of the eye in man, ruminants, and birds, and showed that there are presented no circular or sphinctral fibres in the latter, and discredited their existence in the former. The paper was illustrated with enlarged diagrams, and numerous injected specimens under the microscope. The meeting, which was fully attended, terminated with a *conversazione.* Ten members were elected.

ACADEMY OF SCIENCES.

May 27, 1867.

M. L. D. **Girard** presented and described a new ball governor, giving perfect isochronism and acting instantaneously upon the steam valve. He maintains the balls in any position by giving to the motor a constant velocity, and also an angular velocity equally constant. He proves by calculation that the four-balled regulator, when in equilibrium, is isochronous; that it does not follow the mathematical laws of the conical pendulum of Watt; that it can be applied without any change to all machines.

M. Felix de Luca placed upon the table in the name of his brother Dominique de Luca, director of the ophthalmic section of the "incurable" hospital at Naples, a note "*On the Employment of Sulphate of Soda in the Treatment of Spots on the Cornea.*" After having mentioned the inefficacy, inconvenience, and at the same time the dangers of the known methods—laudanum, alcoholic or tannic liquids, etc.—he thinks that crystallised sulphate of soda, by reason of the property it possesses of maintaining in solution the fibrine of the blood, can exercise a favourable action on spots in the cornea. In the first experiments use was made of an aqueous solution of sulphate of soda saturated in the cold; it was let fall drop by drop on the eyeball. The spots diminish in extent, but with an excessive slowness. For the solution M. de Luca substitutes the sulphate reduced to a fine powder, which he lets fall by pinches on the eye twice a day, the head resting nearly horizontal. The salt is dissolved by the humours of the eye, at the same time producing an agreeable sensation of cold. At the end of a few days the spots commence to disappear, and the patients, who could not see at all, distinguish the movements of the hands and fingers, and the blindness soon ceases.

CHEMICAL SOCIETY.

Thursday, June 6, 1867.

Dr. A. W. Williamson, F.R.S., *Vice-President, in the Chair.*

The minutes of the previous meeting were read and confirmed.

Sir Benjamin C. Brodie, Bart., Professor of Chemistry in the University of Oxford, then delivered the following lecture:—

"*On the Mode of Representation afforded by the Chemical Calculus, as contrasted with the Atomic Theory.*"

Mr. President,—I feel that I have undertaken this evening a truly difficult task, which is to give to the Chemical Society, in the brief space of one hour, an account of a somewhat abstruse and difficult subject, the exact comprehension of which requires that it should be minutely considered in all its details. I should not, however, shrink from this, if I did not feel that the subject is really before those who are most competent to judge of it, in a somewhat imperfect form; that I have as yet offered to the chemical world the first part only of the method of which I am about to speak; and that this method will be much better comprehended, both from a mathematical and chemical point of view, when you have before you the subsequent parts which I hope to present hereafter.

I am to speak of a method of representing the facts of chemistry, which is fundamentally different from the method at present in use. Let me say a few words upon the past history of chemical theories.

I believe that theory is essential to the existence of chemistry. The birth of the science was inaugurated by the construction of a definite theory of chemistry—the first theory which had ever been proposed, and which sought to give a definite and rational account of the facts of the science. This theory was the once world-famous doctrine of Phlogiston. In this theory the facts of chemistry were explained by the agency of a subtle, hypothetical, all-prevading principle, by the transference of which, from one chemical substance to another, it was assumed the facts of chemistry were correctly accounted for. It is easy, from our present point of view, to pass critical remarks upon the doctrine of Phlogiston, but it is not quite so easy really to comprehend that doctrine, and to put ourselves in the position of those great chemists who worked and who studied through its agency. If ever any one was tempted to speak slightingly of the doctrine of Phlogiston, let him remember that through the instrumentality of this doctrine the great discoverer of chlorine, the chemist Scheele, worked. Let him remember that the exact mind of Cavendish was contented with this doctrine. Let him remember again that the illustrious Priestley, that transcendentally inventive genius, in possession of this doctrine, made the great discovery of oxygen; and that not only was he then content with this doctrine, but that he died a firm believer in and adherent to it. However, the doctrine of Phlogiston, like many human things, was destined to pass away,—Lavoisier shattered Phlogiston. For no inconsiderable period after this, chemists appear to have worked, if I may so say, without a theory; that is to say, that, as during the long alchemical ages chemists were occupied in collecting together those facts which were afterwards to be embodied in the theory of Phlogiston; so for a period of above thirty or forty years—that is to say, from the time of Lavoisier to the time of Dalton—chemists were employed in collecting together that exacter system of facts which was to form the basis of a far wider, a far more comprehensive, and a far nobler theory, namely, the great atomic doctrine. However, Davy appears to have worked and to have made his great discoveries without a theory. Davy never admitted the atomic theory, but rested content simply with the facts of numerical analysis.

In the year 1808 there appeared that famous book, "A New System of Chemical Philosophy," which contained the germs—indeed, I may say, almost the full development—of the atomic theory itself. In this atomic doctrine Dalton took up the conception of combination, which was introduced into the science by means of the theory of Phlogiston. He took up that doctrine of combination, and moulded it into a new and a more definite form. It would be useless for me, before the Chemical Society, to dwell upon the atomic theory. It is a theory with which every one is familiar, for every chemist of this day has worked with that theory, has conceived his science from the point of view of that theory; and, indeed, I believe it is, in the opinion of many, almost impossible that that doctrine should ever fall to the ground. This doctrine of Dalton, however, was a doctrine far more audacious than that of Stahl. In the theory of Phlogiston, Stahl considered that he had palpable evidence of the transference of his Phlogiston from chemical system to chemical system; but Dalton told us that this notion of the continuity of matter—that obvious fact which our senses teach us—was simply an illusion of the senses, and that, if only we could see things aright, we should see that this world, which appears to us so connected and so continuous, was really made up of an almost infinite number of disjointed fragments.

From the point of view of the atomic theory, I say, chemists have worked for a period now of about sixty years, and the progress of chemical theory has consisted in the almost constant and unremitting development of this doctrine. I cannot say, however, that this has been an unremitting progress. It has rather been a succession of events. System has followed system, doctrine has followed doctrine; but these doctrines have, one after another, fallen to the ground. We have had but little that is permanent, and at the present moment the theory of chemistry is built upon the ruin of other theories. Now, no one can have more respect or more admiration for these great ideas, which were thus ushered into the science by Dalton, than I myself have. It cannot be necessary for me to express to this Society the admiration which I feel for that theory; but, nevertheless, I cannot but say that I think the atomic doctrine has proved itself inadequate to deal with the complicated system of chemical facts, which has been brought to light by the efforts of modern chemists. I do not think that the atomic theory has succeeded in constructing an adequate, a worthy, or even a useful representation of those facts. I say that for sixty years the united efforts of chemists, including many of the most able men in the world, have been devoted to the development of this doctrine, and they have formed their representations upon this doctrine. Now, let me read to you an account of the last modern representation of the atomic doctrine, and the chemical symbols in which the atomic doctrine has resulted. I will read to you a paragraph headed "Glyptic Formulæ;" it is given in a scientific journal. Here is the paragraph:—

"Those teachers who think, with Dr. Frankland and Dr. Crum Brown, that the fundamental facts of chemical combination may be advantageously symbolized by balls and wires, and those practical students who require tangible demonstration of such facts, will learn with pleasure that a set of models for the construction of glyptic formulæ may now be obtained for a comparatively small sum." (Much laughter.) "At first sight, the collection of bright-coloured and silvered balls suggests anything but abstract chemical truth,"——

And so on. However, I will tell you what you may get for your money:—

"There are seventy balls in all for the representation of atoms—monads, dyads, triads, tetrads, pentads, and hexads, being distinguished by the number of holes pierced in the balls. To connect these into rational formulæ"—[which, I confess I should think was a truly difficult problem]—"brass rods, straight or bent, and occasionally flexible bands are employed." (Laughter.)

And so on. However, the editor seems to have had some misgivings, for he proceeds to say,—

"Whether they are calculated to induce erroneous conceptions is a question about which much might be said."

Now, however much might be said upon this subject, I certainly am not going to say a great deal to the Society upon it; but it is truly a remarkable fact, that the atomic theory, after so many efforts, has resulted in such a symbolical representation as this. I think that great injustice is done in connecting the names of Dr. Frankland or of Dr. Crum Brown specially with such ideas as these, for I cannot but say that I think the promulgation of such ideas—even the partial reception of such views—indicates that the science must have got, somehow or another, upon a wrong track; that the science of chemistry must have got, in its modes of representation, altogether off the rules of philosophy, for it really could only be a long series of errors and of misconceptions which could have landed us in such a bathos as this.

You may, however, ask me, and with reason, "In what way, then, are we to represent the facts of chemistry, if we are not to represent them in this way? Do you mean to deal with this complicated system of facts, and to offer us no mode of representing these facts, and no mode of conceiving these facts?" Now, I certainly believe that any person who seriously attacks these ideas, is bound to show some other, and, I will say, some better way of representing the facts. I think he is bound to do this, or he should refrain from his attacks. You ask me how we are to represent the facts of the science. It is to that question that I wish to offer an answer to-night.

I say that we are to express the numerical facts of the science by means of symbols; but I attach to the term "symbol" a very special signification. We have plenty of what are called "chemical symbols" already; but these chemical symbols are not, from my point of view, symbols at all, and you will presently see why. Not only according to my ideas, but according to the ideas of most persons who consider this question, a symbol may be regarded as a mark by which we express the objects of our thoughts for the purpose of reasoning about those objects; and one which is capable of being combined with other similar marks according to certain definite laws of combination; which laws of combination are to be possible, through the interpretation of the symbol, in the subject matter which is symbolized. That is what I mean by a symbol.

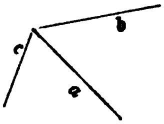
You will readily see that our present notation really can hardly be called, even in courtesy, a symbolic representation. The reason is, in the first place, that these letters are not capable of being combined with other letters, or other marks, according to any definite laws at all; and, in the second place, so far are they from having any definite signification or meaning attached to them, that every chemist thinks himself at liberty to deal with them just as he pleases, according to his fancy. Now, I say, I wish to put a restriction upon that mode of dealing with the subject, and to bring my fellow chemists and myself under some definite laws when they deal with symbols.

Symbols are of two kinds. We may have symbols of things, and we may have symbols of operations. Symbols of operations are simply symbols of what we do to things. Take a popular case; ordinary language is an imperfect symbolic system, and here we have just these two kinds of symbols. A "dog" is the symbol of a thing, and "beating," "caning," "coaxing," and so on, are the symbols of operations, or of something which we may do to a dog. We have marks by which we express things and marks by which we express what we do to things. We might also have a third kind of symbol; we might have the symbol of an operation and a thing together. Thus, if we did not wish to represent particularly what sort of animal we were going to beat, we might have a single mark for "beating an animal;" the thing and the operation being included in one.

I purpose, however, to go into a more exact kind of symbolism; but before I commence my explanations, I should like to remove one or two popular errors upon this subject. I believe there is no error more ingrained in the popular mind than that these marks $+ \; - \; \times \; =$ are the symbols of adding, subtracting, multiplying, and identification or equalization; I mean that these marks are purely arithmetical symbols, and are to be used for the purposes of arithmetic alone, and that in any other subject matter to which they are applied it is essential for us to give these symbols their arithmetical signification. If that were true, the application of symbols to the science of chemistry would simply be, from my point of view, an impossibility.

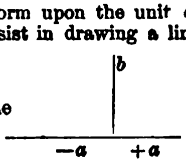
Perhaps I shall best illustrate this matter if I give you, from another subject, an example of the mode of constructing a symbol, and what we mean by a symbol. It is an example which will bring before you clearly how independent symbols are of their arithmetical meaning or interpretation. I say of their arithmetical *meaning*, not of their arithmetical *laws*, which is another thing. In ordinary algebra we denote, by the mark a, the operation of conferring upon the unit of length a certain length which we designate as a. This length we may call three feet, and the mark a will thus stand for a line three feet long. Now, if we take another symbol, b, that may indicate to us a line drawn in the same direction as a, but of another length. We will say that b is five feet. Now a tells us that we are to draw a line of a certain length; and we may say that the symbol $+ a$ means that we are to draw it in a certain direction. Now if we ask what is the meaning of $+ a + b$, this indicates to us that, having drawn a, we are to start again, and we are to draw another straight line of the length of five feet, which we call b. Or, in ordinary geometry, $+ a + b$ would symbolize to us a line, the length of which was the sum of the length of a and b, and drawn in the same direction.

I wish now to bring before you, very briefly, an illustration of how totally unnecessary this arithmetical application of the meaning of the symbol $+$, is to its algebraic meaning. We have another kind of geometry, we may say, in which the symbols a, b, c, and so on, may indicate to us not only length, but direction also; so that if we take a certain point as our starting point, the symbol $+ a$ would indicate to us that we were to start from the point a and draw the line $+$ in a certain direction—we will say towards the horizon. And b would indicate to us that we were to draw a line in another direction, and of another length; and c that we were to draw a line in a third direction, and also of another length. Thus

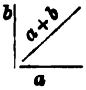


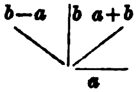
in short, it is open to us, if we choose to do so, to express by letters, not magnitude only, but also position.

Now, I wish you simply to see at what we arrive by following out these principles. What is the interpretation of $a + b$? a tells me to draw a line from our starting point in a certain direction and to a certain length. $a + a$ indicates that I am to make a line in the same direction as a and twice as long; in like manner — would also indicate to us the direction in which we were to draw our line relatively to the starting point. $-a$ would be a line equal in length to a, but in the opposite direction to $+a$.

Now b tells us that we are to perform upon the unit of length an operation which is to consist in drawing a line in another direction. Here is our line 

What therefore do we mean by $+a + b$? Why $+a$ tells us that we are to construct the line a. And having done that we are to go on again and construct b. That is done by beginning again at the end of the line a, and drawing a line in the direction of b, and equal in length to it, by which means we get 

If we draw the diagonal of the parallelogram of which a and b are the sides, the diagonal of that parallelogram is expressed, and properly expressed, as $a + b$ 

And I say that a line drawn through the first point, equal in length to the other diagonal of the parllelogram but in the opposite direction, is properly represented by $b - a$ 

Those two diagonal lines in the system of geometry express $a + b$ and $b - a$. The reason of this you will perceive is very obvious, for, as we all know, the diagonal line, relatively to direction and to the motion which makes it, would be the same in kind and in quantity as the motion which constitutes the lines a and b. In short, we first of all construct the line a, and then we go on again and take up the line b. The diagonal of the parallelogram is therefore properly expressed as $a + b$, but of course this diagonal is not equal in length to the sides of the parallelogram.

There is one other property I must refer to, which is very important, and comes out to us in the symbol $+$. It is, that $a + b$ is the same thing as $b + a$. Why is this? It is simply that when we go along the line in direction a, and then travel through the length of b, we arrive at the same quantity as when we go along the lines $b + a$.

In constructing any calculus or method, then, the principle to be observed, in regard to the symbols, is by no means to give to them their arithmetical meaning or interpretation, but simply to construct them properly according to the laws which they obey in arithmetic and in algebra.

To take another example, using the marks $+$ and $-$ as the simplest illustration. They may be regarded as marks which are subject to a certain system of laws, which laws are given in the following equations:—

$$+ + = +$$
$$+ - = -$$
$$- + = -$$
$$- - = +$$

If, then, you can find in any subject matter, any properties to which you may apply these symbols $+$ and $-$ consistently with this interpretation, I think you are justified in using the symbols $+$ and $-$ to express those properties.

Let me proceed to explain, very briefly, what I mean by a chemical symbol. The object, I should say, of the first part of this method is to discover a proper system by which we are to express the unit of chemical substances. I may put this in another way, and say that we wish to discover what is the nature and the number of the operations by which chemical substances are made or constructed. This is the first object of our method. I should, perhaps, limit myself a little farther, for I should say that before we begin to think about chemical substances at all we should conceive of them as all brought into the condition of perfect gases. Now, the reason of this is one which I am sure will commend itself to every chemist: it is the simplicity of the laws to which gaseous combinations are subject, which simplicity was first discovered by the great chemist Gay-Lussac. Of course we may deal with the properties of the combinations of solids and liquids, but here it is far more difficult for us to arrive at any intelligible and simple results; and, whether rightly or wrongly, before beginning to think about the nature of a chemical substance, I, for my part, always conceive it as brought into the condition of a gas. And to go a little further, and to speak a little more definitely, we shall always consider the chemical substance brought into the condition of a gas at the temperature of 0 degrees, and at a pressure of 760 millimetres. This is the sort of ideal chemical world with which we have to deal. It is a world of gases.

First let me indicate to you the definition which I will take of a unit of matter; for it is absolutely essential, before we think about matter at all, to begin with defining the unit which we are about to consider. That definition is of such great importance that I have had the words placed up before you in this diagram.

The unit of ponderable matter, is that portion of ponderable matter which, at a temperature of 0 *degrees, and at a pressure of* 760 *millimetres of mercury, occupies a space of* 1000 *cubic centimetres.*

From considering the unit of matter, I pass now to the consideration of a unit of another kind, and that is what I call the unit of space—that is, the volume of 1000 cubic centimetres. And just as, before we begin to think about chemical substances, we must bring them all theoretically to the state of gas; so, before beginning to think about the unit of chemical substances, we must begin by thinking about a unit of space. This is the fundamental conception of this method; and it is a notion which appears to me to be almost essential to any constructive chemistry at all—that is, the conception of the unit of space; let us, therefore, clearly understand what the unit of space means. Now, that there may be no doubt about it, I have brought you the unit of space [exhibiting hollow cube with glass walls, and of the dimensions above assigned to the unit of space].

You have to do something else, however, before you get the unit of space. It is indeed the space of 1000 cubic centimetres which is confined within these glass walls; but before you can get at the unit of space, you have to go a step farther, and by the process of imagination, or by the efforts of reason, you have to divest this cube of glass of weight, and take out of it all the ponderable matter which it contains, and conceive the space within the walls divested of matter altogether. Now, this unit of space is so fundamentally important to us that I shall begin by giving it a mark to itself. The mark which I give to that unit of space is, for certain good reasons which I will not explain now, the mark 1. When you see that mark, it is to recall to your mind the matter contained in the unit of space. Now, what is that matter? Why, that matter is simply no matter at all; there is no ponderable matter in it.

Perhaps, however, if I were to speak a little more exactly and precisely, I should say, for the benefit of those persons who may be more philosophically inclined, that the mark 1 is the symbol of the operation of taking the unit of space as it is. That is, take the unit of space as it is, and do nothing at all with it.

However, we must not only consider units of space, the consideration of which alone would lead us to very little, but we are going to consider the units of matter. Now, how are we to conceive a space becoming matter, or of matter getting into space—chemically, I mean? Well, I shall think of this through the aid of an operation, and I shall define by a mark the operation by which this empty unit of space is turned into a unit of ponderable matter. For example, I will take x as such a mark. This is the mark of the operation by which the unit of space becomes a unit of ponderable matter. It is the mark of a certain density which is appropriated to x, and of a certain kind which is also supposed by x. Here, then, is x, the symbol of the operation; and how are we to symbolize the performing this operation upon the unit of space? I shall do this in a natural manner by writing the letter x before the unit of space, $x1$; and that indicates to me matter of a certain density, and at 760 millimetres pressure.

How are we now to conceive a matter, double the density, but the same in kind as x? Having once conferred upon the unit of space this density, we have only to perform the operation a second time. Hence, to double the density, we have only to write x again, thus: $xx1$. This will symbolize that we confer on the unit of space a certain density, and having done that we confer that density on it again. That is, we make it double the density. $xxx1$ will mean that we give it three times the density. We can abbreviate the expressions. We need not write the x's out at length. The unit of space is 1; with the first density conferred upon it, it becomes $x1$; with double the density, $x^2 1$; and with three times the density, $x^3 1$. If you compare these operations with the symbols which express the densities, you will see that the symbols of the units of matter which we have thus constructed, stand to the numbers which express the densities of that matter, in the same relation as numbers do generally.

We will now take another kind of metter: 1, $y1$, $y^2 1$, $y^3 1$. This, again, would be a symbolised ponderable matter which would be contained in this glass box at the pressure and temperature indicated, of the kind indicated by y, and of the density indicated by the number of units of y. You will see this more obviously when we come to speak of the symbols of chemical substances.

If we proceed farther upon the same principles, we come to consider what is the symbol of units of space containing two kinds of matter. Well, on the same principles, you see, we have $xy1$ as the symbol of the unit of space filled with the matter of x, and also filled with the matter of y; that is to say, having the density xy, the sum of the densities of x and y. And of course we can, in this way, symbolize also the unit of space filled with the matters x and y in various proportions.

You will see that there is a real analogy between the symbols which I am here employing, and the symbols which I used just now in my illustration derived from double algebra; for just as the symbols of double algebra indicate to us not only the length of a line, but also its direction or position, so these chemical symbols indicate to us not only the weight, but also the kind of matter. You are not to confound them with the numbers which express the densities, or the letters by which we might express those numbers; but they are, I say, symbols which express to us, at one and the same time, the nature of the matter and the density of the matter, having a double signification of this kind.

Before we go further, let me say a word about the nature of this operation. I am here symbolizing the unit of matter by the symbols of the operation by which the unit of matter is made. But what is that operation?

Well, speaking generally, I may say, without entering into too nice logical distinctions, that it is an operation which every chemist knows better than any other physical operation. It is the operation of combination. That is what x is, and what y is. They are operations of combination. We are getting thus at a definition of our unit in terms perhaps more in accordance with our ordinary language. We will call the matter of x, A, and the matter of y, B; and the matter of unit of space, merely o. That is always definite. What, then, does x stand for, considered from the point of view of combination? It is the operation of combining the matter A with any substance which we please to write after the symbol of the letter. Similarly, y is the symbol of combining the matter B. Then we may call 1 the symbol of no matter; it is the symbol of the unit of space, which has no matter. $x1$ tells us we are to take A and combine it with the matter of unit of space. The result of that is to constitute the matter A. Having done that, I write y to it ($xy1$). That tells me to take the matter B and combine that also with the matter of the unit of space. If you do that, the result is the matter of A combined with the matter of B. These are the operations. Do not imagine there is anything mysterious about these terms. They are the operations about which you think every day of your life; and, I say, if you want to think philosophically about chemistry, you must embody in your symbol the very thing which you are thinking about, namely, the operation of combination itself.

I must not seek to explain to you now the process or method by which we arrive at the symbols of chemical substances, for to explain the process on the board, and to do it any justice, would occupy far more time than is at my disposal. You must allow me now simply to explain what we mean by the symbol of chemical substances (I mean in special cases), and then to consider the general results to which this mode of representation conducts us.

As to the mode of constructing these symbols, it is based in the most absolute way upon facts. We do not construct a symbol at all. We simply look for the symbol of matter and we find it. Where are we to look for the symbols of a chemical substance? Why, plainly to the symbols of matter in the gaseous condition: and where are we to look for the symbols of the operations by which units of matter are made? Why, plainly in the facts of combination. That is the source whence you are to deduce the symbol: it is the fact of combination itself. The facts in gaseous combinations are such as these:—2 volumes of hydrochloric acid consist of the same ponderable matter as 1 vol. of hydrogen and 1 vol. of chlorine. 2 vols. of gaseous water consist of the same ponderable matter as 2 vols. of hydrogen and 1 vol. of oxygen. Again (I will now put it in my way), 2 units of ammonia consist of the same ponderable matter as 3 units of hydrogen and 1 unit of nitrogen. These are the facts, and chemistry supplies us with a vast, but not an infinite number, of such facts. The method which I have ventured to give is simply a method of expressing the facts of the equation in the symbol of the substance. It is simply and purely a method of taking an equation and of embodying in the symbol the facts of the equation. Through the facts of the equation we construct the symbols of the units of ponderable matter. We then take the symbols out of the equations, and we thus separate and analyse the facts one from the other. It is simply an analysis of facts of a peculiar kind.

I have constructed some tables expressive of the general nature of the conclusions, at which we arrive through the aid of this method, as to the composition of these units of matter. I have had a good many of these symbols written out, for really it is easier for you, by looking at these tables, to see the general results which we arrive at by this method, than it would be for me to enter into a long explanation of the process. Here you see are the symbols of the chemical substances. We start with the symbol of the unit of space.

Symbols of the Units of Chemical Substances.

Unit of space	1
Hydrogen	α
Oxygen	ξ^2
Water	$\alpha\xi$
Peroxide of Hydrogen	$\alpha\xi^2$
Sulphur	θ^2
Protosulphide of Hydrogen	$\alpha\theta$
Bisulphide of Hydrogen	$\alpha\theta^2$
Sulphurous Anhydride	$\theta\xi^2$
Sulphuric Anhydride	$\theta\xi^3$
Sulphurous Acid	$\alpha\theta\xi^2$
Sulphuric Acid	$\alpha\theta\xi^4$
Chlorine	$\alpha\chi^2$
Hydrochloric Acid	$\alpha\chi$
Hypochlorous Acid	$\alpha\chi\xi$
Chlorous Acid	$\alpha\chi\xi^2$
Chlorosulphurous Acid	$\alpha\chi^2\theta\xi$
Hypochlorosulphurous Acid	$\alpha\chi\theta\xi^2$
Chlorosulphuric Acid	$\alpha\chi^2\theta\xi^2$
Iodine	$\alpha\omega^2$
Bromine	$\alpha\beta^2$

In the next table is another system of symbols, those of the combination of carbon, hydrogen, and two or three other elements.

Carbon	κ^2
Acetylene	$\alpha\kappa^2$
Marsh Gas	$\alpha^2\kappa$
Olefiant Gas	$\alpha^2\kappa^2$
Carbonic Oxide	$\kappa\xi$
Carbonic Acid	$\kappa\xi^2$
Alcohol	$\alpha^3\kappa^2\xi$
Ether	$\alpha^5\kappa^4\xi$
Glycol	$\alpha^3\kappa^2\xi^2$
Glycerine	$\alpha^4\kappa^3\xi^3$
Anhydrous Acetic Acid	$\alpha^3\kappa\xi^3$
Tetrachloride of Carbon	$\alpha^2\chi^4\kappa^2$
Chloroform	$\alpha^2\chi^3\kappa$
Chloracetic Acid	$\alpha^2\chi\kappa^2\xi^2$
Trichloracetic Acid	$\alpha^2\chi^3\kappa^2\xi^2$
Chloride of Benzoyl	$\alpha^3\chi\kappa^7\xi$
Cyanogen	$\alpha\nu^2\kappa^2$
Hydrocyanic Acid	$\alpha\nu\kappa$
Methylamine	$\alpha^3\nu\kappa$
Mercuric Ethide	$\alpha^5\kappa^4\delta$

You must regard these symbols as being chemical equations turned into another form, and divested of a certain amount of superfluous and useless matter, which we do not want now to consider or think about. Nature does not supply us with the key note to enable us to construct a definite system of chemical symbols. Nature does not tell us absolutely—though I think she does tell us probably—how we are to proceed to construct a system. In order to be able to construct a chemical system we must start with a hypothesis of some kind or other. As we go on constructing our symbols, of course our hypothesis, as we prove it, becomes a fact; but we must, at any rate, start with some hypothesis; that is to say, we must know one symbol. We may construct a complete chemical system from one symbol; and we may view all these symbols as taken from one hypothesis, combined with the facts given to us and supplied by the equation. Now, that hypothesis is this, that the symbol of the unit of hydrogen is expressed by one letter, α. That is my starting point; and I should say that the symbols which you see in the tables, as indicating chemical operations, are regarded as symbols of primary operations, that is to say, operations which you cannot resolve or decompose into any other symobls.

They are symbols of the primary operations; and when I say that the symbol of hydrogen can be expressed in chemical equations by one letter, I mean that in the changes and transformations of chemistry that unit of hydrogen is never broken up; that it moves as a whole from system to system, and that that unit of hydrogen is never decomposed or resolved into parts. The unit of hydrogen is constructed at once, by one operation. What I mean is this: imagine yourself witnessing the formation of hydrogen. To form some substances you want many operations; but to form hydrogen you want only one operation. That [striking a blow on the glass model of the unit of space] represents the formation of hydrogen,—one operation. It is one act. If we could witness chemical transformations, and nature would only become vocal tous, and indicate each combination as it occurred, by a musical note, that [again striking a blow] is what you would hear when hydrogen was formed. Now, as we go on we come to much more complex substances. Let us take oxygen. This is a substance very different indeed from hydrogen in its chemical properties; and as you can conceive of the unit of hydrogen being made at once by one operation, I say that it is impossible for you to conceive of the unit of oxygen being made by less than two operations. To return to our metaphor, when you take water and decompose it, and when you hear the oxygen go away, you ought to hear two notes, like this [striking two blows in close succession]. That is what I mean by saying that oxygen is made by two operations. Again, the unit of water is made by two operations like the unit of oxygen; but it differs from the unit of oxygen in this respect, that one of those operations is the same as that by which hydrogen is made, and the other is the same as that by which oxygen is made. That is to say, in the operation by which water was formed, you would hear two sounds, one different from the other, α, ξ.

The symbol of chlorine is $\alpha\chi^2$. Chlorine from this point of view, is to be conceived as made up of three operations. You are to hear χ, χ, and the α again. One of these operations is the same as that by which hydrogen is made, and the other is an operation peculiar to chlorine itself, namely, χ. Again: a unit of hydrochloric acid—a thousand cubic centimetres, in the condition of a perfect gas at a pressure of 760 millimetres—is to be conceived of as made by two operations, $\alpha\chi$.

To go one step further: let me refer you to this table:—

Nitrogen	$\alpha\nu^2$
Ammonia	$\alpha^3\nu$
Protoxide of Nitrogen	$\alpha\nu^2\xi$
Nitrous Acid	$\alpha\nu\xi^2$
Nitric Acid	$\alpha\nu\xi^3$
Phosphorus	$\alpha^3\phi^4$
Phosphide of Hydrogen	$\alpha^3\phi$
Hypophosphorus Acid	$\alpha^3\phi\xi^2$
Orthophosphoric Acid	$\alpha^3\phi\xi^4$
Terchloride of Phosphorus	$\alpha^3\phi\chi^3$
Pentachloride of Phosphorus	$\alpha^3\phi\chi^5$

Nitrogen is to be conceived of here as made of three operations, $\nu\nu$, and then α upon that. In the formation of the unit of ammonia three operations concur. One of them being one of the operation of nitrogen, ν, and the other two being the operation by which hydrogen is formed, α.

I must not enter into further details upon this subject, but I have little doubt that, with this explanation, you will readily appreciate the meaning of the symbols which are written up before you. You will see that, by following this process of taking the facts of the equations and turning them into the language of symbols, we arrive at a peculiar view as to the nature of matter, which view is embodied in those symbols.

Now, as to the nature of the view which is here indicated, for that, perhaps, will occur to most persons as the most important point to be considered. This view is the only result which I have placed before you in the first part of this method. It is the view as to the nature of matter itself. You will observe that, looking simply from the general point of view of the nature of matter, the point which it is most important for us to insist upon is the nature of the elemental bodies, because it is out of these elemental bodies that everything else is made, and into them all things are capable of being resolved. The view which we take of these bodies gives to us implicitly the view which we are to take of the composition of every other body whatever. To understand this it is only necessary to appreciate the view which is here given of the nature of the elements themselves, and everything else follows from that. We are led to the following singular results,—that, speaking generally, there are, perhaps, four—certainly at least three—fundamentally distinct classes of the elemental bodies.

First of all, there are elemental bodies, the units of which are made by one indivisible operation. These bodies are represented to us by mercury and hydrogen. To this class also probably belong such elements as zinc, cadmium, and tin; but we cannot speak with great confidence on that point.

Secondly, we have a class of double elements, formed by two similar operations; these are oxygen ξ^2, sulphur θ^2, selenium λ^2. Carbon we are not certain about; it belongs, in all probability, to the first or second class, we do not quite know which; but I have symbolized it as κ^2.

But we have another and a very large class—perhaps the largest of all the groups of the elements—and we may take the elements chlorine and nitrogen as representatives of it. Here is the symbol of the element chlorine, $\alpha\chi^2$; here is nitrogen, $\alpha\nu^2$; here is iodine, $\alpha\omega^2$; and so on. You will see that the symbols of these elements occupy a certain intermediate position between this group of elements, α, ι, ζ, etc., and that group of elements, ξ^2, θ^2, λ^2, etc. We have many compound substances which are in every way analogous to this group of elements—analogous as to their properties, analogous as to their symbols. Of this class we have a most interesting and striking example in the peroxide of hydrogen; it is symbolized here as $\alpha\xi^2$. You see the peroxide of hydrogen is really to be regarded as the combination of one unit of the element hydrogen with one unit of oxygen—which things really exist—just as the element chlorine may be regarded as a combination of the unit of hydrogen (α) with a substance which does not exist, and which I have symbolized as χ^2. The unit of nitrogen is to be regarded as similarly composed ($\alpha\nu^2$). We may regard it as α combined with the unknown element ν.

There is one question which must occur to every one, the explanation of which is of fundamental importance to the comprehension of this system. You may ask me, "What do you mean by these symbols—by calling chlorine $\alpha\chi^2$; nitrogen $\alpha\nu^2$; oxygen ξ^2? Do you mean that there are certain portions of matter, really existing, capable of being brought to the lecture-room—theoretically, at any rate—and shown upon the lecture-table: portions of matter which are represented by α and χ, α and ν, and so on? Do you mean this? or do you mean that these things are the creation of your imagination—that they are fictions—illusions? We like," perhaps you may say to me, "we like Dalton a great deal better than we do you; for Dalton, at any rate, dwelt with realities, or possible realities. He, at any rate, showed us the matter of which all substances are made. He brought the elemental bodies into the lecture-room in bottles, and he showed us there the elements out of which matter is made. Are you going to do that? Do you mean to show us α, ξ, χ, in the lecture-room? Again, Dalton dealt with realities through these atoms. Although, certainly, we have never seen them, yet, nevertheless, we most perfectly believe them to exist. There are such things as atoms, although we have never seen them. Dalton brought the elements to the lecture-table; and if he did not actually show us the atoms, you will find pictures of them at the end of his book; he made little bits of wood, which were excessively like atoms although they were wood. But you don't even do this for us." Well, this is rather a perplexing question; for if you ask me if these things really exist—whether they are things capable of being brought to the lecture-table and placed before us—in answer to such an inquiry I say, in the first place, that they do not necessarily exist. Then, again, you ask me this: "Do you say that they are imaginary things, that they are creations of your fancy? Because, if so, we don't trouble ourselves much about your fancies. They are not worth thinking about." I say, no, they are not fancies of mine; I never made them, I only found them. Then you answer: "All things are either imaginary or real; which are these?" Well, I reply, these things are *ideal* things. Well, then, my friend says I am getting beyond him when I say these are ideal, for he does not understand what ideal things are: all things are either imaginary or real. Yes; but I say there is a point which you have overlooked. Either all things exist according to the laws of nature which make it possible for them to exist, or there are insuperable barriers in the laws of nature to their existence. But though we may not know whether certain things exist or do not exist, yet we may reason about these things as if they were real things. A thing may not exist at all, but yet it may serve to us all the purposes of a real thing. That is what I mean by an ideal thing. It is a thing which may exist or may not exist, we do not know which, but which satisfies all the conditions of reality.

I shall venture, at the risk of delaying you a little longer, to give you an illustration on this point, which was suggested to me by some remarks and illustrations of Professor Stokes, with whom I have had the, to me, incomparable advantage of discussing and considering several of these difficult and abstruse questions. My illustration is simply his illustration a little modified for you. I am going to make a general assertion. I will draw a conic section—a curve on the board, and I am going to say that every straight line cuts every conic section in two points. That, you see, is somewhat analogous to my statement, that the unit of every chemical substance is composed of an integral number of prime factors, and is to be regarded as made up of an integral number of these bits of matter which I call symbol weights. There are the two corresponding assertions. But you say, "Do you mean that every straight line really cuts the conic section in two points?" I say, no, I never said that it *really* cut it at all. I said it *cut* it. Then you say, "Do you mean that it cuts in imagination?" I say, no, I do not say this. I do not know whether the line cuts it in reality or in the abstract. I do not know, speaking generally, whether the points at which a straight line cuts a conic section are real or imaginary. They are one or the other. You see that is a perfectly general mathematical truth and principle. Nothing is easier than to prove any proposition; and I may go on to say, that in the case of a straight line, of course, we can go farther: we can investigate the nature of these points; we can determine the nature of these points. But in chemistry we come to a bar. We cannot go on. We have not yet got the data to prove whether these prime factors, these units of weights, are real things or imaginary. They are undoubtedly one or the other, but we cannot tell which. There are facts here which satisfy all the analytical conditions of the facts, whether the matter is real or imaginary. There are symbols of facts, which satisfy the analytical conditions supplied to us by the equations of chemistry, and as such we are bound to accept them. We cannot do otherwise. It is impossible for us not to accept the conclusion.

We cannot, however, entirely dismiss from our consideration the alternative that these portions of matter— α, χ, ξ, ν, and ω, may be real. I *mean* a real thing when I say "a real thing." I mean something which may be brought to the lecture-table and put there, really or theoretically. We cannot close our eyes to the alternative; and there really are, though not at all derived from this method itself, but derived from other considerations, certain real reasons which would lead us to suspect that chemical substances are really composed of a primative system of elemental bodies, analogous in their general nature to our present elements, some of which we possess, but of which we possess only a few. I will take the case of the peroxide of hydrogen. I will throw overboard oxygen and a great class of certain oxygenated combinations, and I will suppose for the moment that I have these combinations—hydrogen, water, peroxide of hydrogen, and certain other substances which I could specify. If I were to apply my method to finding the symbol of peroxide of hydrogen, not regarding the oxygen at all, the symbol at which we arrive for peroxide of hydrogen is $\alpha\xi^2$. Thus the same question would arise in that case about peroxide of hydrogen as now arises about chlorine. In peroxide of hydrogen we really have succeeded in separating the elements which it contains, and this fact leads us to the suspicion that some of these bodies which we speak of as elements may in fact be compounds. In short we are led, through our method, to a certain physical hypothesis, of whatever value that hypothesis may be, as to the origin and causes of chemical phenomena.

Now, what I am going to suggest is of course put before you with reservation, but we may conceive that, either in remote time, or in remote space, there either did exist formerly, or there do exist now, certain simpler forms of matter than we find upon the surface of our globe—α, χ, ξ, ν, and so on—in short, these symbols of chemical operations. I say we may at least conceive of, or imagine the existence, either in time or in space, of these simpler forms of existence, of which we have some records remaining to us. Here they are—hydrogen and mercury—two things. We may consider that in remote ages the temperature of matter was much higher than it is now, and that these other things existed then in the state of perfect gases—separate existences—uncombined at any rate. This is the farthest barrier to which we can reach. There may be something farther, but if so, we can have no suspicion of it from the facts of the science. We may, then, conceive that the temperature begins to fall; these things begin to combine with one another. They enter into new forms of combination, appropriate to the circumstances in which they are placed. The result is the formation of new combinations. We may suppose that at this time water ($\alpha\xi$) hydrochloric acid ($\alpha\chi$), and many other bodies began to exist. Now, we may further consider that as the temperature went on falling, certain forms of matter became more permanent and more stable, to the exclusion of others. We have evidence on the surface of our globe itself, of the permanence of certain forms of matter to the exclusion of others. We may conceive of this process of the lowering of the temperature going on, so that these substances, $\alpha\chi^2$ and $\alpha\nu^2$, when they were once formed could never be decomposed—in fact that the resolution of these bodies into their component elements could never occur again. You then have something of our present system of things. You might yet imagine that it would be possible, on looking carefully at chemical equations, and minutely studying them, to recover from the equations the record of the truths which were buried and preserved in the equations; and some analyst might come and say, "These equations are *only* consistent with this hypothesis, that chlorine is composed of α and χ^2," or, at least, it might be said that the equations *are* consistent with that hypothesis, for I do not want to go farther than that. In short, we can conceive of such a state of things. Now, this is not really and purely an imagination, for when we look upon the surface of our globe, as I said before, we have evidence of similar changes in nature. We talk of the elemental bodies as though they were existing things; but where are they? We have oxygen, nitrogen, sulphur, certain metals, and certain bodies which we could specify, but what has become of the others? Where is hydrogen?. Where is chlorine? Above all, where is fluorine? Where are these things? Why, they are at any rate locked up in combination, in such a way that it is only within the last hundred years that the art of the chemist has revealed them to mankind. Now, if in our globe there had been more hydrogen—if there had been an excess of hydrogen present in the matter from which our globe was made—and if we suppose it to be true that the gases condense in the solid matter of our globe, we cannot doubt that the whole of the free oxygen would have been carried away from our planet, and that we should have had simply oxygen stored up in the form of water. We should have had water, but no oxygen at all; all the hydrogen would have combined with it and carried it all away.

When we look at some of the facts which have been revealed to us, by the extraordinary analyses which have been made of the matter of distant worlds and nebulæ, by means of the spectroscope, it does not seem quite incredible to me that there may even be evidence, some day, of the independent existence of such things as these, χ and ν. We know that Dr. Miller and Mr. Huggins saw a most wonderful hydrogen combustion—at least, what they imagined to be a hydrogen combustion—taking place in a variable star. Now, for aught we know to the contrary, this hydrogen combustion might be merely hydrogen combining with unknown elements, and carrying them all away in the form of chlorine, nitrogen, and the like. One of the nebulæ examined by Dr. Miller and Mr. Huggins afforded them the spectrum of an ignited gas, and in the spectrum of the nebula they saw one of the lines of nitrogen alone. Now, this suggests that this line might have been produced by one of the elements of nitrogen, before it had combined with another substance to form nitrogen. That might have been the element, ν. I am only suggesting that; but I say that if we follow up the subject we may have, one day revealed to us, independent evidence of the existence of these elements.

Let me, in conclusion, make one or two observations upon a point which, of course, must occur to every chemist who has studied this method. If we had taken, not α as the symbol of hydrogen, but had started with a different hypothesis, namely that the symbol of hydrogen was α^2, we should, of course, have arrived at a different symbolic system, that would have been analogous in its form to our present symbolic system—that is to say, you might have given to it an interpretation analogous to that system. We should have had hydrogen as α^2; water $\alpha^2\xi$, and so on. In fact, we should have been led to develop a system different from that which I have brought before you. You may with reason ask me, "Why do you prefer one of these systems to the other; or do you prefer it; or what view do you take of that question?" Let me say, in the first place, that my object has been, hitherto at least, not to give you a very definite answer to this question. For I have not yet placed before you and others, the ideas upon which a judgment can properly be formed on the question; but it is certainly true, in a certain sense, that there is more than one answer to the chemical problem, and that this system as thus developed, leads to another solution of the question. It gives you another answer to your inquiry. But further than this, there may be other answers still, although, perhaps, these are the only two answers necessary in considering the chemical problem; and this point which I wish to bring before you is of a far more subtile nature than it has been suspected to be. It is a method which you cannot attempt by the modes of atomic symbolism. It may be regarded as an equation of which there are not only one root, but several roots. Some of these roots may be thrown away, but some may lead us to a real solution. Now, I am not saying that one answer is the same in kind as the other answer, for I, with a natural preference, select the system α. I think there is something there, which is really of more importance and more necessary, in expressing the symbols than that which is given in the second system. I do not at all disregard that system. Indeed, I shall hereafter consider it, and endeavour to see, at any rate, what it means; but, I say, there is something in my system which is not in the second system—something in the system of α, which is not in the system of α^2. I cannot discuss this question with the hope of producing conviction in your minds, but I will just point out one fact. It is this—that you can pass from the system of α to the system of α^2 by a direct process of substitution. I mean, that if you say that here are two independent systems—the system of α, and the system of α^2—I say those systems are not entirely independent; for if you have constructed the system of α, you can make a substitution of α^2 for all. What that system would be, it is not necessary here to imagine. But having constructed this system of α^2, you cannot go back. It is not a logical consequence at all that, because you can take the square of the first system, therefore you can go back again. It will be absolutely impossible to pass at all from the latter system to the former. The one is derived primarily by substitution, and the other is not derived purely by substitution, but first by substitution, and then by reduction.

Discussion.

Dr. **Frankland**: I am sure, sir, that I only express the feelings of every one present when I say that I have listened to the lecture which Sir Benjamin Brodie has just given us with great interest and admiration. I cannot help thinking that the bringing forward of an entirely new method of viewing chemical phenomena such as has been brought before us to-night, must be fraught with great good to the science; but at the same time, I may be permitted, perhaps, having been alluded to in the earlier part of the lecture as a prominent advocate of what might be termed the opposite system of representing chemical facts, to protest at the outset, in the most emphatic manner, against the view which Sir Benjamin Brodie appears to have of such representations. I am not going to speak on behalf of other chemists who employ those more concrete modes of chemical representation. On my own behalf, however, in repudiation of the notion that I regard such representations as these graphic or glyptic formulæ, or even symbolic formulæ by letters only, in the sense of representations of the constitution of those portions of matter called atoms, or, as representations of the position of these atoms in the compound; perhaps I cannot do better than state, simply and at once, that I neither believe in atoms themselves, nor do I believe in the existence of centres of force, so that I do not think I can be fairly charged with this very crude notion which would otherwise attach to me with regard to the representation of such chemical compounds. Now, sir, many people, I believe, have been much dissatisfied of late with chemical formulæ in one respect, and I confess that I am one of the most dissatisfied. This note of dissatisfaction was, I believe, first expressed by Mr. Waterston. We do not express in our chemical formulæ, and in our chemical symbols, the idea of the *force* which has been involved in the operations expressed in those chemical compounds to which we apply the formulæ; and I think that one of the greatest advances which could possibly be made in the notation of chemical compounds, would be the introduction of this element. When, however, we leave statical formulæ—when we leave the mere representation of the atoms of compounds (if you will allow me figuratively, for a moment, to use the expression)—and when we go to the operations themselves by which those compounds are formed, I think we require then this expression of the forces involved, still more than we do in the statical formulæ that have hitherto been employed by chemists. Now, it appears to me, that we seek in vain for this element in the new development which Sir Benjamin Brodie has so eloquently placed before us this evening. Again, I think that every chemical formula is of use chiefly, if not only, as a means for future discovery in nature. So far as it serves that purpose it is of use; if it does not serve that purpose it is useless. The more, therefore, that a chemical formula expresses of our knowledge of the body for which we put it, the more valuable, I apprehend, that formula is. Now, if we take two well-known chemical compounds—namely, nitric acid on the one hand, and sulphuric acid on the other hand—I believe that if there is anything that we do know with certainty regarding these two acids, it is this: that in the case of nitric acid the hydrogen present in that compound can be taken out of it in one piece only, whilst in the sulphuric acid you can take out the hydrogen in two pieces. Now, when I look at the formula of nitric acid $\alpha\nu\xi^3$, and of sulphuric acid $\alpha\theta\xi^4$, I find in both these formulæ the same expression for the hydrogen; so that, I say, there is not contained in that formula the same amount of information, and of the most essential information, with regard to these two acids, that we possess in the present formula, differ as they may amongst different chemists, and lamentably they do so differ; still, by almost every chemist those two acids are expressed by formulæ representing this peculiarity of the hydrogen in those two compounds. These new formulæ, I say, do not express that idea, do not give us that information. I certainly do not imagine that any evil is likely to arise from such symbolical representations as have been hitherto used, even those of the very crudest kind which have been so strongly censured by Sir Benjamin Brodie; and, further, I do not think that science would ever suffer from the legitimate use of hypothesis. In fact, I cannot conceive of the future progress of science without such use of hypothesis: and I must say that it is to me a great recommendation for the new notation which has just been placed before us, that it involves a very fair amount of such hypothesis, which, I hope, will be capable of being used for the advance of the science, and for the benefit of its representations.

Professor **Clerk Maxwell** said he confessed that when he came into the room his feelings received a wholesome shock from two of the statements in the diagrams—first, that space was a chemical substance, and second, that hydrogen and mercury were operations. He now, however, understood what was meant. The present seemed to be an endeavour to cause the symbols of chemical substances to act in the formulæ according to their own laws. The formulæ at present used were made to express many valuable proper ties of chemical substances, just as a great many formulæ were employed to represent the syllogism in logic, which required a logical mind to form them, to understand them, and to reason upon them. The only successful attempt to introduce a new system in the logical representation was that of Mr. Boole, who accomplished it by the metaphysical and mathematical conception that x^2 was equal to x. In Sir Benjamin Brodie's system a did not mean exactly "hydrogen," but "make hydrogen;" that is, take the cubic centimetre of space, and put hydrogen into it of the proper pressure and temperature. But if they were to compress into that space another volume of hydrogen, that would not be a^2, because it would increase the pressure to double what it was before. If it were possible to get a^2, they would require to combine two volumes together by a process unknown to chemists, keeping the pressure and temperature as before. There was, in this respect, no doubt, an idea which differed from the mere collocation of symbols. The unit of ponderable matter described in the system was one which had been derived by chemists from chemical considerations alone. It had also been advocated by physicists from considerations derived from the theory of heat. In order to decide with certainty on the truth or falsehood of the atomic theory, it would be necessary to consider it from a dynamical point of view. He meant that kind of dynamics treated of in books on mechanics. It was worth while to direct the attention of chemists to the fact that a belief in atoms conducted necessarily to exactly the same definition as was given there—namely, that for every kind of substance the number of atoms, or molecules, in the gaseous state, occupying the space of a litre, at a temperature of 0 degrees, and of a pressure of 760 millimetres, must necessarily be the same. That was a consequence which could be deduced from purely dynamical considerations on the supposition advocated by Professor Clausius and others, that gases consist of molecules floating about in all directions, and producing pressure by their impact. That theory was now under probation among chemists, physicists, and others. The next step was one which might be far off—the finding of the number of these molecules. That number was a fixed one, and when it could be arrived at, we should have another unit of ponderable matter—that of a fixed molecule.

Professor Stokes (on being invited by the President to join in the discussion) said he had very little to add to what had been said; but referring to the way in which the symbols of some of the elements were arrived at, he might say they were based upon the known laws of combination by volume. The chief feature which struck him in the system of Sir Benjamin Brodie was, that it allowed them to express the composition of bodies by a method which took in all their existing knowledge, and did not assume anything besides. Of course the mode of expression was liable to change with an increase of knowledge; but taking their knowledge at a fixed point, such as it was at the present day, the method expressed everything that was known without superfluous hypotheses. The question of binoxide of nitrogen was a very important one; for if the known vapour density of that substance was to be admitted, it would seem to resign the question in favour of Laurent's system. On the other hand, if they agreed to pass over that anomaly for the present—and all chemists would admit that there was an apparent anomaly about it—then they were led to Sir Benjamin Brodie's system.

Mr. Wanklyn said the remarks of Professor Stokes had suggested what probably would occur to every one in the room—that the new method was a one-volume system. The well-known system of Gerhardt was a two-volume system.

Dr. Odling: The pleasure with which I have heard Sir Benjamin Brodie's lecture is, I am afraid, almost counterbalanced by the shock which my feelings received on hearing from Dr. Frankland that he questioned the positive existence of atoms. If Dr. Frankland's opinion on that subject was what he has stated, he has grossly deceived the chemical public. (Laughter.) The Chairman has said quietly aside to me that, after all, Dr. Frankland never really believed in atoms, or he would not have ventured to take such liberties with them. We have been led to believe that not only have we atoms, but that these atoms possess imaginary prongs, and that there is an imaginary clasping between them by means of these imaginary prongs, in a sort of hermaphroditism which it is scarcely possible to refer to. It behoves chemists to give their attention to this matter, as it affects their fundamental notions of chemical constitution and chemical change. After all, the system of symbols is a method of expression, and its value must depend upon its usefulness, upon the accuracy with which it expresses, and the facility with which it can be used. The accuracy with which Sir Benjamin's system can express, has been brought out in the part of his paper already published; but we wait for information on the facility with which the method can be used, and the advantages attending upon its use. It appears to me to possess one prominent merit, and that is expressed by the author in his paper—that the expressions are brought into immediate relations with the facts themselves. The present method of chemical expression, really, is not based directly upon the facts but upon the facts through the intervention of atomic theory. There is nothing of this kind in the method which the lecturer has introduced to the Society. No doubt the expression bears a relation directly to the fact, be the theory what it may. In the ordinary use of our present symbolic language, there are some chemists who, differing from Dr. Frankland, do believe in atoms, but who, not differing from Dr. Frankland, bring the idea of atoms prominently forward. On the other hand, there are some who, like myself, do not believe in atoms, and who keep the idea of atoms in the background as much as possible. But whether we do or do not believe in atoms, it still remains true that our notation is based upon the atomic theory, and without the atomic theory our language has no meaning whatever. One of the advantages of the new method is, that it is not inconsistent, by any means, with the atomic theory; but it is not based upon it. Another point of view from which the system commends itself even more strongly to our attention, is that which has been adverted to by Mr. Wanklyn, and which it does not require anybody to point out—namely, that it is a one-volume system, while the method which has been hitherto in use is a two-volume system. Laurent, however, has also introduced a one-volume system; but in that of the present author there is this additional feature: that it is the only mode introduced to chemists by which the volumes of substances can be represented by integral numbers without fractions. In Laurent's system there are fractions. It seems a thing altogether ridiculous that, under any system, we should adopt, as a unit of chemical substance, that which consists of two units, and which we are obliged to express in every way as consisting of two units. The new system is the first in which all prime factors can be written by an integral number. Another point of interest is the dissipation of that extraordinary law of even numbers which was brought into notice by Laurent and Gerhardt. Why that law should exist upon the atomic hypothesis, which is a dualistic hypothesis—the hypothesis which also exists in the expressions based on Laurent's one-volume system—is truly inconceivable; but in the mode of expression introduced by Sir Benjamin Brodie, this disappears altogether, and we get, at any rate, an entire series of compounds formed by the simple increment of atoms, one upon another, and without any of the objections which the ordinary formulæ present. Dr. Frankland has adduced, as a recommendation to this method, that it really introduces a great deal of hypothesis. There is no doubt that the whole system, as compared with the system of a^2, is based upon an hypothesis; but with the exception of that hypothesis, I do not think that there is anything hypothetical in the whole system. Admit, for one instant, the not improbable hypothesis that the unit of weight of hydrogen is an indivisible quantity, and all the remaining portion follows as a matter of course. There remains only the question whether the consequences, which the adoption of such an hypothesis leads to, are of such a character as to confirm the hypothesis, and, in fact, to demonstrate its truth. On that point I must say that I feel the difficulty which has been suggested by Dr. Frankland respecting the bodies which chemically behave in a different manner, and which are distributed in a different way, although, judging from their mere formulæ, it would be supposed that they should be distributed in an identical way. We have no means of separating, in our minds, that hydrogen which is distributable from that which is not. Altogether, I view the paper as of very great use to chemists, as calling upon them to consider the ground upon which their present opinions are based, and as shattering to the ground a great many of the superstructures which have been raised upon the old theory. The paper is of the highest importance and the greatest interest. I feel, as many must have felt upon former occasions, that a blow has been given to one's long-cherished convictions. But, at the same time, I am not at the present moment inclined to abandon them altogether. I look upon myself as vanquished for the moment, but I am not altogether disinclined to renew the combat, when further knowledge shall give me the means of doing so.

Mr. **Brayley** said that it appeared that one of the most striking and important elements of the subject which Sir Benjamin Brodie had placed before the Society, had not been adverted to by any of the gentlemen who had joined in the discussion. That was, the probability which it shadowed forth of discovering the compound nature of some of our present elements by a kind of chemical calculus. Professor Stokes had characterised the new method as embodying the representation of all we knew, but it appeared to add to this a hypothetical shadowing forth of what we might hope to know hereafter. It would be acceptable to chemists if the author would have the goodness to explain the manner in which he arrived, by his method, at the conclusion that whatever was represented by α (whether hydrogen or any other substance) was contained in all those haloid elements to which the sign of α was attached, such as chlorine, iodine, and bromine.

Mr. **Foster** said he thought the point which must strike chemists, at first sight at any rate, as the most interesting result of Sir Benjamin Brodie's method, was that it enabled them to arrive at the conclusion that some elements were, in point of fact, compounds. This, however, was by no means the first time it had occurred in the history of the science that chemists had arrived, by pure reasoning, at the conclusion that some substances, whose elements they did not know, were compounds. For instance, before it was known that the alkalies and the alkaline earths could be separated into oxygen and a metal, the majority of chemists were pretty well convinced that they were compound substances. And with regard to chlorine itself, that substance was, at its very discovery, viewed in a manner which was analogous to, if not, in point of fact, identical with, the view which was indicated by Sir Benjamin Brodie's notation. Hydrogen, hydrochloric acid, and chlorine were regarded in early times as bodies which formed a series with each other, hydrochloric acid being exactly half-way between hydrogen and chlorine. One point which had been alluded to, as well by Dr. Frankland as by Dr. Odling, was that the symbols exhibited in the diagram did not, in all cases, express quite as much, from some points of view, as the ordinary chemical formulæ. For instance, the formulæ of sulphuric acid and of nitric acid did not show that the sulphuric acid was what was commonly called bibasic, and nitric acid monobasic. That, he thought, was hardly a fair criticism upon the formulæ. For sulphuric acid the symbol was $\alpha\theta\xi^4$; for nitric acid the symbol was $\alpha\nu\xi^3$. But although the symbol representing hydrogen occurred merely once in each formula, still, having regard to the other letters, these tell us that the hydrogen is distributable in one case and not in the other. Take again the similar cases of water and hydrochloric acid. The first was represented by $\alpha\xi$, and the other by $\alpha\chi$. One of the properties of the symbol χ was to render the α with which it was combined undistributable, whereas the ξ combined with the hydrogen rendered the hydrogen distributable; so that although α occurred once only in the formulæ of water and hydrochloric acid, the other letters showed that its properties varied. They should, therefore, judge of the formula, not by one of its signs alone, but by the varied meaning due to the collocation of symbols.

Dr. **Williamson** (Chairman): I cannot refrain from expressing on my own behalf, what I am sure must have been felt by all present—the strong sense of the obligation under which we are placed to our eminent colleague, for the most laborious and most important work which he has been carrying on for many years, though it is only now becoming known to chemists. It is a peculiarly difficult thing to dissociate oneself from the prevailing ideas on such a subject as has been put before the Society; and yet such dissociation is essential to the working out of any truly new conception, and especially any new mode of expression. The novelties of expression which Professor Maxwell spoke of as having been a shock to him, are in themselves the essential condition of the working out of the new system of notation. One is, perhaps, pained on first hearing that 1 is equal to zero. Nor are we accustomed to understand an operation performed merely upon space. I must confess that although I have given considerable time and attention to the paper, I do not feel that I am in a condition to fully appreciate all the expressions used in that part of the system already published. There are some things which I feel hardly able to accept on the present basis; but, at the same time, I feel that until one can understand in plain English and in ordinary words the meaning of those operations and things denoted, it will be exceedingly presumptuous to doubt the correctness of results which have *primâ facie* evidence of great consistency. Every chemist must be struck with the unity and consistency of the new method; but those qualities cannot be attributed to the present system of notation. I am quite convinced that whatever modifications Sir Benjamin Brodie's system may undergo at the hands of its author, its introduction will inaugurate an exceedingly important era in chemical language and notation. I am sure the Society will join with me in thanking the author most cordially for the great intellectual feat which he has performed in working out this subject, and in hoping that before long he may accomplish some further steps in his great work.

The Lecturer's Reply.

Sir **Benjamin Brodie**: I do not know that there really remains very much for me to say upon this matter. With many of the remarks that have been made, both by Professor Frankland and Dr. Odling, I cannot but agree. I think, myself, that the object of a method is not simply to give us statical formulæ, but that we must also consider the dynamics of the science. I mean by "statical formulæ," that we are not merely to consider what matter is, but that we are to consider the laws also, by which matter changes; and that is a point upon which I hope to throw some light at a future time, in the second part of the paper. It is then that the question will naturally arise as to the way in which the kinds of facts, which it has been attempted to express by the theories of atomicity, will appear in this method. You must not suppose, because I have not entered upon these subjects, that I have ignored them; I only postpone the consideration of them. With regard to the other point, about the relative merits of this mode of statement and the one ordinarily in use, I think that some of the remarks of Dr. Frankland, and my friend Dr. Odling too, are based simply upon a misconception. Dr. Frankland seems to imagine, in the case of such symbols as those of nitric acid $\alpha\nu\xi^3$, and of sulphuric acid $\alpha\theta\xi^4$, that the important and fundamental distinction which is assumed in our present system to consist in the different number of atoms of replaceable hydrogen which these substances respectively contain, is altogether obliterated and lost sight of. But this is not so. By simple inspection of the symbols, you can ascertain precisely, as in our present notation, the changes of this order of which chemical substances are susceptible. But, as Professor Foster justly observes, you must take into account the whole symbol, and not a bit of it only. If you take into account the fact that the matter of all known chemical substances is identical with the matter of the elementary bodies, then there is nothing to be added. To one who is familiarised with the use of these expressions (when this point is properly regarded), it is at once apparent, that, whereas we can perform two of the operations, for example, symbolised in χ, and two only of those operations, upon the symbol $\alpha\theta\xi^4$, we can perform one, and one only, of those operations upon the symbol $\alpha\nu\xi^3$. These important questions, however, cannot be thus briefly discussed, and I must defer their consideration to other opportunities.

ROYAL INSTITUTION.

Friday, May 31, 1867.

*On the Chemistry of the Primeval Earth**, *by* T. Sterry Hunt, M.A., F.R.S

The subject of my lecture this evening, as has been announced, is the Chemistry of the Primeval Earth. The natural history of our planet, to which we give the name of geology, is, necessarily, a very complex science, including, as it does, the concrete sciences of mineralogy, of botany, and zoology, and the abstract sciences of chemistry and physics. These latter sustain a necessary and very important relation to the whole process of development of our earth from its earliest ages, and we find that the same chemical laws which have presided over its changes apply also to those of extra-terrestrial matter. Recent investigations show the presence in the sun, and even in the fixed stars—suns of other systems—the same chemical elements as in our own planet. The spectroscope, that marvellous instrument, has, in the hands of modern investigators, thrown new light upon the composition of the farthest bodies of the universe, and has made clear many points which the telescope was impotent to resolve. The results of extra-terrestrial spectroscopic research have lately been set forth in an admirable manner by one of its most successful students, Mr. Huggins. We see by its aid matter in all its stages, and trace the process of condensation and the formation of worlds. It is long since Herschel, the first of his illustrious name, conceived the nebulæ, which his telescope could not resolve, to be the uncondensed matter from which worlds are made. Subsequent astronomers, with more powerful glasses, have been able to show that many of these nebulæ are really groups of stars, and thus a doubt was thrown over the existence in space of nebulous luminous matter: but the spectroscope has now placed the matter beyond doubt. We thus find in the heavens planets, bodies like our earth, shining only by reflected light; suns, self-luminous, radiating light from solid matter; and, moreover, true nebulæ, or masses of luminous gaseous matter. These three forms represent three distinct phases in the condensation of the primeval matter, from which our own and other planetary systems have been formed.

This nebulous matter is conceived to be so intensely heated as to be in the state of true gas or vapour, and, for this reason, feebly luminous when compared with the sun. It would be out of place, on the present occasion, to discuss the detailed results of spectroscopic investigation, or the beautiful and ingenious methods by which modern science has shown the existence in the sun, and in many other luminous bodies in space, of the same chemical elements that are met with in our earth, and even in our own bodies; realising, in a most literal manner, the genial intuition of the poet, who

> "Sees alike in stars and flowers a part
> Of the self-same universal being
> That is throbbing in his mind and heart."

Calculations based on the amount of light and heat radiated from the sun show that the temperature which reigns at its surface is so great that we can hardly form an adequate idea of it. Of the chemical relations of such intensely heated matter modern chemistry has made known to us some curious facts, which help to throw light on the constitution and luminosity of the sun. Heat, under ordinary conditions, is favourable to chemical combination, but a higher temperature reverses all affinities. Thus, the so-called noble metals, gold, silver, mercury, &c., unite with oxygen and other elements; but these compounds are decomposed by heat, and the pure metals are regenerated. A similar reaction was many years since shown by Mr. Grove with regard to water, whose elements—oxygen and hydrogen—when mingled and kindled by flame, or by the electric spark, unite to form water, which, however, at a much higher temperature, is again resolved into its component gases. Hence, if we had these two gases existing in admixture at a very high temperature, cold would actually effect their combination precisely as heat would do if the mixed gases were at the ordinary temperature, and literally it would be found that "frost performs the effect of fire." The recent researches of Henry Ste.-Claire Deville and others go far to show that this breaking up of compounds, or dissociation of elements by intense heat, is a principle of universal application; so that we may suppose that all the elements, which make up the sun or our planet, would, when so intensely heated as to be in that gaseous condition which all matter is capable of assuming, be uncombined—that is to say, would exist together in the condition of what we call chemical elements, whose further dissociation in stellar or nebulous masses may even give us evidence of matter still more elemental than that revealed by the experiments of the laboratory, where we can only conjecture the compound nature of many of the so-called elementary substances.

The sun, then, is to be conceived as an immense mass of intensely heated gaseous and dissociated matter, so condensed, however, that, notwithstanding its excessive temperature, it has a specific gravity not much below that of water, probably offering a condition analogous to that which Cagniard de la Tour observed for volatile bodies when submitted to great pressure at temperatures much above their boiling point. The radiation of heat going on from the surface of such an intensely heated mass of uncombined gases, will produce a superficial cooling, which will permit the combination of certain elements and the production of solid or liquid particles, which, suspended in the still dissociated vapours, become intensely luminous and form the solar photosphere. The condensed particles, carried down into the intensely heated mass, again meet with a heat of dissociation, so that the process of combination at the surface is incessantly renewed, while the heat of the sun may be supposed to be maintained by the slow condensation of its mass; a diminution by $\frac{1}{1000}$th of its present diameter being sufficient, according to Helmholtz, to maintain the present supply of heat for 21,000 years.

This hypothesis of the nature of the sun and of the luminous process going on at its surface, is the one lately put forward by Faye, and, although it has met with opposition, appears to be the one which accords best with our present knowledge of the chemical and physical conditions of matter, such as we must suppose it to exist in the condensing gaseous mass, which, according to the nebular hypothesis, should form the centre of our solar system. Taking this, as we have already done, for granted, it matters little whether we imagine the different planets to have been successively detached as rings during the rotation of the primal mass, as is generally conceived, or whether we admit with Chacornac a process of aggregation, or concretion, operating within the primal nebular mass, resulting in the production of sun and planets. In either case we come to the conclusion that our

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* Reported specially for this paper, and revised by the author.

earth must at one time have been in an intensely heated gaseous condition, such as the sun now presents, self-luminous, and with a process of condensation going on at first at the surface only, until by cooling it must have reached the point where the gaseous centre was exchanged for one of combined and liquefied matter.

Here commences the chemistry of the earth, to the discussion of which the foregoing considerations have been only preliminary. So long as the gaseous condition of the earth lasted, we may suppose the whole mass to have been homogeneous; but when the temperature became so reduced that the existence of chemical compounds at the centre became possible, those which were most stable at the elevated temperature then prevailing, would be first formed. Thus, for example, while compounds of oxygen with mercury, or even with hydrogen, could not exist, oxides of silicon, aluminum, calcium, magnesium, and iron might be formed and condense in a liquid form at the centre of the globe. By progressive cooling, still other elements would be removed from the gaseous mass, which would now become the atmosphere of the non-gaseous nucleus. We may suppose an arrangement of the condensed matters at the centre according to their respective specific gravities, and thus the fact that the density of the earth as a whole is about twice the mean density of the matters which form its solid surface. Metallic or metalloidal compounds of elements grouped differently from any compounds known to us, and far more dense, may exist in the centre of the earth.

The process of combination and cooling having gone on, until those elements which are not volatile in the heat of our ordinary furnaces, were condensed into a liquid form, we may here inquire what would be the result, upon the mass, of a further reduction of temperature. It is generally assumed that in the cooling of a liquid globe of mineral matter, congelation would commence at the surface, as in the case of water; but water offers an exception to most other liquids, inasmuch as it is denser in the liquid than in the solid form. Hence ice floats on water, and freezing water becomes covered with a layer of ice, which protects the liquid below. With most other matters, however, and notably with the various mineral and earthy compounds analogous to those which may be supposed to have formed the fiery-fluid earth, numerous and careful experiments show that the products of solidification are much denser than the liquid mass; so that solidification would have commenced at the centre, whose temperature would thus be the congealing point of these liquid compounds. The important researches of Hopkins and Fairbairn on the influence of pressure in augmenting the melting-point of such compounds as contract in solidifying, are to be considered in this connexion.

It is with the superficial portions of the fused mineral mass of the globe that we have now to do, since there is no good reason for supposing that the deeply-seated portions have intervened in any direct manner in the production of the rocks which form the superficial crust. This, at the time of its first solidification, presented probably an irregular, diversified surface, from the result of contraction of the congealing mass, which at last formed a liquid bath of no great depth, surrounding the solid nucleus. It is to the composition of this crust that we must now direct our attention, since therein would be found all the elements (with the exception of such as were still in the gaseous form) now met with in the known rocks of the earth. This crust is now everywhere buried beneath its own ruins, and we can only, from chemical considerations, attempt to reconstruct it. If we consider the conditions through which it has passed, and the chemical affinities which must have come into play, we shall see that they are just what would now result if the solid land, sea, and air were made to react upon each other under the influence of intense heat. To the chemist it is at once evident that from this would result the conversion of all carbonates, chlorides, and sulphates into silicates, and the separation of the carbon, chlorine, and sulphur in the form of acid gases, which, with nitrogen, watery vapour, and a probable excess of oxygen, would form the dense primeval atmosphere. The resulting fused mass would contain all the bases as silicates, and must have much resembled in composition certain furnace slags, or volcanic glasses. The atmosphere, charged with acid gases which surrounded this primitive rock, must have been of immense density. Under the pressure of such a high barometric column, condensation would take place at a temperature much above the present boiling-point of water, and the depressed portions of the half-cooled crust would be flooded with a highly heated solution of hydrochloric acid, whose action in decomposing the silicates is easily intelligible to the chemist. The formation of chlorides of the various bases, and the separation of silica, would go on until the affinities of the acid were satisfied, and there would be a separation of silica taking the form of quartz, and the production of a sea-water holding in solution, besides the chlorides of sodium, calcium, and magnesium, salts of aluminium and other metallic bases. The atmosphere being thus deprived of its volatile chlorine and sulphur compounds, would approximate to that of our own time, but differ in its greater amount of carbonic acid.

We next enter into the second phase in the action of the atmosphere upon the earth's crust. This, unlike the first, which was subaqueous, or operative only on the portion covered with the precipitated water, is sub-aerial, and consists in the decomposition of the exposed parts of the primitive crust under the influence of the carbonic acid and moisture of the air, which would convert the complex silicate of the crust into a silicate of alumina, or clay, while the separated lime, magnesia, and alkalies, being converted into carbonates, would be carried down into the sea in a state of solution. The first effect of these dissolved carbonates would be to precipitate the dissolved alumina and the heavy metals, after which would result a decomposition of the chloride of calcium of the sea-water, resulting in the production of carbonate of lime or limestone, and chloride of sodium or common salt. This process is one still going on at the earth's surface, slowly breaking down and destroying the hardest rocks, and, aided by mechanical processes, transforming them into clays; although the action, from the comparative rarity of carbonic acid in the atmosphere, is less energetic than in earlier times, when the abundance of this gas and a higher temperature, favoured the chemical decomposition of the rocks. But now, as then, every clod of clay formed from the decay of a crystalline rock corresponded to an equivalent of carbonic acid abstracted from the atmosphere, and equivalents of carbonate of lime and common salt formed from the chloride of calcium of the sea-water.

It is very instructive, in this connexion, to compare the composition of the waters of the modern ocean with that of the sea in ancient times, whose composition we learn from the fossil sea-waters which are still to be found in certain regions, imprisoned in the pores of the older stratified rocks. These are vastly richer in salts of lime and magnesia than those of the present sea, from which have been separated, by chemical processes, all the carbonate of lime of our limestones with the exception of that derived from the sub-aerial decay of calcareous silicates belonging to the primitive crust.

The gradual removal, in the form of carbonate of lime, of the carbonic acid from the primeval atmosphere, has been connected with great changes in the organic life of the globe. The air was doubtless at first unfit for the respiration of warm-blooded animals, and we find the higher forms of life coming gradually into existence as we approach the present period of a purer air. Calculations lead us to conclude that the amount of carbon thus removed in the form of carbonic acid has been so enormous, that we must suppose the earlier forms of air-breathing animals to have been peculiarly adapted to live in an atmosphere which would probably be too impure to support modern reptilian life. The agency of plants in purifying the primitive atmosphere was long since pointed out by Brongniart, and our great stores of fossil fuel have been derived from the decomposition, by the ancient vegetation, of the excess of carbonic acid of the early atmosphere, which through this agency was exchanged for oxygen gas. In this connexion the vegetation of former periods presents the curious phenomenon of plants, allied to those now growing beneath the tropics, formerly flourishing within the polar circles. Many ingenious hypotheses have been proposed to account for the warmer climate of earlier times, but are at best unsatisfactory, and it appears to me that the true solution of the problem may be found in the constitution of the early atmosphere, when considered in the light of Dr. Tyndall's beautiful researches on radiant heat. He has found that the presence of a few hundredths of carbonic acid gas in the atmosphere, while offering almost no obstacle to the passage of the solar rays, would suffice to prevent almost entirely the loss by radiation of obscure heat, so that the surface of the land beneath such an atmosphere would become like a vast orchard-house, in which the conditions of climate, necessary to a luxuriant vegetation, would be extended even to the polar regions. This peculiar condition of the early atmosphere cannot fail to have influenced in many other ways the processes going on at the earth's surface. To take a single example: one of the processes by which gypsum may be produced at the earth's surface involves the simultaneous production of carbonate of magnesia. This, being more soluble than the gypsum, is not always now found associated with it, but we have indirect evidence that it was formed, and subsequently carried away, in the case of many gypsum deposits whose thickness indicates a long continuance of the process, under conditions much more perfect and complete than we can attain under our present atmosphere. While studying this reaction I was led to inquire whether the carbonic acid of the earlier periods might not have favoured the formation of gypsum, and I found, by repeating the experiments in an artificial atmosphere impregnated with carbonic acid, that such was really the case. We may thence conclude that the peculiar composition of the primeval atmosphere, was the essential condition under which the great deposits of gypsum, generally associated with magnesian limestones, were formed.

The reactions of the atmosphere which we have considered, would have the effect of breaking down and disintegrating the surface of the primeval globe, covering it everywhere with beds of stratified rock of mechanical or of chemical origin. These would now so deeply cover the partially cooled surface that the amount of heat escaping from below is inconsiderable, although in earlier times it was very much greater, and the increase of temperature met with in descending into the earth must have been many times more rapid than now. The effect of this heat upon the buried sediments would be to soften them, producing new chemical reactions between their elements, and converting them into what are known as crystalline or metamorphic rocks, such as gneiss, greenstone, granite, &c. We are often told that granite is the primitive rock or substratum of the earth, but this is not only unproved, but extremely improbable. As I endeavoured to show in the early part of this lecture, the composition of this primitive rock, now everywhere hidden, must have been very much like that of a slag or lava, and there are excellent chemical reasons for maintaining that granite is in every case a rock of sedimentary origin—that is to say, it is made up of materials which are deposited from water like beds of modern sand and gravel, and includes in its composition quartz, which so far as we know, can only be generated by aqueous agencies, and at comparatively low temperatures.

The action of heat upon many buried sedimentary rocks, however, not only softens or melts them, but gives rise to a great disengagement of gases, such as carbonic and hydrochloric acids, and sulphur compounds, all results of the reaction of the elements of sedimentary rocks, heated in presence of the water which everywhere filled their pores. In the products thus generated we have a rational explanation of the chemical phenomena of volcanos, which are vents through which these fused rock and confined gases find their way to the surface of the earth. In some cases, as where there is no disengagement of gases, the fused or half-fused rocks solidify *in situ*, or in rents or fissures in the overlying strata, and constitute eruptive or plutonic rocks like granite and basalt.

This theory of volcanic phenomena was put forward in germ by Sir John F. W. Herschel thirty years since, and, as I have during the past few years endeavoured to show, it is the one most in accordance with what we know both of the chemistry and the physics of the earth. That all volcanic and plutonic phenomena have their seat in the deeply buried and softened zone of sedimentary deposits of the earth, and not in its primitive nucleus, accords with the conclusions already arrived at relative to the solidity of that nucleus; and also with the remarkable mathematical and astronomical deductions of the late Mr. Hopkins, of Cambridge, based upon the phenomena of precession and nutation; those of Archdeacon Pratt; and those of Professor Thompson on the theory of the tides; all of which lead to the same conclusion—namely, that the earth, if not solid to the centre, must have a crust several hundred miles in thickness, which would practically exclude it from any participation in the plutonic phenomena of the earth's surface, except such as would result from its high temperature communicated by conduction to the sedimentary strata reposing upon it.

The old question between the plutonists and the neptunists, which divided the scientific world in the last generation, was, in brief, this—whether fire or water had been the great agent in giving origin and form to the rocks of the earth's crust. While some maintained the direct igneous origin of such rocks as gneiss, mica-schist, and serpentine, and ascribed to fire the filling of metallic veins, others—the neptunian school—were disposed to shut their eyes to the evidences of igneous action on the earth, and even sought to derive all rocks from a primal aqueous magma. In the light of the exposition which I have laid before you this evening, we can, I think, render justice to both of these opposing schools. We have seen how actions dependent on water and acid solutions have operated on the primitive plutonic mass, and how the resulting aqueous sediments, when deeply buried, come again within the domain of fire, to be transformed into crystalline and so-called plutonic or volcanic rocks.

The scheme which I have endeavoured to put before you in the short time allotted, is, as I have endeavoured to show, in strict conformity with known chemical laws and the facts of physical and geological science. Did time permit, I would gladly have attempted to demonstrate at greater length its adaptation to the explanation of the origin of the various classes of rocks, of metallic veins and deposits, of mineral springs, and of gaseous exhalation. I shall not, however, have failed in my object, if, in the hour which we have spent together, I shall have succeeded in showing that chemistry is able to throw a great light upon the history of the formation of our globe, and to explain in a satisfactory manner some of the most difficult problems of geology; and I feel that there is a peculiar fitness in bringing such an exposition before the members of this Royal Institution, which has been for so many years devoted to the study of pure science, and whose glory it is, through the illustrious men who have filled, and those who now fill, its professorial chairs, to have contributed more than any other school in the world to the progress of modern chemistry and physics.

ACADEMY OF SCIENCES.

June 3, 1867.

(FROM OUR SPECIAL CORRESPONDENT.)

M. DEBRAY, aided by M. Wisnegg, described and set to work two apparatus for producing very elevated temperatures by means of common gas mixed with air. The first was that of M. Schlösing, modified by M. Wisnegg, the second that of M. Perrot.

If a certain number of Bunsen burners be united together so as to form one single jet of flame, without, however, complete incorporation, the heating power is most remarkable, provided a sufficiently energetic and swift draught is given to it. The form of the furnace must also be varied, and the draught regulated, according to circumstances.

With an apparatus burning 70 cubic feet per hour, under a pressure of two or three inches of water, and without any draught but that obtained by a sheet-iron pipe 6¼ feet high, M. Debray was able in fifteen minutes to melt 670 grammes (1·48 lb.) of silver. It only takes half an hour at most, when the operation is at full work, to melt and cast a kilogramme of copper into a bar. Lastly, M. Debray melted several specimens of grey and white iron; a pound of a sort of cast-iron which passes for being very difficult to melt, was run in thirty minutes; another piece weighing 1¼ lb. was melted in an hour or so. During the operation the crucible can be examined in the interior by the aid of a mirror, or a bucket of water, which can receive the metal in case of accident.

M. Chevreul announced the melancholy news of the death of our esteemed friend, M. Pelouze, in a touching letter from his son. He was equally excellent as a father, husband, and practical chemist.

M. Tresca read the third part of his memoir "*On the Flowing of Solid Bodies, especially Metals, through Apertures.*" Pressure exerted on any point of a substance is transmitted throughout the interior of the mass.

M. Hulot, director of the workshops of the manufactory of postage-stamps at the Paris Imperial Mint, made two communications. "*On Aluminium Bronze and Aluminium Soldering.*" The paper, and especially that gummed and dried, as used for postage-stamps, rapidly deteriorates tools even of the best tempered steel. The 300 perforators for piercing the postage-stamps are used up after a day's work, in a few hours their ends become blunted, and instead of piercing only crush the paper, the last holes made being considerably enlarged. M. Hulot replaces the steel by aluminium bronze at 10 per cent., and the new tool, striking 126,000 blows per day, or 180,000,000 holes, has worked for several months without need of repairs. Aluminium bronze does not unite freely with solder by the old process; but if we take equal quantities of zinc amalgam and common solder, aluminium bronze can be admirably soldered together by it. This solder becomes better, again, if it is alloyed with once or twice its weight of tin. Thus we have three excellent solders—1st, solder with half its weight of amalgam; 2nd, with a fourth; 3rd, with an eighth. This is an excellent discovery, as it places aluminium on a new footing as regards mechanical appliances, especially for bushes or bearings for machinery, as the metal is almost indestructible by friction.

The Academy then proceeded to the election of a member in the section of medicine and surgery. M. Nélaton was elected, the majority consisting of thirty-two votes, against twenty-six given to M. Laugier.

June 10.

M. Schönbein placed a sealed letter in the hands of the Academy, containing a description of an important discovery by M. Liebig.

M. Boussingault communicated a new mode of finding the quantities of lime in analyses. The process consists in precipitating the lime in the state of sulphate, which is decomposed either by a Bunsen gas blowpipe or by one of Schlösing's furnaces, the sulphuric acid being vaporised, and the lime remaining pure. In several experiments on the decomposition of earthy or metallic sulphates, M. Boussingault remarked frequent anomalies, the quantity of the base remaining often being less than it ought to be. This fact is not easy to account for.

M. Payen reproduced in part his communication made to the Society of Encouragement, on the fabrication of paper pulp from wood fibre, and on the transformation of ligneous matters into cellulose and glucose.

MM. de Luca and Ubaldine communicated a "*Note on the Reaction between Sulphurous Acid and Sulphuretted Hydrogen.*" The sulphur which is deposited by the reciprocal action of sulphurous and hydrosulphuric acid consists of two varieties of sulphur, one soluble and the other insoluble in bisulphide of carbon. The relative proportion of these two sulphurs depends upon the conditions under which the operation is made; the quantity of insoluble sulphur is increased when an excess of sulphurous acid is present. The reaction expressed by the formula

$$2\,HS + SO_2 = 2\,HO + 3\,S$$

has its analogy in the reaction between sulphuretted hydrogen and selenious acid

$$2\,HS + SeO_2 = 2\,HO + SeS_2,$$

with this difference—that the bisulphide of sulphur $SS_2 = 3S$, etc., is replaced by the bisulphide of selenium SeS_2.

M. de la Rive read the principal conclusions of a memoir "*On the Sources and Phenomena of Atmospheric Electricity,*" but there was nothing new in it.

June 17.

M. Becquerel placed before the eyes of the Academy several specimens of metals reduced and precipitated by capillary action. In order to answer the objection that these phenomena of reduction or precipitation might be attributed to the action of the alkalies of the split glass tube, he employed polished plates of rock crystal applied one against the other so as to leave only a very small interval, and produce coloured rings; he has thus obtained perfect reduction of several metal. The interval between the plates must be varied according to the different metals, and he has obtained by this means the reduction of such metals as cobalt, nickel, copper, gold, etc. For the reduction of gold, for example, the space between the plates must be less than that for copper. When the solution is dilute, there is no further reduction of metal, but a precipitation of oxide.

M. Leverrier communicated the observations made by M. Wolf on the crater of Linnæus in the moon, from May 10 to June 10. His observations confirm those made by M. Rospighi at Rome. and lead to the same conclusion—viz., that it is not proved that there has been any change in this crater for a century.

M. Dumas read, in the name of M. de la Rive, of Geneva, a note upon an apparatus for measuring the different degrees of transparency of the air. According to M. de la Rive, the great transparency of the air before rain is due to the presence, in the air, of a quantity of invisible vapour which renders transparent the numerous germs floating in the air, to whose presence light mists are attributed.

CHEMICAL SOCIETY.

Thursday, June 20, 1867.

Dr. Warren De la Rue, *F.R.S., President, in the Chair.*

On the minutes being read, Dr. Thudichum protested against the interruption of Mr. R. Davey's speech by the Chairman on the last occasion; upon which Dr. Williamson answered by saying that he merely called to order a gentleman who, instead of speaking on the subject of Sir Benjamin Brodie's paper, was addressing inquiries to Mr. Davey. After this little *contretemps*, and a few words by way of further explanation from Mr. J. Newlands, the minutes were duly confirmed. Mr. J. C. Brough was formally admitted a Fellow of the Society, and the name of Mr. Charles M. Tidy, F.R.C.S., was read for the first time.

Mr. W. H. Perkin read a paper "*On Some New Derivatives of the Hydride of Salicyl.*" The author referred to a previous communication in which he had stated that the hydride of salicyl partook of the properties both of an alcohol and an aldehyde, and, upon extending these experiments, he has succeeded in producing several new derivatives containing the alcohol radicals.

Hydride of Methyl-Salicyl was formed by the action of iodide of methyl upon salicylate of sodium, in sealed tubes heated to 135-140° C. It separates as a light yellow or nearly colourless oil on adding water to the contents of the tubes. Washed with dilute solution of hydrate of potassium and re-distilled, the product came over at 238°. Its formula was established by two combustions, which gave—

$$C_8H_8O_2=HC_7H_4(CH_3)O_2.$$

The hydride of methyl-salicyl is isomeric with the hydride of anisyl, but differs from it both in boiling point and odour. It is a true aldehyde, and combines with the bisulphites, the potassium, sodium, and ammonium compounds of which are described.

Hydride of methyl bromosalical was formed as a substitution product by the action of bromine upon the hydride of methyl-salicyl. It separates from a hot alcoholic solution in the form of flat prismatic crystals, which fuse at 113-114·5° C., and have the following composition—

$$C_8H_7BrO_2=HC_7H_3Br(CH_3)O_2.$$

The actions of ammonia and of nitric acid have been studied; the latter gave rise to the production of two bodies —viz., a dark-coloured oil, which is probably the *hydride of mehtyl-nitrosalicyl* and a pale yellow crystalline body, assumed to be *methyl-nitrosalicylic acid.*

Hydride of ethyl-salicyl was formed in a manner similar to the first of this series. It is a nearly colourless, highly refractive oil, boiling at 247-249° C. Its composition is—

$$C_9H_{10}O_2=HC_7H_4(C_2H_5)O_2.$$

It possesses the properties of an aldehyde, and combines with the alkaline bisulphites.

The hydride of ethyl-bromosalicyl was produced by the action of bromine upon the preceding compound. The crystals fuse at 67-68° C., and contain $C_9H_9BrO_2$.

A new base *hydro-ethyl salicylamide*, $C_{27}H_{30}N_2O_3$, was formed by the action of alcoholic ammonia upon the hydride of ethyl-salicyl, according to the following equation :—

$$3C_9H_{10}O_2+{}_2H_3N=C_{27}H_{30}N_2O_3+3H_2O;$$

and from this again, by the action of heat, another base which is isomeric with the last, named *ethylsalidine*, the platinum salt of which was prepared and analysed.

By the action of nitric acid upon the hydride of ethyl-salicyl, two bodies appeared to be formed; the first is a yellow oil, heavier than water, assumed to be the *hydride of nitro-ethyl-salicyl*, and the ultimate product gave on analysis numbers corresponding to those demanded by ethyl nitrosalicylic acid. Their production was thus accounted for—

$$\text{I. } C_7H_5(C_2H_5)O_2+HNO_3=C_7H_4(NO_2)\,C_2H_5)O_2+H_2O.$$

$$\text{II. } C_7H_4(NO_2)(C_2H_5)O_2+O=C_7H_4(NO_2)(C_2H_5)O_3$$

The second stage of this oxidation was said to be analogous to the conversion of aldehyde into acetic acid.

The author further described the compounds resulting from the actions of the iodides of allyl and amyl respectively upon hydride of sodium-salicyl (salicylate of sodium), and concludes with some interesting theoretical considerations arising out of the results of his investigation. It is thus proved that the replacement by alcohol radicals, of the hydrogen usually regarded as alcoholic in the hydride of salicyl, does not in any way interfere with its aldehydic characters. The formation of hydrosalycylamide from the same hydride by the action of ammonia affords, on the other hand, a demonstration of the fact that its properties as an alcohol or hydrate remain unimpaired.

Dr. J. H. Gladstone communicated some further particulars of his research "*On Pyrophosphoric Acid.*" According to Graham's original view of the constitution of this acid, when regarded as $2HO,PO_5$, it was believed to be a bibasic acid. Since the atomic weight of oxygen has been doubled its formula is written $2H_2O,P_2O_5=P_2H_4O_7$, and it becomes a tetrabasic acid. The correctness of the latter view receives support from the existence of amides containing one, two, and even three molecules of NH_2 in the place of HO. The present paper treats of some normal pyrophosphates and certain allotropic modifications of these salts, and indicates the constitution of the acid as deduced from several modes of formation. The author confirms Schwartzenburg's analysis of the ferric pyrophosphate, $P_2fe_4O_7+3H_2O$,* and suggests the existence of a soluble double salt $P_2Na_2fe_2O_7$. The cupric salt was found to have a similar composition, but contained only two atoms of water. Dr. Gladstone mentioned some remarkable facts tending to establish the existence of an allotropic ferric pyrophosphate. Thus, if a solution of sodium pyrophosphate be mixed with a large excess of sulphuric acid and ferric chloride then added, there is no precipitate in the cold, but, on heating, a white flocculent compound is formed, which differs from the ordinary modification of that substance by being insoluble either in dilute mineral acids, ferric chloride, or in alkaline pyrophosphates. A quantity of this substance was prepared and analysed. Its composition was found to be identical with that previously recognised—viz., $P_2fe_4O_7,3H_2O$. It is proposed to use this reaction as a test for the acid in question. Similar results were observed in the case of copper. Regarding the modes of preparation, it is stated that the pyrophosphates had hitherto been obtained by the action of heat upon the orthophosphates, but Dr. Gladstone succeeded in producing them by dissolving phosphoric anhydride in an alcoholic solution of hydrate of potassium or other alkaline base; or if the oxychloride of phosphorus be dropped into a strong aqueous solution of the same hydrate the product was identical. That this result is true in the case of the strongest ammonia, had been previously shown. The author concluded by tracing the formation of pyrophosphoric acid in stages, as follows ;—

$$\text{I. } PCl_5+H_2O=PCl_3O+2HCl.$$

$$\text{II. } 2\left\{\begin{array}{c}Cl\\PClO\\Cl\end{array}\right\}+H_2O=2HCl+\left.\begin{array}{c}PCl_2O\\PCl_2O\end{array}\right\}O$$

$$\text{III. } \left.\begin{array}{c}PCl_2O\\PCl_2O\end{array}\right\}O+4H_2O=4HCl+\left.\begin{array}{c}P(HO)_2O\\P(HO)_2O\end{array}\right\}O$$

Pyrophosphoric acid, $P_2H_4O_7$.

The limited space at our disposal in the present number compels us to defer until next week a full account of the remaining communications.

The titles of the other papers which were read at the meeting were:—"*Water Analysis; Determination of the Nitrogenous Organic Matter,*" by Messrs. J. A. Wanklyn, E. T. Chapman, and Miles H. Smith. "*Analysis of a Biliary Concretion, and on a New Method of Preparing Biliverdine,*" by Dr. T. L. Phipson. "*The Action of Chloride of Iodine on Picric Acid,*" by Dr. John Stenhouse. "*On Julius' Chloride of Carbon, C_6Cl_6.*" by Mr. Henry Bassett.

The President moved a vote of thanks to the authors of these communications, and adjourned the meeting until after the recess.

NOTICES OF BOOKS.

Analysis, etc., of Coal Gas. By Rev. W. R. Bowditch, M.A., F.C.S. London: E. and F. V. Spon, 16 Bucklersbury. 1867.

Nothing in the history of chemistry offers a parallel to the extraordinary series of results obtained by the close study of the products of the distillation of coal. The matter has been treated by almost every chemist of eminence, and an endless series of new products have enriched the philosopher, chemist, physician, economist, photographer, and dyer. No worthier or abler historian of coal gas, as regards its purification and use, could well be found than Mr.

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* Ferricum (fe)—18·66. (Williamson.)

Bowditch, who has, year after year, by his discoveries fairly earned the name of a public benefactor. The chemical readers will obtain from Mr. Bowditch facts not to be found elsewhere, and before unknown to him, and also a more forcible expression of fragmentary knowledge contained in scientific works, in which diffusion of matter is so often unavoidable, and whose only remedy is a compilation of this kind. The book is of necessity, as regards its mass, a compilation, but it is also a comparison of the author's own results with those of other authors, whether antecedent or subsequent.

Perhaps the greatest, certainly the most obvious, of the benefits for which we have to thank Mr. Bowditch, is the attention that he has drawn to the effects arising from carburetting or naphthalising gas. Indeed, if we mistake not, the spirit of the present works points to a division of labour—to operations necessary at the place of manufacture, not to be confused with others more expedient at the place of consumption—but a strong and forcible expression of this is wanting. On the question of carburetting gas, we find: "The large proportion of carbon in naphthalin pointed it out as being fitted to yield much light with little heat, if it could be burnt. But only a short time ago it was the prevailing opinion among chemists that naphthalin could not be so burnt. At the instant of liberation it crystallises, and so clogs the pipe, being regarded by the manufacturer as a waste product only—and a very inconvenient one too. Experiment also proves that ordinary gas at common atmospheric pressures and temperatures has no power to combine with the crystallised naphthalin." With these facts in mind, Mr. Bowditch contrived an apparatus, which he fully describes, on the principle of using naphthalin when in an available state—*i. e.*, not as a solid; and this object is effected by the gas burner itself, which so by its own waste heat supplies to itself increased light—a simple device, but, as will be shown, of great practical application. Carbolen is used for the apparatus, a commercial general term for these heavy oils, differing so little from naphthalin that the latter, as being a definite chemical body, is taken as its type. A gallon of this, weighing 10 lbs., costing 2s. 6d., yields a light of 14,000 candles. As a simple tabulated result we find—

	Weight burnt.	Light.	Cost. £	s.	d.
Naphthalin .	10 lbs.	14,000 candles	0	2	6
Gas . .	280 lbs.	14,000 "	1	17	4

For equal *weights* the heating powers of gas and naphthalin are as 12,000 to 8,786. But add to this fact one more important still—or rather the sister fact to it—viz., that all the carbon added by the method gives light, and so lessens the amount of gas used. No unnecessary carbonic acid being formed from the marsh gas and carbonic oxide unused (if not carburetted) for lighting purposes, the smaller quantity of carbonic acid, of course, as will be at once seen, is not so as a total result, but is so per unit of light, an excess of the material being thus avoided.

The reader will grant also to the author his other two statements of minor importance—first, the greater steadiness of carburetted light; for "highly carburetted gas cannot be burnt without burners of small aperture and under considerable pressure, without smoke and diminished light"—these are the very conditions of a steady light. The second point is that by the light of carburetted gas we can distinguish colours with almost the same ease, distinctness, and accuracy as by daylight—this as a result of quality, not of more quantity of light.

This also the chemist might expect; but he could not expect the following strange result:—"In fact, the spectrum of my light approaches the spectrum of sunlight more nearly than that of any other artificial light does." The materials used by the inventor of the apparatus are cymol and naphthalin—both very cheap and abundant. As regards the safety of these materials, "they will not burn alone, but require the heating power of a combustible gas to sustain the combustion. When lighted matches, candles, etc., were plunged into the materials at 100° C., the lights were extinguished as they would have been by immersion in water, and in no instance were the hydrocarbons ignited." The officers of insurance companies state that they are unacquainted with anything in use as an illuminant which is so safe. To conclude the question, Laurent has found that naphthalin thrown into a red-hot crucible volatilises undecomposed, and condenses in the air in snowy spangles. For our part we should like to know if any purification of the so-called carbolen is required for the separation of the sulphur which is taken up by naphthalin in very notable quantity, and what is the extra cost involved by such purification. If the crude oil is to be used, it would be necessary to purify from sulphur at the place of manufacture; otherwise, for the same amount of light, if the carbonic acid be less, the sulphurous acid will be more, and poisoning by the former is preferable to the same result by the latter, with less general destruction of property.

We have followed Mr. Bowditch so fully in this portion of his work that we cannot treat other most interesting subjects at the same length. It is sufficient to say that the same comprehensiveness and enthusiasm in the subject are features common to every chapter. A full history is given of M. Berthelot's synthesis of acetylen, and the great chain of results hanging upon that discovery up to the time when the book went to press. One fact, stated in a very modest way, will doubtless be thought worthy of greater interest by insurance companies. We allude to the proved gnawing of gas-pipes by rats as a probable explanation of sudden gas explosions, caused, of course, as it was formerly said, by carelessness in turning off the gas. A diagram of a pipe so injured is appended. An iron pipe it was; but the dangers attending lead are of course much greater.

Methods are given *seriatim* for tar-testing in coal gas. "Some of the oils so dissolved from gas are of such a high boiling-point that they cannot be distilled, except at a temperature which *softens Florence flasks*, and their density is far greater than [that of?] water." Kraut's flame test for ammonia in gas by combustion is quoted from the Chemical News, No. 311; also, Dr. Hofmann's original demonstration of the inflammability of ammonia. The author remarks:—"I must caution inexperienced manipulators against buying their test-papers. I have seen purchased turmeric paper, held in a full stream of scandalously ammoniacal gas for more than a minute, remain perfectly unaffected; whereas properly made paper became red-brown in a single second. Managers and boards of directors have been so known to have been deceived."

As regards the difficulty of obtaining tri-ethy-phosphine for testing for bisulphide of carbon, it is remarked that it is not sold as an article of commerce, and only experienced chemists can make it. Mr. Bowditch has known 5*l.* to have been offered and refused for an ounce of this compound. It is therefore practically inapplicable as a test. This makes Mr. Bowditch's own test for the same compound by solution in alcohol, and subsequent dilution by water, the more valuable. A figure of the necessary apparatus is given, with a full description. The author has also thrown much light upon sulphur generally in gas, founded upon the power (as our readers doubtless remember) of heated lime to *act upon all* the sulphur compounds in gas. Of course, the origin of such sulphur is not learnt thus. As regards sulphur testing, we come to some statements that will be of the greatest interest to chemists. "I have passed this sulphur compound into *nitroprusside of sodium* for some time, and have failed to have obtained any indication of the presence of a sulphide; and *yet lead paper has been coloured.* I think I can name the compound, and, indeed, I have done so to friends in private; but as I am not prepared here to give proof of the accuracy of the conjecture, I forbear." Also, from steps in the process specified, the author continues:—"I therefore conclude that the blackening of the lead paper in the above case was not due to hydrosulpho-carbonic acid, but to some unknown or unsuspected compound."

Mr. Bowditch has found by experiment that when the test candle is thinned, *either an increase or decrease* of consumption may result—which is very satisfactory to us, for it serves to show that, apart from the clumsiness and inaccuracy of expression involved by the use of the term "so many candles," a positive radical evil attends their use. Arago by his photometer was on the more trustworthy track, and to his method, improved so as to be practically worked, all experience tends to point. The author gives us the outline of a new and ingenious scheme for the improvement of the use of candles, which he calls "the new photometric candle balance and elevator;" we hope, however, that he will turn his energies to the question of examining light by any refractive or polarising means, admitting of a concise expression of results. It will not be the first time that Mr. Bowditch will have rendered a useful original process useless by a subsequent and more perfect one.

Ure's Dictionary of Arts, Manufactures, and Mines. Edited by Robert Hunt, F.R.S., F.S.S. Fifth Edition. Chiefly rewritten and greatly enlarged. 3 vols. Longmans.

A Dictionary of Science, Literature, and Art. Edited by W. T. Brande, D.C.L., F.R.S.L. and E., of Her Majesty's Mint, and Rev. George W. Cox, M.A., late Scholar of Trinity College, Oxford. 3 vols. Longmans.

The new editions of these two well-known dictionaries, each consisting of some 3000 pages, and each dealing largely with chemistry, are now before us, and it behoves us to offer some criticism upon them. Few tasks could be more difficult. We might indeed, with perfect honesty, confine ourselves to general praise, speak of the great difficulties which attend the compilation of such works, and point out the number of admirable articles, and the vast mass of important information which each contains. But if we approach the work of criticism, and, while bestowing fair praise on their many excellences, endeavour to point out some of their faults of fact and arrangement, we are met by the fear lest, our praise being necessarily vague while our objections must needs be specific, we should end by leaving upon our readers' minds an unfair impression of the true worth of the books. The criticism which employs itself in picking out small errors and blemishes from a substantially good book, arraying them in rows, and parading them before the eyes of the world, is unfair and ungracious, and cannot be atoned for by any amount of indefinite praise. We should be sorry to be betrayed into injustice of this kind, and shall therefore deal as lightly as possible with minor blemishes, and shall only point out errors of fact when they appear to go hand in hand with faults of method.

Two general objections will, however, suggest themselves to the chemist, on his first glance at the new dictionaries—for new they are, to all intents and purposes. One of these, indeed, they share with but too many modern works of reference. Original authorities are not quoted in either book, nearly as often, or as fully, as they should be; and this, we fear, will curtail their usefulness to scientific men in a marked manner. To select an illustration at random. Suppose a chemist wishes to "read up" Madder. He turns to Ure, and there finds an excellent article by Dr. Schunck, in which the results of Persoz, Runge, Robiguet, and many other experimenters, besides those of the writer, are chronicled; but scarcely in one single instance will he find a direct reference to the memoirs in which those results were first announced. This is hardly fair. The original memoirs are often just what the chemist wishes to get hold of, and he has a right to expect that his dictionary shall help him in what is generally a troublesome and laborious search.

The other objection is a less serious one. We, of course, are not likely to quarrel with editors for putting too much chemistry into their books, but we cannot but think it would have been better to have left to treatises on pure chemistry a good deal that is included in these volumes. Would it not have been better, for instance, in a technological work like Ure, to have omitted such articles as "atoms," "atomic theory," "combining numbers," and "equivalents"? And what possible object can there be in describing such compounds as "hederic acid," "ambreic acid," "chloriodoform," "pittacal," and "anisidine," all of which, and many more as unimportant, are to be found in one or other of the dictionaries? When every line of space is so valuable, the greatest care should be exercised in the selection of matter, and the excision of these irrelevant subjects would have allowed of a fuller treatment of many which now fare badly.

And now, could we but afford the space, we should enter upon a more detailed examination of the works before us. Mr. Robert Hunt, the editor of "Ure," has done his work conscientiously and well, and each of his volumes is crowded with articles of the utmost interest and importance to the practical chemist. We can do no more than give the titles of a few of them. "Coal-gas," an admirable treatise, 60 pages long, by Dr. Frankland; "Bread," by Dr. Normandy; "Bleaching," and "Calico-printing," "Caoutchouc," "Manure," by Dr. Voelcker; "Sugar," by Mr. Fryer; "Candles," "Glass," "Disinfectants," by Dr. Angus Smith; and "Naphtha," by C. Greville Williams, are all elaborate and most valuable articles. Mr. Greville Williams's contributions to the work are numerous and uniformly well written. Dr. Schunck and Dr. Noad have likewise been large contributors, the article "Iron," by the latter, occupying over 80 pages.

The smaller articles, though frequently leaving much to be desired, are on the whole satisfactory. They seem generally to have been compiled with care, and the editor has wisely allotted them a fair proportion of the space at his command, and has not suffered them to be entirely swamped by the more important ones. We are, nevertheless, inclined to believe that they might advantageously have filled an even larger proportion of the book than they do: for the true province of a book like this is to supply knowledge which cannot easily be obtained elsewhere, and important manufactures usually have special treatises devoted to them, whereas the others can only hope to find their places in a work of reference. We have purposely said nothing of some very important sections of the book. The metallurgy alone would form a good-sized manual, and the descriptions of machinery and mechanical processes are probably quite as extensive; but our task would be even more hopeless than it it is now, if we attempted to comment on subjects such as these. We leave the book safely to our readers' criticism, convinced that very few chemists will be able to do without it.

"Brande's Dictionary," the republication of which is only just completed, is very different from the preceding work in object and execution. Much of it, of course, deals with subjects with which we have no concern, and upon which we have no right to speak; but as chemistry, naturally enough, occupies a somewhat prominent position in it, we have a fair *locus standi*. We, indeed, almost wish we had not, for the respect which every chemist must feel for the memory of its original editor makes it an unpleasant task to disparage it. But the truth must be told, and although we admit very cheerfully that many valuble chemical articles are to be found in it, it is on the whole a most unsatisfactory epitome of modern chemical knowledge. In regard to theory this is not to be wondered at. No one expected any very cordial recognition of "croquet-ball" atoms, though we might have begged that our teeth might not be set on edge by barbarous and long-exploded definitions and doctrines coeval with Lavoisier. But we surely have a right to expect accuracy in facts. We do not complain that there are so few formulæ in the book, although the most out-of-the-way compounds are described. We will not quarrel with CH_2 as a representation of marsh-gas or C_4H_3 for acetyl, and we will only object on the score of convenience to our old friend CS_2 being described as carburet of sulphur! (we only found the compound by chance), but we must assert in the most earnest manner that the atomic weight of chlorine is *not* 36, nor that of idone 126, nor that of bromine *about* 78.

And, moreover, if we concede C_4H_3 as the formula for acetyl, we must refuse our assent to the further proposition that acetamide is "derived from ammonia by the replacement of one equivalent of hydrogen by acetyle. There is, it appears, a species of lead poisoning to which compositors are liable, and which is called "wrist-drop." "It may be cured," we are told, by soaking the hand in a solution of potassium and eliminating the lead." We do not attempt to deny it; but we anticipate some difficulty in applying the cure.

Principes de Chimie, fondée sur les Théories modernes. Par A. Naquet, Professeur agrégé à la Faculté de Médecine de Paris, &c. 2e édition, considérablement augmentée. Paris: Savy. 1867.

Leçons élementaires de Chimie moderne. Par M. Ad. Wurtz. Doyen de la Faculté de Médecine de Paris. Premier fascicule. Paris: Masson. 1867.

Traités Elémentaires de Chimie Médicale. Par Ad. Wurtz, Parts I. et II. Paris: Masson. 1864-5.

M. Naquet's little book, published less than two years ago, has already reached a second edition, and we are glad to find that the author has taken the opportunity of increasing its bulk very considerably. The book is no doubt well known to many of our readers, and we believe that no one who has seen it will hesitate to join in our cordial praise of it. It is a well written, clear, and succinct account of the leading doctrines of modern chemistry, and is, in the best sense of the word, an original work. We do not not mean that the author has invented a new system of classification, of nomenclature, or of notation, or that he crowds his pages with records of his own discoveries, but simply that every chapter he writes bears the stamp of individual thought—that the facts and theories he describes are presented, not as raw material cut by a compiler's scissors from a hundred different sources, but digested, assimilated, and, so to speak, organised in the brain of an author who takes the trouble to make a thing his own before issuing it to others. The book is too well known to require a detailed description from us, even if we could afford the space for one, and we will content ourselves with glancing at a few of the new additions.*

M. Naquet, wisely, as we think, devotes a hundred pages at the beginning of his book to laws and general principles, thus dissenting from the fashion which appears to have taken root in England of interspersing everything of the kind, theories of atomicity, rules of nomenclature, laws of combination, and the like, among the sections on the history of the elements. Many portions of this introductory matter have been entirely rewritten, and all have received some modification. At page 22 we find an historical sketch of the great "dissociation" controversy, the author finally giving in his adhesion to that remarkably convenient discovery. The section on the atomicity radicals contains, of course, an account of Kekulé's views very clearly stated. Hofmann's term "quantivalence" is adopted, and is employed as distinct from "atomicity," the atomicity of an element being regarded as invariable, while its quantivalence may be subject to change. We think the author has made a mistake in devoting three pages to the absurd hypothesis of sub-atoms invented to account for the observed variations, in atomicity, but we can easily pardon it for the sake of the distinct rejection which is accorded to it. We are disposed to agree heartily with the following passage from M. Naquet: "Quant à nous, ne voulant pas aller au delà des faits, nous admettons que des molécules non saturées existent à l'état de liberté, parce que l'oxyde de carbone et beaucoup d'autres corps en fournissent les preuves, et nous admettons aussi que les radicaux d'atomicité impaire peuvent exister sans se doubler, parce que cela a lieu pour le bioxyde d'azote AxO, et l'hypoazotide AzO_2." †—Page 48.

With respect to two of the gravest difficulties on the new views, the compounds $AgNaCl_2$ and ICl_3, our author rejects Kekulé's hypothesis of "molecular combination," and prefers to regard chlorine and iodine as being triatomic. The wisdom of such an assumption is, however, extremely questionable.

In the section on acids, bases, and salts, the views of Cannizzaro, Wurtz, and Kekulé are described with a clearness which we have never seen surpassed. No student could have the least difficulty in mastering them.

Before leaving the book, we must, however, advert to one or two somewhat important omission—omissions which surprise us in such a carefully edited manual. In the account of ozone the fact that when the ozone is absorbed from ozonised oxygen, no contraction of volume is observed, is duly noted, and the hypothetical explanation of this fact suggested by Dr. Odling—namely, that the true formula for ozone is O_2O, and that the removal of one atom of oxygen, therefore, leaves the volume unchanged—is likewise given; but the beautiful experimental verification of this hypothesis recently supplied by M. Soret, we succeeded in absorbing the whole molecule of ozone by oil of turpentine, is unaccountably omitted. The other omission is even more important. The author is evidently ignorant of the splendid memoir of H. Kopp an atomic heats, which was published in the *Philosophical Transactions* for 1865. The result is, that he not only assumes the atomic heats of all elements except carbon, boron, and silicon, to be identical; but he makes the grave mistake of deducing the molecular constitution of water from the specific heat of water, instead of from that of ice.

M. Wurtz, two of whose manuals we take this opportunity of noticing, is certainly a most diligent writer. Besides his completed works, he has three books at present in progress. The titles of two of them are given at the head of this article, while the third, a large dictionary of chemstry, is announced for speedy publication. Of the *Chimie Médicale* two parts have already appeared, one devoted to inorganic, the other to organic chemistry. The third, on "Chimie Biologique," has been expected for some time. As far as it has gone, it is a very good and practical work. Its title, however, hardly gives a true idea of it, for it is simply a manual of chemistry, well written and arranged in a clear though somewhat old-fashioned manner. The only thing peculiar about it is that all subjects especially interesting to medical men, such as mineral waters, substances used in pharmacy, the detection of poisons, and the like, are brought into marked prominence, and are treated in great detail. Strange to say, the old equivalents are employed throughout. The author apologises for it in his preface, by alleging the necessity of conforming to the official teaching of Paris; but HO, in a work by M. Wurtz, seems almost as much out of place as an account of phlogiston would be.

Only one part of the *Leçons* has as yet reached us, and it is not of a character to call for any particular remark. It is simple, clear, and concise; but though the new atomic weights are adopted in it, it lacks the original and scientific arrangement which distinguishes M. Naquet's *Principes*. In spite of some advantages, we cannot but regard it as far inferior to the last-named work.

Theology and Natural Science; their Mutual Relations. A lecture by J. H. Gladstone, F.R.S. Jos. Nisbet and Co.

Dr. Gladstone has endeavoured to show in the present lecture how the study of natural science, being the study of one volume which has issued from the Divine Being, prepares the mind for the reception of the truths delivered in the companion volume of God's word. Both are difficult studies, and, in many cases wrong interpretations are arrived at; but the fact that the interpretation of the one record, clashes with the apparent meaning of the other, shows not

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* We are glad to hear that a translation of the new edition is in progress, and will be published before long.

† Nitric peroxide has been shown to dissociate below 70° C., and Dr. Frankland (Lecture Notes, p. 61) tries to get over the NO difficulty by regarding it as N^2O^2 dissociated at a very low temperature. But if this were the case it could only dissociate into NO and NO, and the obnoxious compound must, therefore, at ordinary temperatures, contain diatomic nitrogen.

that the original works are inconsistent the one with the other, but that our interpretations in neither case are perfect. All the inspired teachers of religion have drawn lessons from the study of nature, whereas Nature has acted the part of a terrible giant, a destructive Jupiter, or an awful Thor, in the religions that have had no revelation. Science repays the debt by clearing the mind from superstition, by exciting an earnest reverend spirit, by inducing humility of mind, clearness of definition, calmness of judgment.

The slur cast upon science, that it leads to infidelity, is well rebutted by Dr. Gladstone. He states it as his experience that there are no more irreligious men in the walks of science than in other professions. What a man is before he begins to study, that he remains. The religious man becomes more firmly convinced, the irreligious man gains greater scope for scoffing. Science is not necessarily religious, nor is it the reverse; it may be conducive to either end. Dr. Gladstone has spoken kindly, thoughtfully, and well on one of the questions of the day that touches us not as scientific men, but as *men*, who cannot be indifferent when it is sought to place science in opposition to religion.

CONTEMPORARY SCIENTIFIC PRESS.

[Under this heading it is intended to give the titles of all the chemical papers which are published in the principal scientific periodicals of the Continent. Articles which are merely reprints or abstracts of papers already noticed will be omitted. Abstracts of the more important papers here announced will appear in future numbers of the Chemical News.]

Zeitschrift des Architecten und Ingenieur-Vereins für Hannover. No. 4. 1866.

Von Kaven "*On the Elasticity and Tensile Strength of Iron and Steel.*"—Reder "*On the Action of the Atmosphere on Coal.*"—F. Rziha "*On the Cost of Manufacturing Cement.*"—Oppermann "*On Romain-Lavaud's Glass Manufactory at la Villette, Paris;*" "*On Guillaumet's Dye Works at Puteaux-sur-Seine.*"—A. Wöhler: "*Results of Experiments on the Relative Strength of Iron, Steel, and Copper.*"

Dingler's Polytechnisches Journal. No. 3, February, 1867.

A. von Waltenhofen "*On the Electro-motive Force of certain Voltaic Batteries.*"—R. Wagner "*On the Quantitative Estimation of Tannic Acid.*"—A. Ott "*On Petroleum as a Lubricant.*"—F. Kick "*On a Cheap and Rapid Method of Preparing Lecture Diagrams.*"—"*On Keeping Sodium in Paraffin Oil.*"—"*On the Use of Glycerine in Gas Meters.*"—"*On the Manufacture of Black Paraffin Candles.*"—Kekulé "*On a Simple Method of Transforming Nitrobenzol into Aniline.*"—E. F. Richter "*On the Extraction of Fixed Oils by Bisulphide of Carbon.*"—G. C. Wittstein "*On the Use of Poisonous Colours on Wearing Apparel.*"—J. Oser "*On a Simple Method of Testing Flour.*"

No. 4, February.—A. Scheurer-Kestner "*On Rivière's Method of Manufacturing Caustic Baryta.*"—C. Scheibler: "*An Apparatus for ascertaining the Quantity of Carbonic Acid contained in the Gas used for removing the Lime from Beetroot Juice.*"—H. Schulz "*On the Chemical Composition and on some Physical Properties of different kinds of Animal Charcoal.*"—H. Vohl "*On the Use of Peat for Preparing Lighting and Lubricating Materials, and for Producing Acetic Acid, Wood Naphtha, Ammoniacal Salts, &c.*"—Baeyer "*On the Reduction of Aromatic Compounds by means of Zinc.*"

No. 5, March.—H. Wagner "*On the Extraction of Copper from Poor Ores in the Wet Way.*"—R. Wagner "*On the best Method of Preserving Sodium.*"—Dangevillé and Gautin "*On a Method of Discharging Aniline Colours.*"

No. 6, March.—A. von Waltenhofen "*On a new Electro-magnetic Machine, and on the Useful Effect and Cost of such Machines.*"—C. Puscher "*On a Method of Preparing Gelatine from Glue;*" "*On the Preparation of the new Mother-of-Pearl Paper;*" "*On Landscapes in Glass, a new Fancy Article;*" "*A Cement for attaching Brass to Glass.*"—R. Linner "*On the Use of Movable Tubs as Receptacles for Excrementitious Matters at Gratz.*"—W. Stein: "*A Method of ascertaining the Presence of Free Alkalies in Soap and Alkaline Salts.*"—C. Feldmann: "*A Bottle-brush made of Cane-strips.*"—T. Wimmel "*On the Adulteration of Japanese Wax.*"

Journal für praktische Chemie. March 7, 1867.

C. G. Lautsch "*On the Saturating Capacity of Periodic Acid.*"—F. W. Fernlunds *On the same subject.*—R. Hermann "*On the Composition of Rutile from the Ilmen Mountains.*"—E. von Sommaruga "*On the Equivalents of Cobalt and Nickel.*"—M. Delafontaine "*On the Oxides of Niobium.*"—P. T. Cleve and A. E. Nordenskjöld "*On the Hydrated Silicates of Iron.*"—C. Birnbaum "*On the Action of Sulphurous Acid on Hydrated Oxide of Platinum.*"—L. J. Ingelström: "*An Analysis of some new Swedish Minerals.*"

Le Technologiste. April, 1867.

T. Becker "*On the Method in Use at Stassfurt for Estimating Potash as a Tartrate.*"—C. Giseke "*On the Detection of Free Sulphuric Acid in Sulphate of Alumina.*"—Coignet "*On a Solvent of Indigo and Cochineal.*"—L. Walkhoff "*On a Self-acting Apparatus for Drying Animal Charcoal.*"—Muth "*On a Method of Purifying Naphthaline.*"

Comptes Rendus. April 8, 1867.

A. Secchi "*On a Spectroscope for Celestial Observations.*"—A. Wurtz "*On the Transformation of Aromatic Carbides into Phenols.*"—A. Kekulé "*On some Derivatives of Benzol.*"—Marié-Davy "*On the Electro-motive Force of Voltaic Batteries.*"—P. Christofle and H. Bouilhet: "*Observations on Dufresne's Memoir on a process for Gilding and Silvering by Amalgamation without Danger to the Workmen.*"—E. Duchemin "*On the Use of Picric Acid in Voltaic Batteries.*"—Berthelot "*On a Method of Reducing and Saturating Organic Compounds with Hydrogen.*"—Dubrunfaut "*On the Presence and Formation of Crystallisable Sugar in the Tubers of the Jerusalem Artichoke.*"

Poggendorff's Annalen. March 15, 1867.

G. Magnus "*On the Influence of the Adhesion of Vapour in Experiments on the Absorption of Heat.*"—E. Voit "*On the Diffusion of Liquids.*"—E Zettnow: "*Contributions to the Knowledge of Wolfram and its Compounds;*" "*On a new Method of Qualitative Analysis, in which Sulphuretted Hydrogen and Sulphide of Ammonium are dispensed with.*"—R. Weber "*On the Theory of the Manufacture of Sulphuric Acid.*"—W. Holtz "*On the Construction of Induction Machines for obtaining Electricity of High Tension.*"—R. Schneider: "*Remarks on Von Sommaruga's Memoir on the Equivalents of Nickel and Cobalt.*"—A. Brio "*On the Crystalline Form and Optical Properties of Formiate of Cadmium and Baryta.*"

Annalen der Chemie und Pharmacie. March, 1867.

R. Bunsen "*On a Method of Estimating the Specific Gravity of Vapours and Gases.*"—M. von Pettenkofer and K. Voit "*On the Quantity of Carbonic Acid given out, and the Oxygen consumed, by the Human Subject during Waking and Sleeping.*"—W. Schlebusch "*On the Chlorinated Substitution Products of Fatty Acids.*"—H. Hlasiwetz and A. Grabowski "*On Carminic Acid.*"—G. Malik "*On a Derivative of Rufigallic Acid.*"—A. Gescher "*On Sulphide of Copper and Ammonium.*"—W. Heintz "*On the Action of Carbonate of Ammonia on Chloracetic Ether.*"—R. Otto and H. Ostrop "*On Benzo-sulphurous Acid.*"

Romberg's Zeitschrift für praktische Baukunst.

Nos. 1–3. 1867.

Brad "*On a Method of Testing the Capability of Natural* *and Artificial Stone for Resisting Frost and Wet.*"—SCHMOOK "*On the Impregnation of Wood with Preservative Solutions.*"—SUVERN: "*A Method of Purifying the Waste Waters of Sugar and other Manufactories.*"

Journal des Fabricants de Papier. March 1, 1867.

E. BOURDILLIAT "*On Testing the Chemical Products used in Paper-making.*"

NOTICES OF PATENTS.

2110. *Treating Fatty and Oily Matters.* G. PAYNE, Battersea. August 16, 1866.

AN increased quantity of fatty acids is thus obtained. After the breaking up of any fat into its acid, and glycerine as its base, by sulphuric acid. as a further treatment the fatty acid is combined with an inorganic base (as litharge or potash), and then, by a stronger acid, the fatty acid is again liberated and may be distilled off.

2101. *Steel and Iron Manufacture.* J. CAMERON, Mount Pleasant, Barrow-in-Furness. August 15, 1866.

THE Bessemer process is modified by the use of lime, calcic, barytic, sodic, potassic, and ferric carbonates; with other iron ores, fluor spar and salt, as fluxes.

2083. *Production of Green Colouring Matter for Dyeing and Printing Textile Fabrics and Yarns.* J. A. WANKLYN, City, London, and A. PARAF, Manchester. August 14, 1866.—Not proceeded with.

EQUAL weights of rosaniline and an alcohol or suitable solvent are taken, with ethylic or isopropylic iodide (or other replacer of hydrogen), and are subjected to a temperature 230° Fahr. for three hours under pressure. The solution of sodic carbonate (one percentage strength) is added to the product in the proportion of four times its weight. There results a green dye in solution, and violet dye as a precipitate. The latter is converted by soda ley into a nearly colourless base, with violet salts. The base is powdered again, heated to 230° Fahr., and treated as before, more green solution resulting, and so on for two or three times until all is convered into green dye.

2115. *Use and Application of an Inorganic Glyceric Ether.* A. PARAF, Manchester. Dated August 17, 1866.

DEPENDS on the formation of a neutral arsenite which is of value in madder dyeing. Arsenious acid of commerce is dissolved in an equal weight of glycerine. This so-called arsenious glycerine ether, with residual arsenic dissolved in glycerine, easily decomposes in presence of steam, leaving the insoluble arsenious acid. 1 lb. of the ether mixed with 2 oz. of any crystallised aniline colour is dissolved in starch solution; steaming for about half an hour deposits the acid, and fixes the colouring matter in a printed textile fabric.

Communicated by Mr. VAUGHAN, F.C.S., PATENT AGENT, 54, Chancery Lane, W.C.

GRANTS OF PROVISIONAL PROTECTION FOR SIX MONTHS.

133. W. Weldon, Park Villa, West Hill, Highgate, Middlesex, "Improvements in the manufacture of chlorine, and in the production of artificial oxides of manganese for employment both in that and other manufactures."—Petition recorded January 18, 1867.

284. J. Buhrer, Munich, Bavaria, and A. P. Price, Lincoln's Inn Fields, Middlesex, "Improvements in effecting the distillation of coal, shale, wood, peat, and other bituminous or carbonaceous substances."—February 1, 1867.

1122. J. Hargreaves, Appleton-within-Widnes, Lancashire, "Improvements in the manufacture of steel and soft iron from cast iron."—April 16, 1867.

1356. C. D. Abel, Southampton Buildings, Chancery Lane, "A new or improved method and apparatus for converting the gaseous products of combustion into combustible gases."—A communication from N. Lebedeff, St. Petersburg, Russia.—May 8, 1867.

1382. G. McKenzie, Glasgow, N. B., "Improvements in the manufacture of illuminating gas."—May 10, 1867.

NOTICES TO PROCEED.

39. B. Biggs, Laurence Pountney Hill, London, "Improvements in and applicable to candles."—Petition recorded January 7, 1867.

87. W. G. Blagden, Hackney Wick, Middlesex, "An improved method of separating silver from lead."—A communication from F. Marquez-Millan, Rue Liandier, Marseilles,—January 14, 1867.

119. E. Süvern, Halle, Prussia, "An improved mode of, and apparatus for, purifying the impure waters emanating from sugar factories and other industrial establishments, applicable also to the purification of sewage water."—January 17, 1867.

605. S. Newington, Ticehurst, Sussex, "An improved compound for destroying insects and preventing and checking blight in plants."—March 4, 1867.

Communicated by Mr. VAUGHAN, F.C.S., PATENT AGENT, 54, Chancery Lane, W. C.

GRANTS OF PROVISIONAL PROTECTION FOR SIX MONTHS.

1272. P. Salmon, Westminster, Middlesex, "Improvements in the manufacture of gas, and in apparatus for holding the same."—Petition recorded May 2, 1867.

1345. W. E. Newton, Chancery Lane, "Improvements in explosive compounds, and in the means of igniting the same." A communication from A. Nobel, Rue St. Sebastien, Paris.—May 7, 1867.

1408. G. A. Neumeyer, Döbitz, Prussia, "Improvements in gunpowder for mining purposes."

1409. J. G. N. Alleyne, Alfreton, Derbyshire, "Improvements in puddling furnaces, also applicable to other furnaces of similar construction."

1411. G. Lunge, Ph.D., South Shields, Durham, "Improvements in the preparation of ores, metals, and other substances for working in furnaces."

1416. W. E. de Bourran, Rue Hustin, Bordeaux, France, "Improvements in evaporators for concentrating saccharine fluids."—May 13, 1867.

1464. W. R. Lake, Southampton Buildings, Chancery Lane, "Improvements in the manufacture of white lead." A communication from T. M. Fell and A. G. Fell, New York, U.S.A.—May 17, 1867.

1472. T. Richardson, Newcastle-on-Tyne, "Improvements in the extraction of oils from vegetable substances."—May 18, 1867.

NOTICES TO PROCEED.

131. J. G. Franklin, Broadway, Somersetshire, "Improvements in tanning."

133. W. Weldon, Park Villa, West Hill, Highgate, Middlesex, "Improvements in the manufactnre of chlorine, and in the production of artificial oxides of manganese for employment both in that and other manufactures."

134. W. Weldon, Park Villa, West Hill, Highgate, Middlesex, "An improved method of manufacturing chlorine."—Petitions recorded January 18, 1867.

148. G. L. Loversidge, Greenfield, Saddleworth, Yorkshire, "Improvements in the tanning of hides and skins, and in the apparatus employed therein."

152. J. Rowley, Camberwell, Surrey, "An improved process for hardening, bleaching, and sweetening crude paraffin."—January 21, 1867.

172. H. A. Bonneville, Rue de Mont Thabor, Paris, "A

new and improved process of treating skins in order to separate therefrom the hair and wool, and in the preparation of the hair for the manufacture of hats." A communication from A. Fryssé, Junior, Rue Croix-des-Petits-Champs, Paris."—January 23, 1867.

285. W. E. Newton, Chancery Lane, "An improved process for obtaining metals from their ores." A communication from J. N. Wyckoff, Brooklyn, New York, U.S.A. —February 1, 1867.

379. W. Clark, Chancery Lane, "Improvements in preserving animal or vegetable matters, whether fluid or solid, in a wholesome and edible condition, without material loss or change in their natural flavour." A communication from L. H. Spear, Braintree, Orange, Vermont, U.S.A.—February 9, 1867.

684. H. A. Bonneville, Rue du Mont Thabor, Paris, "A new and improved means of preserving solutions of certain plants and matters in a concentrated state." A communication from C. d'Estains, Rue de Chaillot, Paris.—March 11, 1867.

1151. S. V. Lee, Macclesfield, Cheshire, and C. E. Lankester, Peckham, Surrey, "Improvements in the manufacture of colours and the extraction of colouring and dyeing matters from coal oil, shale oil, or coal tar, combined with peat or turf, and for a combination of any of these substances, and also for the utilisation of the residue for the purpose of what is known as moulders' blacking, after the colouring and dyeing matters have been extracted." —April 20, 1867.

1295. J. Heaton, Langley Mill, Derbyshire, "Improvements in the conversion of cast iron into wrought iron, part of which improvement is also applicable to the conversion of cast iron into steel."—May 3, 1867.

Patent Vital Force.

The great mystery of life, that sages have meditated upon, and philosophers wondered at, in all ages—the Archæus of Paracelsus, the Anima of Aristotle, the Vital spirits of the older physiologists—has at last been cleared up by a brilliant genius who resides in Paris. M. Martin Ziegler has not only demonstrated the existence of a "vital fluid," but has even made the astounding discovery that it is disengaged "whenever azote and carbon are brought into contact." His method is simple—all grand truths are simple —he immerses a porous cell containing ammonia in a vessel filled with treacle! The end of a silk thread is placed in each, and then, on connecting them, "the circuit is closed, and the current of vital fluid passes," capable of producing "on an animated being very considerable effects." No wonder the lucky inventor rushed off and patented his invention at once. He does not, it is true, tell us the precise nature of the "very considerable effects" which he is able to produce, but we can of course have no difficulty in guessing at them. We picture to ourselves the tottering steps and shrivelled limbs of the scarce "animated being," who will seek the patentee's laboratory, his stock of "vital fluid" well-nigh exhausted, and we see him return, after a brief exposure to the life-giving treacle, with the energy and fire of manhood's prime, and with a stock of "vital fluid" enough to last for another fifty years! We can fancy the inventor, with a cask of treacle and a carboy of ammonia, operating at the Morgue before a distinguished assemblage, and bringing back the warm tone of life and health to the cheeks and limbs of the ghastly corpses arranged there. Nay, the experience of Pygmalion himself seems hardly incredible in the light of this new discovery.

We could wish, however, for a little more detail. We should very much like to learn the equivalent in treacle of the vital force of an average man. How many pounds of treacle would be required to bring a man from seventy back to twenty? Would rejuvenescence be a *very* expensive process? And we feel no little anxiety to know whether we are justified in going on living without paying a royalty to M. Martin Ziegler.

We have not been playing upon our readers' credulity. The patent we have been describing has actually been granted to the well-known patent agent Mr. Brooman. Its date is October 3, 1866, and its number 2536. We recommend our readers to examine it, for a more powerful satire on the present state of the patent laws we have never seen. From the following quotations the character of the document can be told:—

"Whenever azote and carbon are brought into contact, whether an azoted body and a carbonated body, or even a body strongly azoted and another which is only feebly azoted, there is disengaged an imponderable fluid, the presence of which is made known by particular effects on the organism of living things, animal or vegetable. This fluid, which the present inventor calls the vital or organic fluid, is thus a new agent generated in the midst of chemical circumstances, like heat, light, and electricity. It is collected, manifested, and transmitted by currents like the electric fluid, as hereafter explained, but notwithstanding that the phenomena by which it manifests itself have a certain analogy with electric phenomena, this vital or organic fluid has really an existence of itself, autonomous and independent, and the best proof of this is, that its currents can pass through conductors which are insulating for electricity. . . . .

"Azoted bodies are those which are the best conductors of the vital or organic fluids, and among them preference is given to silk, which has the advantage of intercepting the electrical currents, the intervention of which would be inconvenient. As insulating bodies of the vital fluid I may mention glass, enamels, and minerals in general.

"The present inventor has also observed that if an acid or an alkali be made to act upon an organic matter a large quantity of vegetable or organic fluid is disengaged; also that if the organic matter is devoid of azote the disengagement of the fluid takes place if an acid or an alkali be made to react upon a hydrocarburet, or even upon carbon.

"I now proceed to describe several arangements of apparatus for producing the vital or organic fluid, which are comparable in one point of view with voltaic piles or batteries. . . . .

"The following is a good arrangement:—A porous vessel or bladder is filled with caustic ammonia and immersed up to the neck in molasses contained in an ordinary vessel; a silk thread is attached to the neck of the porous vessel or bladder, and the end of a second silk thread is placed in the molasses; the two ends of the silk threads being connected, the circuit is closed, and the current of vital fluid passes; its effect will become manifested on an organised being in the line of the current. If a certain number of these elements are gathered together in couples, a dozen for example, by plunging the thread from the ammonia of the first element in the molasses of the following element, and so on, or by connecting on the one side all the threads from the ammonia, and on the other all those from the molasses, a powerful current will be obtained, which produces on an animated being very considerable effects."

CORRESPONDENCE.

Transparency of Red-hot Metals.

To the Editor of the Chemical News.

Sir,—Every reader of your valuable journal would note with interest, in your last week's impression, a letter upon the above subject signed "A. Adriani," but very few, I think, would endorse the author's opinions. With regard to Father Secchi's discovery of a crack in the interior of an iron tube by (seeming) transparency, is it not more likely that the crack exhibited itself because where it existed the metal would be thinner than throughout the rest of the tube, and would therefore cool more rapidly, becoming in consequence darker coloured, thus showing on the exterior both its extent and direction?

I am, &c

W. F. K. Stock, F.C.S.

Laboratory, Darlington.

South Kensington Science.

To the Editor of the Chemical News.

Sir,—Can you or any one else tell me the meaning of *Science* in the South Kensington vocabulary, and why it should always be coupled with the word *Art* when it is spoken of west of Hyde Park Corner? What possible connection is there between the two? and what occult reason is there that the public should have the idea incessantly dinned into them that the same faculties which have given us steam, the telegraph, the soda trade, and aniline dyes, are so intimately connected with Majolica Ware and Old Masters that they must perpetually form a binary compound of "Science and Art," as inseparable as "Box and Cox," "Pyramus and Thisbe," or "Medes and Persians?"

I rather suspect the true interpretation is this. The practical English public don't much care for Art, but they *do* care for Science; so the latter is tacked on to the former to make it go down. If this is not the reason, and if *Science* is really valued at South Kensington, the officials there have shown a most unappreciative spirit in keeping it so much in the background as they did at the late inauguration of the Albert Hall of Science and Art.

I was one of the spectators there. I noticed numerous *Art* celebrities and official personages in seats of honour, but poor *Science* had to content itself in the crowd.

I am, &c., F.R.S.

[Our correspondent evidently had not a good "standpoint" in the crowd, or he would have seen two or three scientific men in reserved seats. We cannot say we feel the same indignation which our correspondent expresses at the non-recognition of science. English science requires no petting to enable it to flourish, and can exist in a healthy state without official patronage.—Ed. *C. N.*]

A Good Suggestion.

To the Editor of the Chemical News.

Sir,—Would you allow me, through the medium of your valuable journal, to make a suggestion? I would suggest that chemical students should learn some system of shorthand. This art is easily acquired. If the student would devote, say, an hour daily, or two hours a day three times a week, to learning and practising, he might attain such a degree of proficiency as would enable him in the course of three or six months to write at the rate of 80 to 90 words per minute. This rate would be sufficient for the purpose of taking notes at lectures, or, indeed, reporting most lectures verbatim. Shorthand writing will be found useful on many occasions—for instance, jotting down thoughts arising on the spur of the moment, and for making memoranda intended to be read by yourself only. Many other uses would suggest themselves for the employment of this art. Of course, to follow a very quick speaker and report his speech word for word, much time must be devoted to hard practice. I was able, after six months' practice, to write at the rate of 100 words per minute; many, however, find it impossible to go beyond 80 words.

The system I recommend is Pitman's "Phonography," which is written according to sound. Nothing, I am convinced, strengthens the memory so much as the use of shorthand. I have often attended lectures and taken down the greater part of them and the discussions arising, and have found, on coming to transcribe my note into longhand, that I could do so with but seldom looking at the shorthand notes.

I trust many of your readers may be incited to learn the art of shorthand. The books required are cheap, the time taken to learn shorthand, especially Pitman's, is short, and it can be done without a teacher. I think that by drawing attention to this subject much good may be done.

I am, &c. J. H. Swindells.

Edible Earths.

To the Editor of the Chemical News.

Sir,—I notice in a recent number of your journal a paragraph containing the analysis of a species of clay eaten by the natives of Borneo, and where it is asserted that "no other analyses of any substances used as such have been made, or at least published.

Perhaps you will allow me to state that Vanquelin formerly analysed the clay eaten by the Ottomacs (Humboldt, "Ansichten der Natur"), and that an analysis by Dr. Trail of an earth used as an aliment is given in one of my works ("Utilisation of Minute Life"), where the results of another analysis by Liebig are also alluded to. Finally, I beg to subjoin my own analysis made in 1865, of the earth which is extensively mixed with flour in Umea Lapmark in periods of scarcity:—

Water and organic matter	15·0
Silica	80·0
Sand	0·9
Alumina and oxide of iron	3·5
Carbonate of lime	0·6
	100·0

I am, &c. T. L. Phipson, Ph.D.

London, May 26.

Transparency of Red-hot Metals.

To the Editor of the Chemical News.

Sir,—In reference to the above subject, I may state that platinum, at least, appears to me transparent when it is at a strong white heat. If some carbonate of sodium is fused in a thin crucible, and then raised to a very high temperature, on shaking the crucible the motion of the fused salt may be discerned *through* the sides of the crucible.

I am, &c. W. B. G.

Liverpool, June 3.

[This is more probably due to the fact that the platinum cools more rapidly where not in contact with the fused salt than it does where the salt touches it.—Ed. C. N.]

To the Editor of the Chemical News.

Sir,—As to the transparency of some metals at bright red heat, workers in metals know it pretty well. Molten copper poured out of a ladle in a thick stream is as transparent as glass. I was acquainted many years ago, while at Utrecht University, with a gentleman residing there, who was the owner of an extensive establishment for rolling copper, etc.; the quantity of metal usually molten down at once exceeded ten tons in weight, and the workmen, among other tests about the proper degree of purity of the metal (*i. e.* freedom from suboxide), used to pour out 100 kilos. in a stream, the thickness of which was fully from 1 to 1·7 centimètre, and was as perfectly transparent as glass, with a slight bluish hue. Copper in bars heated for forging is the same; iron and platinum far more so; while gold 0·7 centimètre thick presents the same property; silver does not become transparent.

I am, &c. Dr. Adriani.

[We still think these alleged facts require verification by scientific men accustomed to exact observation.—Ed. *C. N.*]

Science and Art.

To the Editor of the Chemical News.

Sir,—Your correspondent "F.R.S." wants to know the

Brompton meaning of the wood "Science." A good many would like to know the exact sense in which "Art" is there used. "F.R.S." and the majority of the public appear to associate it only with painting, sculpture, etc. But from the mysterious addition of a final *s*. which has lately taken place (*e. g.* Albert Hall of Science and *Arts*), I suspect that the term is now intended to mean the useful and manufacturing arts. No one can deny that the word *Science* is properly associated with the *Art* of dyeing and calico-printing, the Society for the Encouragement of *Arts*, Manufactures, and Commerce, or the Dictionary of *Arts*, Manufactures, and Mines; but remove the final *s* in the last two instances, and the meaning becomes totally different.

Whatever may have been its original derivation and meaning, the word *Art* at present refers to two distinct functions. In one sense "Science and Art" is an incongruity, in the other sense it is a legitimate combination.

I am, &c. PUZZLED.

Mnemonic Nomenclature.

To the Editor of the CHEMICAL NEWS.

SIR,—Permit me to rectify a statement made by a correspondent on page 200, No. 285 of the CHEMICAL NEWS. Gmelin did not publish any new plan for a chemical nomenclature in the year 1827. At that time Laurent was still at school, and Dumas had not yet made the discoveries on which the doctrine of substitution was based. However, in the next "generation" (1848) a chapter on that subject appeared for the first time in the fourth edition of Gmelin's "Handbook of Chemistry." In the English translation of that work, this chapter, entitled "Suggestions for a New Chemical Nomenclature, particularly for Organic Compounds," occupies just four pages (vol. vii. pp. 149—153). The article, by its allusions to recently discovered compounds and new chemical reactions, bears internal evidence that it was written immediately prior to its publication in 1848. Long anterior to this date I had conceived the idea which was the germ of my own scheme. In 1850 I extended the system of counting so as to express the highest combinations. My first promulgation can be proved by the following statements by well-known scientists:—

"On consulting my diary I find that the first time you mentioned your new chemical nomenclature to me, in which you proposed to use vowels as numerals, and to distinguish the non-metallic elements by different consonant terminals, was on Saturday, May 13, 1848. . . . .

"This is no occasion on which to offer a criticism of your nomenclature, but I cannot refrain from adding my testimony to its wonderful adaptation to the simplest expression of the most complex chemical compounds.

"CHARLES A. JOY,
"Professor of Chemistry in Columbia College
and in the School of Mines, New York."

"For several years preceding 1846 Professor S. D. Tillman and I resided in the same town. I well remember his plan, then and there made, of a new chemical nomenclature, in which the name of a compound denoted its exact component parts. The number of equivalents was expressed by a vowel immediately preceding the last letter (m) in the names of all metals, as well as before the different consonants expressing non-metallic elements. . . . .

"EDWARD BAYARD, M.D.
"No. 6, West Fourteenth Street, New York."

The question of priority of conception will be deemed of minor importance by those who have carefully examined what has been done by Gmelin and by me. That Gmelin did not "fully elaborate" his scheme is evident from his own admissions. In the chapter alluded to (page 151, Watts's Translation, 1852) he says: "In working our the details of this nomenclature it would doubtless be found that many additions and corrections were necessary. . . . In a nomenclature for organic compounds something more is required than the names of elements and the expression of their numbers by vowels." This author gave new names to all the elements, yet failed to express any but the simplest facts of combination. After devising a very defective numerical system, in which the value of vowels depended on their position, he was compelled to coin new words for each nucleus having no reference to their composition, thus virtually abandoning the vowel plan. By scrutinising Gmelin's method any one who comprehends the requirements of a complete nomenclature will be convinced that the scheme cannot be used as a substitute for the notation. Similar objections may be made to Mansfield's method (see "Theory of Salts," London, 1865). This author shows that the use of the five vowels as numerals was of English origin, and was employed, although not in connexion with chemistry, in Dr. R. Gray's "Memoria Technica" (new edition, Oxford, 1831).

I claim for my scheme, which is adapted to Gerhardt's unitary system, and may properly be called "the unitary nomenclature," as its chief merit, simplicity of construction. Accepting the old names of the elements, I have so modified them that each begins with its old symbol, and ends with its new; the latter so generally correspond with the former that only seven new characters are introduced. With these materials and a more perfect numerical method than that employed by Gmelin, I have succeeded in representing, by a combination of letters and syllables, bodies of the most intricate structure. For the first time, systematic terms have been applied to radicals, so-called residues, and their numerous combinations. Every hydrocarbon has an appropriate symbolic appellation, and not only polymers but isomers have distinctive names. The only paper which I have yet published on the subject contains word-formulæ for more than *seven thousand compounds*.

Although the unitary nomenclature is commensurate with all possible atomic combinations, I do not claim that the new names would be preferable to a portion of the corresponding names now employed, nor do I entertain the hope that any part of the new scheme will be adopted until combinations of terms on the old plan become too cumbersome for common use.

I am, &c. SAMUEL D. TILLMAN.

American Institute, New York, May 11, 1867.

Manufacture of Sulphuric Acid.

To the Editor of the CHEMICAL NEWS.

SIR—In answer to the queries of "T. G. H., Lisbon," I beg to offer the following remarks:—

When pyrites is burnt, a considerable less quantity of sulphuric acid can be made with a given quantity of chamber space then when pure sulphur is employed, owing to the consumption of additional air in order to oxidise the iron, etc., in the pyrites; the nitrogen of the extra air thus employed occupies a portion of the chamber space, and thus renders the chamber relatively smaller. Also, to overcome the effect of the dilution of the gases, additional nitre must be employed. The quantity of sulphur which can be economically burnt with a given quantity of chamber space depends much on the mode adopted in working the chambers, on the size of condensing towers, etc., used. Usually there is a greater waste of sulphur when only one or two chambers are used than when several are worked together, arranged as "working" and "receiving" chambers. In Richardson and Watts's "Technological Dictionary," i. iii. p. 80, about 3000 cubic feet to 112 lbs. of sulphur per diem is given as an ordinary average quantity. This represents 1·672 cubic metre per kilogramme of sulphur per diem.

I am, however, acquainted with instances where a much larger proportion of sulphur has been regularly consumed than this. Thus, a single chamber, furnished with a coke condensing tower, burnt regularly 1 kilog. of sulphur (in the shape of pyrites, containing about 48 per cent. of sulphur) per diem to 1·047 cubic metre of chamber space; and at one time, when the manufacture was pushed to the utmost, only 0·726 cubic metres were allowed.

Several chambers worked together, on the English system, and furnished with good coke towers, averaged 1·080 cubic metre per kilog. of sulphur (as pyrites) per diem.

Allowing 1·200 cubic metre per kilogramme of pure sulphur per diem (representing about 2·1 kilos. of Huelva pyrites), a chamber of 29,000 cubic feet (English), or 821 cubic metres, should consume about 1437 kilogs. of Huelva pyrites daily. A kiln of the undermentioned dimensions will allow about 350 kilogs. or such pyrites per diem to be properly burnt. A larger quantity of pyrites may be passed through the kiln, but the burnt pyrites will frequently contain a considerable amount of unburnt sulphur. From 2 to 3 per cent. of sulphur is always left in the pyrites, so that four kilns of these dimensions would about suffice for the above chamber.

A convenient form of burner for Huelva pyrites is one measuring about 1·3 to 1·4 metre square at the level of the bottom of the charging door (*i. e.*, at the top of the mass of burning pyrites), and about 1·0 to 1·1 metre square at the level of the bars; between these levels a vertical distance of 0·6 to 0·7 metre is allowed. If deeper kilns be used, fluxing of the ore becomes a very probable occurrence. Larger or smaller kilns, with about the same relative areas, may be used.

A cast-iron pipe would, I think, be preferable to an earthenware one for conducting away the gases to the chamber, both on account of durability and the cooling of the gases. When pyrites is burnt, a thick sublimate is often formed in the pipe, materially diminishing its capacity. A tube of 0·5 metre internal diameter would suffice for the above kilns and chamber; but a larger one would be desirable, on account of this tendency to being blocked up.

A pipe of twenty-one metres length will hardly prevent fine dust of ferric oxide, etc., from being carried over into the chamber. For ordinary uses the presence of a little ferric sulphate, etc., in the acid produced is immaterial; but where concentrated acid is manufactured the presence of such bodies will often injuriously affect its sale. Arsenic is almost invariably present in acid made from pyrites. I am acquainted with an instance where the manufacture of sulphuric acid (rectified) from pyrites was given up, solely on account of the impossibility of selling the impure acid thus produced. It is, however, quite possible to obtain a colourless acid at 66° Baumé from Huelva pyrites.

Should "T. G. H." desire any further information, plans of burners, etc., I shall be happy to communicate with him.

I am, etc.,

Charles R. Wright, B.Sc.

St. Thomas's Hospital, Surrey, S., June 4.

Chemistry in Schools.

To the Editor of the Chemical News.

Sir,—Having now for three years made chemistry a regular and important part of my pupils' studies, I am able to speak most favorably of the general results of my experiment. At first I feared the boys would neglect their other work for the sake of this most fascinating subject, and that I should detect a falling off in their mathematics, or Latin, or history, or what not. My fear was entirely groundless. They have not learnt less of languages, but more of science. Boys who before had no idea of study for its own sake have been tempted to apply themselves, of their own accord, to chemistry, and, with the earnestness which this pursuit has put into them, have worked all the better at their other lessons. Of course they take more interest in the experiments than in the explanations. But even this characteristic of boys' nature has its good side. They are nor liable to value theories more than facts; and in course of time they learn to *ask* for explanations, and are not content till they get them. Chemistry teaches habits of careful observation, patience, caution, neat-handedness, and quickness. It stimulates ingenuity, and strengthens the faculty of generalisation. The other day I found that one of the boys had for months been colouring his maps with prussian blue of his own manufacture, having got it, by an obvious method, out of the red marl which abounds in this district. He dissolved the peroxide of iron with sulphuric acid, and precipitated his prussian blue with ferrocyanide of potassium. Without enlarging upon the topic of my letter further, I very strongly recommend those of my fellow-teachers who have not yet thoroughly tried the experiment, to introduce chemistry forthwith into their school-course; only it must be taught *well*—as accurately and as fully as the Latin or the mathematics. I am convinced that the most exact of the experimental sciences, lying at the basis of so many other departments of inquiry, must form an indispensable part of a judicious and philosophical scheme of education.

I am, etc., E. S.

Nottingham, June 6th, 1867.

The Tricks of Trade.

To the Editor of the Chemical News.

Sir,—Will you permit me to call public attention to a system which is undermining the chemical manufactures of the United Kingdom, and which, if not checked, will drive these important branches of industry, for which we possess such eminent natural facilities, more and more into foreign countries?

The evil to which I refer is that organized system of bribery and corruption prevailing in the sale of drugs and chemicals, such at least as are used in the cotton, woollen, and silk trades for dyeing, printing, bleaching, etc. Suppose, *e.g.*, that a traveller calls at a dyeworks and obtains an order. He immediately sends a message to the foreman who is to use the article, requesting an interview. The parties meet at some out-of-the-way public-house, and after a little general conversation over a glass of brandy, business begins. Says the traveller, "I've just seen your governor and got a trial order for a dozen bottles of 'double muriate,' and if you will give it a good character we will make it a snug thing for you." The foreman, putting on an air of complete indifference, asserts that he is very well satisfied with the article he has in use, but is, of course, not averse to anything really better. At length, after much bargaining and more brandy, it is agreed that, in consideration of a bribe of from 5s. to 10s. per carboy, the new article is to be reported better than the old. A couple of sovereigns are then handed to the foreman as a retaining fee, and the meeting terminates. It must be distinctly understood that if a traveller neglects thus to "make all right" with the foreman, his wares, how excellent soever, will, in most establishments, be condemned. If needful, a quantity of goods will be spoiled by some intentional neglect, and the blame will be laid on "that new lot of bottles."

If a traveller fails to obtain an order, he still seeks out the foreman, and, by dint of cash and promises, attempts to gain him over. "You are using Messrs. N. N's. wares? Well, you can say by opportunity that they have been falling off in quality, and that you have heard ours very highly spoken of. If you can get us in, you shall have so-and-so."

The amount thus given in bribery is enough to startle outsiders. I could point out a foreman dyer who demanded as the price of his good will 10s. per carboy on "scarlet finishing spirits," invoiced at 4d. per lb. As the carboys would average 130 lbs. net, this man's modest share of the plunder would amount to 23 per cent. On nitrate and nitro-sulphate of iron the "present quotations" are 1s. to 2s. 6d. per carboy, and on liquid ammonia 1s. to 2s. Solids and pastes, from obvious reasons, do not allow as much "tip" as liquids. Still the amount is in some cases considerable. On extract of indigo the fee averages 2s. 6d. per cwt. On cudbear and archil it is also heavy. I could name a foreman who received 14*l*. for using about 7½ tons of these articles. Other chemicals, such as oil of vitriol, soda-ash, and bleaching powder, are sold honestly when coming direct from the maker, though there are plenty of middlemen who sell these articles, of course duly adulterated, on the bribery system. I have never yet met with an authenticated case of "tip" being given on the aniline colours.

But to get in at an extensive dye or print works, some manufacturing chemists and drysalters will for a time go far beyond the tariff above given, even offering to hand over to the foreman who can manage the "job" the entire profits on all transactions for the first two or three months. A pattern-dyer, about to obtain a position in a first-rate establishment, holds a perfect *levee* of travellers and agents, and literally sells his future employer to the highest bidder. Nor is this all: the drysalter is expected to treat the foreman-dyer whenever they meet, to lend him a trap for an airing on Sundays, and to assist him if out of work or in trouble. Some firms give, yearly or quarterly, a dinner or supper to all foremen who patronise their wares. In one town there was established a so-called "dyers' club," ostensibly for discussing trade mysteries. It struck the public as suspicious that the traveller of one particular manufacturing chemist was unremitting in persuading dyers to attend the convivial meetings of the "club," and that this same chemist, or some representative of his, invariably occupied the chair on these occasions.

I must mention that, though foremen-dyers take a leading part in this system, yet the plunder is in many establishments shared by clerks, warehousemen, pattern-designers, and, in short, by any one who has contrived to gain a share of influence, and has opportunities of "ear-wigging" the master. Sometimes even one member of a firm is found accessible to bribery, and will, for so much per cask or per carboy, connive at the robbery of his partners and of himself.

Transactions of this nature sometimes come to light in an amusing manner. A young man who had represented an eminent drysaltery firm lately left them, and accepted a different employment in the same district. One day he received from his late principal a letter to this effect: — "We are forwarding a sample chest to Messrs. ———. As a large order is depending upon it, will you oblige by seeing their dyer, whom you know? Tell him that the chest is marked ———, and that if he will speak well of it we will make it a good thing for him. Please not to mention the matter to our present agent, as we are cutting the job too fine to allow him his commission." Another traveller offered the head dyer at a certain establishment 2s. 6d. per cwt. on a cask of extract of ———, for which he had just taken an order. The dyer, who had a share in the concern, at once mentioned this in the office; and when in due course the traveller called for the account, the amount, less 2s. 6d. per cwt., was paid him, with the intimation that any future visits would be useless.

Two makers, A. and B., had been in the habit of supplying "blue iron" to a certain dyeworks. A new head dyer who had been appointed declared himself unable to use A.'s, which he stated to be deficient in bloom. One day he sought up A., and said to him, "If you will give me 1s. 6d. per bottle, as B. does, I will give your blue iron a good character." A. not only refused, but told the man's employers, who, when B. called, informed him that they could only continue to do business with him on condition that he would supply them with the same quality of iron at 1s. 6d. per bottle less, to which, after a little bargaining, he consented.

The first result of the system is to encourage adulteration and other phases of commercial dishonesty. The object of the bribe-giver is to make the very poorest article which will pass muster. He therefore dilutes his liquid preparations with water as far as practicable, and, to keep up the specific gravity, adds matters more or less prejudicial. The weight of packages is often deficient. A dyer, bargaining with a traveller for an increase of "tip," has been known to say, "Empties are never tared at our shop, so you can write your tare three or four pounds lighter than it really is, keeping the gross right, and give me the price of what that will save you." The following ingenious scheme was practised for some time successfully by a manufacturing chemist: — The tops of his carboys were covered with several pounds of loose damp straw and rubbish, the tare being represented as lighter, and the net weight as heavier, than was the fact. The bottles being weighed, the gross was found correct, and they were passed on to the dye-house. In pouring out the contents of the bottles, this wet thatch fell off, and the empties, when returned to be tared, were found quite correct. A chemist has been known, among a dozen bottles of "red cotton spirits" and oxalo-chloride of tin, to send one or two bottles filled with water, and distinguished by a mark known to the dyer, who poured the contents down the sink and coolly fetched another bottle. The same dyer was in the habit of regularly wasting his mordants and colours in order that a fresh supply and a new bribe might be the result. He also, with the connivance of the gate-keeper, sent back bottles when only half empty. The chemist then filled them up and sold them afresh. By such means consumers are robbed of the money which is to corrupt their servants.

Another result of the system is the discouragement of invention and improvement. Success does not turn upon the question who can make preparations yielding at the cheapest rate the brightest, fastest, evenest, and solidest colours, but simply upon this—who can and will go farthest in the career of bribery and corruption?—a contest in which a Runge, a Hofmann, or a Perkin would inevitably be beaten by less scrupulous persons. You may make better mordants than ——, the old-established bribers. It is of no avail. The dyer will shake his head and say that he "can't get his colour" with your preparations. Nor is the case altered if you bring forward something entirely new. Your invention may save time and labour, and may give a better result, but the general question head dyers will ask is simply—How will it affect our perquisites? And unless this can be satisfactorily answered you will be "wet-blanketed" to your heart's content. Novelties like the aniline colours, of course, take the trade by storm; but many improvements, valuable if less striking, never get a fair trial. Thus invention is driven to seek elsewhere that scope which is denied at home.

Further, when a master dyer discovers how he is being robbed, he very naturally grows jealous, and, as far as possible, procures his requisites from foreign makers.

It may, perhaps, be objected that a dyer must get his colour, and, if so, that he cannot use really bad articles. I reply that a colour may be generally got in various ways, but that it is commercially essential to get it in the cheapest and quickest manner, and in the one which interferes least with the strength, durability, and suppleness of the fibre. There is also a wide margin between a colour which barely passes muster and one which is pronounced equal to the very best in the market. The bribe-loving dyer does not aim at excellence; his attention is sufficiently occupied with producing a passable result with the rubbish which his tempter puts in his hands.

The tinctorial trades of England are evidently not progressing as they ought. It is painful to see woollen mills running night and day to produce "sale-yarns"—*i. e.*, yarns which, instead of being dyed in this country, are exported uncoloured.

With your kind permission I will on a future occasion point out how the bribe-givers and bribe-takers may, to a great extent, be baffled. I am, etc., W.

[The statements in this letter are so well authenticated by the writer that we feel bound to give them insertion. At the same time we must express our conviction, in justice to many honourable firms, that such practices as above described are the exception rather than the rule.—Ed. C. N.]

Manufacture of the Bisulphide of Carbon.

To the Editor of the Chemical News.

Sir,—On looking over the article on the French Exhibition, in your last week's number, I was surprised to find that the writer had given M. Deiss, of Paris, the merit of being the first to introduce the manufacture of bisulphide of carbon on a large scale and at a low price for manufacturing purposes.

This is a mistake, and an injustice to myself, and for the sake of truth in the history of a new manufacture I beg to inform you that I took out my first patent in England, June 27, 1843, for the application of bisulphide of carbon as a solvent of india-rubber and other gums, and also as a solvent of phosphorus in connexion with some improvements in electro-metallurgy. Shortly before the date of this patent, all the sulphide of carbon I could procure either in England or Germany for my experiments was only six or seven ounces, costing sixty shillings per pound, and even for this I had to wait nearly six months. On applying to an eminent manufacturing chemist in London for help in the manufacture of bisulphide of carbon on a large scale, I was politely informed that I must be mad to expect that a hundredweight could be made, as the danger of preparing it would be fatal to any one making the attempt.

Being fully convinced of the very valuable properties of bisulphide of carbon for commercial purposes, I put up apparatus to prove that it could be produced in a large way and at a trifling cost, and completely succeeded.

In justice to a gentleman, Mr. Jesse Fisher, Madeley, Salop, whom I then employed to manufacture it on a large scale for Messrs. Elkington and Mason, who were interested in my patent, I would observe that under my instruction he erected the necessary apparatus, and in the year 1844 made many tons weight at a cost of one shilling per pound, and very soon afterwards he was able to produce it at threepence per pound; and that gentleman has continued the manufacture in England ever since. My object in furnishing this information is to show that, long before M. Deiss gave his attention to the subject, I had succeeded in making a large commercial manufacture of bisulphide of carbon, and I was also the first to introduce it for manufacturing purposes. And, in fact, before M. Deiss commenced his operations in the manufacture of this article it had been made by the ton at threepence per pound, and it was not used at all in France until the introduction of my patent for improvements in treating india-rubber in that country; from which you will see that it was in the year 1844 that I made it a success, and not, as stated in the article in the Chemical News, 1848.

I am, etc., Alexander Parkes.

Birmingham, May 28.

Manufacture of Sulphuric Acid.

To the Editor of the Chemical News.

Sir,—In No. 392 of your journal I notice a letter on this subject. Among other matters the writer states that "it is quite possible to obtain a colourless acid at 66° Beaumé from Huelva pyrites." I was not aware that there was any difficulty in preparing an acid, colourless and pretty pure, from this class of pyrites. With regard to the fixing of the pyrites, I can say that carelessness has as much to do with this irregularity as wrong construction of the burners. A want of proper attention on the part of the burner-men in breaking up the sulphur ore, and managing a regular and sufficient supply of air, is a most frequent cause of fluxing; in ninety-nine cases out of a hundred this is the sole cause. Then we have the complaint of the small quantity of acid got from chambers which have been constructed to produce a far larger amount than is ever obtained from them; in many cases the cause of this proceeds from the too slow combustion of the sulphur ore; a greater part of the pyrites may be "dead out" or only half ignited, generally proceeding from the want of a proper supply of air.

Again, the steam may not be admitted into the chamber in a satisfactory way. This matter is much overlooked. Some makers admit the steam at stated intervals. Now this is wrong; the admittance of steam should be continuous and at a proper pressure. We often hear of the acid doing serious damage to the platinum stills, and the cause is often overlooked for a length of time; this is more particularly the case where the proprietor has no scientific supervision. At last it is discovered that the cause of the damage proceeds from the presence of oxide of nitrogen in the acid. It is well known that acid so contaminated has the property of attacking platinum. The most important part of the process is frequently left in the hands of the burner-men, and left entirely to them as to how the manufacture shall be conducted. In some cases this plan may work well, when the men happen to be intelligent; but in many cases the men are lamentably ignorant. This mostly arises from the fact that no trouble is taken by the manager or chemist to explain to the men in simple language the various processes connected with their work. This is seldom the case; the men go mechanically about their work, and so long as things happen to go right no further interest is taken, but let anything out of their ordinary course occur, and the men are at sea.

However unimportant what I have said may appear, I have always found it to my interest to have the men as well informed as possible. Carelessness and inattention to trivial matters often lead to the downfall of many operations in manufacturing chemistry.

In your "Suggestion to Manufacturers and Inventors" you make a very good remark respecting the difficulties which often beset the manufacturer, and which might easily be overcome by a short investigation by some competent chemist, "and in many cases a difficulty which appears insuperable to the manufacturer would prove a mere bagatelle to the chemist." Your ideas coincide with mine, but I know many manufacturers who object to a scientific man having access to their works. I can say, however, that such supervision is much needed by them.

I am, etc. J. H. Swindells.

Cure for Dry Rot in Houses.

To the Editor of the Chemical News.

Sir,—It may be of interest to many of your readers, whose houses are infested with dry rot, to learn that a perfect cure, or rather a way of preventing it, has been found out in the alkali works at Saaran, in Silesia. The dry rot is caused by the spores of a fungus (*Merulius lacrymans*), which may be carried away by currents of air, but are mostly contained in the soil. If any soil containing them be used for filling the spaces between the joists and boarding of a chamber-floor, the appearance of dry rot in the house need not create any surprise. A little of such earthy matter may easily be mixed among the sand or slag, if such is used for filling those spaces, or even among the sand which forms part of the mortar. Of course dry rot will not set in if the timber is kept completely dry; but it is well known that this condition is often not easily achieved. A radical remedy against dry rot is, however, found, if the space underneath the flooring boards be filled with a mass capable of destroying vegetable life without doing any injury to the timber or brickwork. No doubt many such compositions might be found, but the builders would be deterred by any considerable expense from using them. Now, on the Saaran alkali works it has been observed (as the manager, Mr. Junker, reports in the *Breslauer Gewerbeblatt*) that tank-waste, that great nuisance of alkali works, is an excellent remedy against dry rot. If some of it is used along with the rubbish for filling the spaces between the joists, and if the whole is beaten down into a mass, it hardens very well, like ordinary tank-waste floors, and it never allows any dry rot to spring up. It makes no difference whether the air has access or not; there is also no smell whatever exhaled. Besides many other striking illustrations of the efficacy of tank-waste in this respect, Mr. Junker mentions a case where two adjoining souterrains had been boarded, the one with, the other (by mistake) without, the application of tank-waste. The latter room was at once infested with dry rot; the former remained quite sound. The boards of the infected room were then taken up again, and tank-waste was applied; since that time (two years and a half) no dry rot whatever has set in, and the decay of the boards, half-rotten as these had already become, was stopped altogether.

None of your readers need be told that tank-waste may be had at all alkali works for the carting away.

I am, etc. G. Lunge.

Test for Cobalt.

To the Editor of the Chemical News.

Sir,—Having tried some experiments on the new test for cobalt (viz., the production of a deep red coloration on the addition of tartaric acid, excess of ammonia, and ferricyanide of potassium), I found that not only was the coloration produced when tartaric or oxalic acid had been added, but with any other acid (as many as I tried) or salt of ammonia, provided that the acid (or salt), ammonia, and ferricyanide were first mixed and then added to the solution of cobalt.

For instance, the coloration is developed quite as well by the addition of a mixture of chloride of ammonium, ammonia, and ferricyanide, as in the method by tartaric acid. The acids tried were hydrochloric, nitric, sulphuric, carbonic, chromic, and acetic. Probably these experiments have been already tried, but, not having seen them mentioned, I thought it best to place them on record.

I am, etc. Tyro.

Tricks of Trade.

To the Editor of the Chemical News.

Sir,—Drugs and chemicals are not the only things that absorb so much "bribing;" here is a sample of one style, to hand this morning:—

"Messrs. G——— and Co., D**** Street.—The *employés* in the above firm most respectfully beg to intimate that they intend celebrating their second annual dinner on Saturday, July 27, 1867. The favour of your subscription in aid of the above will be most thankfully received. Yours respectfully, for the *employés*, A. D**. N.B.—Post-Office Orders to be made payable at Gray's Inn Road."

Persistent neglect of all such dishonesty has lost the writer many a customer's trade. But what are we to expect when masters are so ignorant of their own businesses as is generally the case? Having no judgment of their own, nor capability of testing for themselves and proving, they are afraid to unsettle this foreman or that workman. I shall be glad to hear your correspondent's solution of the problem, *how* to neutralise it; but when competition is strong, British honour is a myth.

I am, etc. T. C.

Manufacture of Bisulphide of Carbon.

To the Editor of the Chemical News.

Sir,—Mr. Parkes errs in conveying the impression that my apparatus for the manufacture of bisulphide of carbon was erected under his instructions. The only assistance he rendered me was in directing my attention to the article in Mitscherlich's "Practical and Experimental Chemistry," translated by Dr. Hammock in 1838 (pp. 209–12), which upon examination, you will find, was not calculated to aid me much; and Mr. Parkes' first visit to my works was in 1849, some years after I had been engaged in the manufacture.

I am, etc. Jesse Fisher.

Chemical Works, Ironbridge, Salop, June 24.

CHEMICAL NOTICES FROM FOREIGN SOURCES.

Colours from Coal-tar. (C=6).—De Laire, Girard, and Chapoteaut. If, in preparing violaniline (3 molecules of aniline conjoined with elimination of H_6), aniline containing toluidine be used, the product is contaminated with a small quantity of rosaniline, and a larger proportion of mauvaniline—

$$\left.\begin{matrix} C_{12}H_4,H \\ C_{12}H_4,H \\ C_{14}H_4,H \end{matrix}\right\} N_3,HO,$$

a base possessing remarkable tinctorial properties. Its formula is substantiated—1st. By its formation by appropriate oxidation of a mixture of aniline and toluidine containing a greater proportion of the former. 2nd. By simultanous formation of water. 3rd. By the replacement in its molecule of H_3 by methyl, ethyl, phenyl. 4th. By its decomposition on distillation into the primary and secondary monamines of the phenylic and toluylic radicals. It is derived from two molecules of aniline and one molecule of toluidine, H_6 being eliminated; it is crystallisable; retains HO even after several hours' exposure to 120—130°, but loses it at a higher temperature; it is soluble in ether, benzol, and alcohol; insoluble in cold, sparingly so in boiling water; it forms crystallisable salts with acids; the acetate and chlorhydrate are especially fine bodies; the salts are slightly soluble in cold water; rather soluble in boiling water. Triphenylomauvaniline is obtained by acting upon mauvaniline with aniline; it is a crystallisable base, soluble in ether and alcohol, insoluble in water; its salts form splendid blue dyes.

Violaniline, mauvaniline, rosaniline, and chrysotoluidine (derived from the conjunction of three molecules of toluidine, H_6 being eliminated) are the first four consecutive terms of a series having a common difference C_2H_2.—*Comptes R.* lxiv. 416.)

Retene: its Constitution (C= 6).—M. Berthelot. Retene $C_{36}H_{18}$ is distinguishable from naphthalene, which it resembles in aspect by its having no odour, by its lesser solubility in alcohol, and by its melting at 95°; it boils at at about 400°; the vapour, mixed with hydrogen and passed through a red-hot tube, decomposes, producing large quantities of anthracene, carbon, acetylene, and some gaseous hydrocarbons, one of which is absorbed by monohydrated sulphuric acid. Yielding, under these conditions, anthracene, from which it differs by $4C_2H_2$, it cannot be derived from benzol, although its formula is triple that of benzol. but it might be derived from benzol and a hydrocarbon such as ethylene or formene, capable of supplying the acetylene necessary for the production of anthracene, its lower homologue. Attempts to obtain it from cumol in a reaction similar to that which gives anthracene from toluol have not been successful. The first members of the homologous series commencing with anthracene, and including retene, will probably be found among the solid hydrocarbons which crystallise immediately after naphthalene in the heavy oil of coal-tar.—(*Bull. Soc. Chim. Paris*, 1867, 231.)

Xylol, Chloro derivatives of (C=12).—C. Lauth and E. Grimaux. (Xylol distilling entirely between 137° and 139°) vapour and chlorine gave a liquid from which a fraction collected between 190—195° precipitated cold argentic nitrate solution: boiled with argentic acetate, a liquid of agreeable odour, probably tolylic acetate was formed; boiled with plumbic nitrate solution, a liquid having an odour resembling bitter almond oil, combining with sodic disulphite, and boiling at 200°, was produced, evidently C_8H_8O tolylic aldehyde. Therefore, in the first-mentioned reaction a change occurs analogous to that by which benzylic chloride is produced from toluol, and the chlorxylol obtained should be represented by

$$C_6H_4\left\{\begin{matrix} CH_2Cl \\ CH_3 \end{matrix}\right.$$

The fractions distilling between 230—260° contained $C_8H_8Cl_2$, a crystalline body, but in smaller quantity than was necessary for the study of its reactions; it melts at 100°. It is proposed to extend this reaction to trimethylphenyl, cumol from coal-tar, the alcohol $C_9H_{12}O$ being at present unknown.—(*Bull. Soc. Chim. Paris*, 1867, 233.)

Benzolsulphurous Acid (C = 12).—R. Otto and H Ostrop. Benzolsulphurous chloride, $C_6H_5ClSO_2$, most carefully dried, was diluted with ether perfectly free from alcohol and water; sodium amalgam was added by small portions till a sample of the fluid, after expulsion of the ether, dissolved in water; the ether was then expelled, and a small quantity of water was added; the aqueous solution was separated from an oily body and mixed with chlorhydric acid; the crystals which formed were recrystallised from hot water, and about two-thirds the theoretical quantity of benzolsulphurous acid was obtained. Its properties were the same as described by its discoverer, Kalle, who made it from zincic ethide and benzolsulphurous chloride. With fuming nitric acid benzolsulphurous acid gave $C_{18}H_{16}N_2S_3O_6$, for the unravelling of the constitution of which material was wanting; nitrobenzolsulphuric acid $C_6H_5(NO_2)SO_3$, was also formed. With bromine, benzolsulphurous bromide was produced, C_6H_5,SO_2,Br, and not brombenzolsulphurous acid, C_6H_4Br,SO_2,H, for ammonia gives with it an amide, and not ammonic brombenzolsulphite. With phosphoric pentachloride, benzolsulphurous chloride, and a body probably $\Theta_{12}H_{10}S_2O_3$, separated from the former by dilute solution of potash, were obtained. The oily body formed in the first reaction, and toluol- and xylol-sulphurous acids, will form the subjects of future communications.—(*Ann. Chem. Pharm.* cxli. 365.)

Cyanin (C = 12).—Dr. G. Nadler and Dr. V. Merz. The cyanin, or chinoline blue, which forms the subject of this paper, was prepared from tolerably pure chinoline, and was, therefore, an amylchinoline derivative. Chinoline blue, or iodocyanin, $C_{28}H_{35}N_2I$, is almost insoluble in ether and cold water, rather soluble in hot water, sparingly so in cold alcohol, and very soluble in hot alcohol; the green crystals melt and lose water at 100°; their solution in alcoholic solution of nitric acid is precipitated by argentic nitrate. Iodocyanin is capable of uniting with one or two molecules of acids; its solution in chlorhydric acid, if allowed to evaporate spontaneously in the presence of lime, deposits colourless crystals, $C_{28}H_{35}NI_2H2Cl$, which lose HCl at 90—100°. A warm alcoholic solution of iodocyanin digested with fresh argentic oxide gave cyanin $C_{28}H_{36}N_2O$. A chlorhydric acid solution of cyanin precipitated by ammonia, or an alcoholic solution of iodocyanin digested with argentic chloride, gives chlorocyanin, $C_{28}H_{35}N_2Cl$, easily soluble in alcohol and hot water, sparingly so in ether and cold water, combining with acids to form colourless compounds. Nitrato-cyanin was prepared by precipitating an alcoholic solution of iodocyanin, acidulated with nitric acid, with argentic nitrate; it is slightly soluble in ether and cold water, easily so in alcohol; its formula is

$$\left.\begin{matrix}C_{28}H_{35}N_2\\ NO_2\end{matrix}\right\}O;$$

it combines with acids to form colourless bodies; treated with ammonic sulphide at 100°, $C_{56}H_{68}N_4S_2O_2$ was obtained. Iodocyanin, heated with concentrated sulphuric acid, gave sulphatocyanin—

$$\left.\begin{matrix}2C_{28}H_{35}N_2\\ SO_2\end{matrix}\right\}O_2,$$

which, air-dried, is a light brown body, which decomposes, without melting, at 120°. From it other similar bodies, such as oxalocyanin.

$$\left.\begin{matrix}2C_{28}H_{35}N_2\\ C_2O_2\end{matrix}\right\}O_2,$$

are obtainable.

It appears from the above that cyanin is capable of forming with acids three kinds of bodies—the monacid bodies are intensely coloured, the triacid compounds are colourless and easily decomposed, giving rise to diacid compounds. All the coloured compounds are unstable in sunlight.—(*Journ. prakt. Chem.* c. 129.)

Alizarin.—C. Koechlin. The colouring matter from madder, after sublimation or exposure to 280°, no longer gives the tints sought by dyers: a yellow tint is wanting, in searching for which the author is engaged with Schützenberger. —(*Bull. Soc. Chim. Paris*, 1867, 235.)

Valerylene, Polymers of (C = 12).—E. Reboul. Concentrated sulphuric acid acts violently on valerylene; no conjugated acid is formed. A fraction of the product between 175—177° was divalerylenic hydrate, $2C_5H_8,H_2O$. The fraction between 265—275°, sp. gr. 0·862, at 15°, was trivalerylene, $(C_5H_8)_3$. The distillate between 280—350°, in which interval the thermometer rises continuously, as also the residue, which, on cooling, solidified to a semi-transparent mass, is a mixture of polymers of different degrees of condensation. Sulphuric acid diluted with one or one-third volume of water produces the same results, but more slowly, and consequently with less violence.—(*Comptes R.* lxiv. 419.)

Oxalohydroxamic Acid (C=12).—H. Lossen. This is one of two acid bodies formed by the action of hydroxylamine on ethylic oxalate; it is very difficultly soluble in cold water, but crystallises from boiling water; it explodes at 105°; dried over sulphuric acid it contains $C_2H_4N_2O_4$, which formula, however, may need to be tripled. Its salts are generally insoluble, or sparingly soluble, in water, and explode if heated to 130° or 180°, or if put into concentrated sulphuric acid. The constitution of the acid is probably expressed by

$$\left.\begin{matrix}C_2O_2\\ (HO)_2\\ H_2\end{matrix}\right\}N_2$$

(*Zeitschr. Chem.*, N. F., iii. 129.)

Sarcolactic Acid.—O. Schultzen. The urine of men or animals that have been poisoned by phosphorus, when the poisoning has reached the stage of colouring the skin, contains large quantities of this acid. To extract it the urine was evaporated to syrupy consistence, the alcoholic extract of the syrup was evaporated, the residue was treated with sulphuric acid and ether, the ether was decanted, and the substance left on its evaporation was purified by a small quantity of plumbic acetate freed from lead by sulphydric acid, and from acetic acid by evaporation; sarcolactic acid remained, which was identified by analysis of the zincic, cupric, and calcic salts. (*Zeitschr. Chem.* N. F., iii. 138.)

Bichlorsulphobenzide (C = 12).—R. Otto. This body—

$$\left.\begin{matrix}C_6H_4Cl\\ C_6H_4Cl\end{matrix}\right\}SO_2$$

is easily obtained by the action of chlorobenzol on sulphuric anhydride. It is insoluble in water, and crystallises from hot alcohol in long white needles, melting at 140—141°.—(*Zeitschr. Chem.*, N. F., iii. 143.)

Chlorbenzolsulphuric Acid (C=12).—R. Otto. This acid is a by-product in the preparation or bichlorsulphobenzide. It is obtainable in long white asbestoslike deliquescent needles, fusible in the water oven, easily soluble in water and alcohol, insoluble in ether and benzol. It is monobasic—

$$\left.\begin{matrix}C_6H_4Cl\\ SO_2\\ H\end{matrix}\right\}O.$$

Its salts are mostly soluble in water, and may be heated to 200° without decomposition. The sodic, potassic, calcic, baric, plumbic, cupric, and ethylic salts are described.

The chloride

$$\left.\begin{matrix}C_6H_4ClSO_2\\ Cl\end{matrix}\right\}$$

is easily obtained by acting upon sodic chlorbenzolsulphate with phosphoric pentachloride. It is soluble in ether, benzol, and alcohol; insoluble in water. It forms magnificent four-sided rhombic tables, often of considerable dimensions. It melts at 50—51°. With alcoholic solution of ammonia, sulphochlorbenzolamide is formed,

$$\left.\begin{matrix}C_6H_4ClSO_2\\ H_2\end{matrix}\right\}N,$$

crystallising from water in four-sided rhombic pillars of large size. It melts at 143—144°. By acting on chlorbenzolsulphuric chloride with zinc and sulphuric acid, chlorphenylphydrate is obtained,

$$\left.\begin{matrix}C_6H_4Cl\\ H\end{matrix}\right\}S,$$

which forms superb crystals, melting at 53—54°, distilling undecomposed, insoluble in water, soluble in ether and alcohol. It combines with mercuric chloride to form a compound insoluble in boiling water or alcohol, and gives with nitric acid chlorphenylic sulphide,

$$\left.\begin{matrix} C_6H_4Cl \\ C_6H_4Cl \end{matrix}\right\} S_2,$$

insoluble in water, easily soluble in hot alcohol, ether, or benzol, melting at 71°, and distilling undecomposed.

Chlorbenzolsulphurous acid results from chlorbenzolsulphuric chloride under the influence of sodium amalgam. It crystallises in small rhombic needles, easily soluble in hot water and alcohol, melting at 88—90°. The sodic, calcic, plumbic, and ethylic salts are described. Its formula is

$$\left.\begin{matrix} C_6H_4Cl \\ SO \\ H \end{matrix}\right\} O.$$

—(*Zeitschr. Chem.*, N. F., iii. 144.)

Organic Acids, a New Series of (C=12).—P. Greiss. Diazobenzoic perbromide (*Ann. Chem. Pharm.* cxxxv. 121), in solution of ammonia, decomposes with formation of $C_7H_5N_3O_2$, easily soluble in alcohol and ether, difficultly so in boiling, insoluble in cold water, melting at about 160°; it forms definite salts. The argentic salt is a white amorphous precipitate, $C_7H_4AgN_3O_2$. Analogous acids are formed by the action of ammonia on diazodracylic and diazosalylic perbromides. Diazoanissic and diazobippuric perbromides react in a similar manner. The reaction is therefore general. —(*Zeitschr. Chem.*, N. F., iii. 164.)

Ethyl- and Diethyl-benzol, products of Oxidation of (C=12.)—R. Fittig and J. König. Ethylbenzol is easily oxidised by dilute nitric acid, benzoic acid with traces of a nitro-acid being produced. Monobromethylbenzol gave by oxidation bromdracylic acid, identical with that obtained by Hubner and Philipp from bromtoluol. Diethylbenzol, treated with potassic dichromate and sulphuric acid, gave terephthalic acid, but, if oxidised with dilute nitric acid, an acid isomeric with xylylic acid is produced, and which may be called ethylbenzoic acid,

$$C_6H_4\left\{\begin{matrix} CO_2H \\ C_2H_5. \end{matrix}\right.$$

It crystallises from water or alcohol, and melts at about 110°. The baric, calcic, argentic, and cupric salts were analysed. This acid differs from all others of similar constitution. It may be regarded as bromdracylic acid, in which Br is replaced by ethyl; and it will perhaps be identical with an acid which Kekulé hopes to obtain by the action of bromethylbenzol on sodium and carbonic dioxide. The production of bromdracylic acid above mentioned proves that in bromethylbenzol the same H atom in the benzol residue is replaced by bromine, as is the case in bromtoluol.—(*Zeitschr. Chem.*, N. F., iii. 167.)

Opium, Alkaloids of: their Separation.—M. Kubly. The substance to be examined is extracted with benzol; narcotine, papaverine, thebaine, and codeine, are dissolved. Amylic alcohol dissolves morphine from the residue, and alcohol will extract narceine, if present in what is left undissolved by the two former solvents. To separate the four alkaloids soluble in benzol, cold amylic alcohol is used, which dissolves codeine; very dilute acetic acid will then extract papaverine and thebaine from the narcotine which remains. Finally, thebaine is precipitated from the sulphuric acid solution of papaverine and thebaine by bismuthic iodide in potassic iodide solution, papaverine remaining in solution.—(*Pharm. Journ. Russ.* Nov. 1866, 457.)

Cleaning Glass.—A method of cleaning glass, which may be useful where other methods fail, is given in the appendix to the second edition of Major Russell's "Tannin Process," published by Robert Hardwicke, Piccadilly. Dilute the ordinary hydrofluoric acid sold in gutta-percha bottles, with four or five parts of water, drop it on a cotton rubber (not on the glass), and rub well over, afterwards washing till the acid is removed. The action is the same as that of sulphuric acid when used for cleaning copper; a little of the glass is dissolved off and a fresh surface exposed. The solution of the acid in water does not leave a dead surface on the glass, as the vapour would; if a strong solution is left long enough to produce a visible depression, the part affected will be quite bright. This method is recommended in some cases for cleaning photographic plates, but we should think it might also be useful in cleaning the insides of bottles, flasks, etc., which have got stained through use.

A Quinine Famine.—The following is an extract from a letter from Mr. Colville Barclay, President of Poor Relief Committee of the Island of Mauritius: "The mortality from the effects of fever just now is from 180 to 200 every day throughout the island. All the quinine in the place is finished, and half a dozen bottles received from Reunion last week were sold at 135 dollars the ounce (27*l.* 10*s.*). The doctors are using arsenic as a substitute. The Government has sent home for 30 lbs. of quinine, as well as to Ceylon and the Cape for all that can be obtained."

Cantharidin.—Professor Dragendorff has found in cantharides a volatile body which acts on the organism in a similar manner to cantharidin. Freshly powdered cantharides are moistened with water and distilled; the portion going over below and at 100° C., which has an acid reaction, contains the new body.—(*Pharm. Zeitschr. f. Russl.*, Jan. 1867, i.)

Borates.—F. P. le Roux. Equal equivalents of calcined magnesia and boric anhydride, heated to whiteness, melt readily together, forming a slightly green, strong, and light glass. Rapid cooling is necessary to obtain it amorphous and transparent. Three equivalents of boric anhydride and one equivalent of suboxide of copper, melted together, and poured on an iron plate, form a glass, the surface of which has a different colour from the interior.

Other borates behave in a similar manner; most of them form glasses of different colours, according to whether, after melting, they are cooled slowly or rapidly.—(*Comptes R.* lxiv. 26.)

Electrolysis of Alkalic Sulphides.—H. Buff. In an electrolyte under the action of an electric current, elements, or groups of elements, travel in opposite directions, carrying an equal amount of electricity of opposite nature; they are electric-equivalent. Constituents not taking part in this locomotion do not contribute to the conduction of the electric current.

From his experiments with alkalic sulphides, the author concludes that the decomposition of the various mono- or poly-sulphides of potassium or sodium always takes place in this manner, that the metals travel towards the one electrode and all the sulphur towards the other, or that a group of, for instance, five atoms of potassic pentasulphide is electric-equivalent to one atom of potassic monosulphide. —(*Ann. Chem. Pharm.* Supplem. iv. 257.)

Niobium and Tantalum, Chloro- and Chloro-oxygen Compounds of.—H. Sainte-Claire Deville and L. Troost (O=16). The vapour densities of the volatile niobic chlorides and oxychlorides agree with Marignac's formulæ $NbCl_5$ and $NbOCl_3$. The presence of oxygen in the oxychloride may also be proved synthetically. If $NbCl_5$ (melting at 194° C., boiling at 240° C.) in a state of vapour is repeatedly driven over red-hot niobic anhydride (in a current of carbonic anhydride), it is converted into a white non-fusible body, volatile at about 400° C., having all the properties of the oxychloride.

$TaCl_5$ and tantalic anhydride, under the same conditions, do not act upon each other. The chloride obtained from pure tantalic anhydride, prepared according to Marignac's method, is crystallisable, melts at 211·3° C., and boils at 241·6°. It has a slight yellow colour, and decomposes slowly when exposed to air. The density of its vapour was found to be 12·8; theory requires 12·5 (Ta = 182).—(*Comptes R.* lxiv. 294.)

Rosaniline, Derivatives of.—H. Schiff (C = 12). Sulphite of rosaniline, dissolved in aqueous sulphurous acid (a yellow solution, containing leucaniline salt and polysulphite of rosaniline), shaken with a few drops of an aldehyde, evolves sulphurous acid, and a violet crystalline precipitate is gradually formed. Rosaniline salts treated in this manner with oil of bitter almonds, œnanthol, or valeric aldehyde, gives rise to the formation of the new bases

$$N_3 \left\{ \begin{array}{l} C_{20}H_{16} \\ C\ H_6 \\ H \end{array} \right. \qquad N_3 \left\{ \begin{array}{l} C_{20}H_{16} \\ C_7H_{14} \\ H \end{array} \right. \qquad N_3 \left\{ \begin{array}{l} C_{20}H_{16} \\ C_5H_{10} \\ H \end{array} \right.$$

Benzyliden-rosaniline. Œnanthyliden-rosaniline. Valeryliden-rosaniline.

The salts of these bases contain one equivalent of acid. The typical hydrogen may be replaced by ethyl. They are insoluble in ether, water, or dilute acids; soluble in alcohol, with a violet blue colour.

The author believes that the blue and violet dyes obtained by the action of the bromides of terebene and ethylene upon rosaniline have a similar constitution to the bases described. —(*Comptes R.*, lxiv. 182.)

Triamidophenol and Amidodiimidophenol.—C. Heintzel (C=12). The product of the reduction of picric acid with tin and chlorhydric acid the author finds to be hydrochlorate of triamidophenol, protochloride of tin $C_6H_2(HO)(NH_2)_3(HCl)_3(SnCl_2)$, and not, as stated by Beilstein, $C_6H_3(NH_2)_3(HCl)_3(SnCl_2)$, hydrochlorate of picrammonium, protochloride of tin. The body is soluble in water, alcohol, and ether, and is obtained in white crystals by precipitating its saturated aqueous solution with chlorhydric acid. Sulphuretted hydrogen decomposes it, and forms hydrochlorate of triamidophenol, $C_6H_2(HO)(NH_2)_3(HCl)_3$, easily soluble in water, and obtained from its solution in white crystals on adding chlorhydric acid. It is less soluble in alcohol and ether.

Hydriodic acid reduces picric acid to hydriodate of triamidophenol $C_6H_2(HO)(NH_2)_3(HI)_3$, and not, as Lautemann stated, to hydriodate of triamidobenzol, or, as he called it, picrammonic iodide, $C_6H_3(NH_2)_3(HI)_3$.

Triamidophenol cannot be separated from its salts without decomposition.

Ferric chloride acting on the hydrochlorate produces the hydrochlorate of amidodiimidophenol,

$$C_6H_2(HO)(NH_2)(N_2H_2).HCl.$$

It dissolves easily in water, with a beautiful blue colour, is sparingly soluble in alcohol, insoluble in ether, and crystallises in dark yellow needles. The base cannot be separated from its salts without decomposition. Digestion with dilute chlorhydric or sulphuric acid converts this body into the hydrochlorate of a new base, the probable formula of which is $C_6H_2(HO)(NH_2)(NH)(HO).HCl$. That is hydrochlorate of amidoimidohydroxylphenol, soluble in water and alcohol.

Another new base is obtained from hydrochlorate of amidodiimidophenol on reduction with tin or zinc and chlorhydric acid of its aqueous, or, with sodium amalgam, of its acid solution. It can be isolated from its salts by means of an alkali. It crystallises in white needles, dissolves readily in water, sparingly in alcohol.

The author here corrects his former statement, published in a preliminary communication (*Zeitschr. Chem.* 1866, 211), that by this reduction the hydrochlorate of triamidophenol was regenerated.—(*Journ. prakt. Chem.* c. 193.)

Methods of Reduction, new Application of.—Berthelot (C=12). Bromide of ethylene with water and potassic iodide, heated in sealed tubes for ten hours, is first converted into iodide of ethylene, and this is decomposed again according to the following equation:—

$$7\,C_2H_4I_2 + 4\,H_2O = 6\,C_2H_6 + 2\,CO_2 + 7\,I_2.$$

The reaction can only be explained by the supposition of a temporary formation of hydriodic acid. This caused the author to investigate the action of a strong aqueous solution of this acid on various substances at a high temperature (275° C.) in sealed tubes. Under these conditions hydrocarbons of the marsh-gas series were obtained in most cases.

Marsh gas was produced in this way from methylic formate, besides carbonic oxide and water, resulting from the decomposition of the formic acid.

Ethylic hydride resulted from the reduction of chloride, bromide, and iodide of ethylene, bihydriodate of acetylene, ethylic iodide, carbonic sesquichloride, ethylene, acetylene, alcohol, aldehyde, acetic and tartronic acid. The last-named body also gave rise to the formation of carbonic anhydride, evidently in consequence of its having split up originally into carbonic anhydride and acetic acid, the latter only giving ethylic hydride.

Propylic hydride was obtained from allylic iodide, glycerin, and acetone.

Butylic hydride from butyric acid and succinic acid. The gaseous hydrocarbons in most cases contained an admixture of hydrogen, derived, no doubt, from spontaneous decomposition of the hydriodic acid used in the experiments. From oxalic acid no carburetted hydrogen could be obtained, only carbonic oxide and carbonic acid, from formic acid carbonic oxide only (*Bull. Soc. Chem.* 7, 53).

Benzol and Phenol, Sulpho-derivatives of, (C=12).—A. Kekulé. Vogt's benzylmercaptan may be prepared by substituting sulphur for oxygen in phenol. Phosphoric sulphide and phenol are heated together in a retort, and the mixture distilled. The principal product of the reaction is thiophenol C_6H_5SH, identical with the benzylmercaptan of Vogt, obtained by the action of zinc and sulphuric acid on benzolsulphochloride. The other bodies formed are benzol, sulphide of benzol, and liquids of a high boiling point, probably phenylethers of sulphophosphoric acid. The sulphide of benzol thus obtained has been found identical with the sulphide obtained by Stenhouse from sodic benzolsulphate. The substance produced by oxidising sulphide of benzol, and called by this chemist sulphobenzolen, the author declares to be identical with sulphoxide of benzol.

Benzylmercaptan may be converted into sulphide of benzol by exposing one of its metal compounds to dry distillation. The decomposition takes place according to the following equation:—

$$2(C_6H_5SM) = (C_6H_5)_2S + M_2S.$$

Experiments in progress seem to show that thiophenol may be converted into benzol by losing its sulphur, or into phenol by exchanging it for oxygen.—(*Zeitschr. Chem.*, N F., iii. 193.)

Thiacetic Acid from Acetic acid Phenol (C=12).—A. Kekulé. Phenol is still very generally believed to be in every respect analogous to the monatomic alcohols. It may now, however, be taken as certain that the so-called phenylic chloride, iodide, etc., are identical with the corresponding substitution compounds of benzol, and that the phenyl therein is incapable of being transferred to other compounds. According to the old view, acetic acid phenol acted upon by potassic sulphhydrate ought to give potassic acetate and thiophenol:—

$$\left.\begin{array}{l} C_6H_5O \\ C_2H_3 \end{array}\right\} O + \left.\begin{array}{l} K \\ H \end{array}\right\} S = \left.\begin{array}{l} C_6H_5 \\ H \end{array}\right\} S + \left.\begin{array}{l} C_2H_3O \\ K \end{array}\right\} O$$

The reaction, however, takes place in a different manner; phenol is formed and potassic thiacetate:—

$$\left.\begin{array}{l} C_6H_5 \\ C_2H_3O \end{array}\right\} O + \left.\begin{array}{l} K \\ H \end{array}\right\} S = \left.\begin{array}{l} C_6H_5 \\ H \end{array}\right\} O + \left.\begin{array}{l} C_2H_3O \\ K \end{array}\right\} S$$

Showing that it is not phenyl and the metal of the sulphhydrate which exchange their places, but hydrogen and acetyl, and that acetic acid phenol should not be viewed as phenylic acetate, but rather as acetylic phenylate.—(*Zeitschr. Chem.*, N.F., iii. 196.)

Phenol, Sulphoacids of (C=12).—A. Kekulé. The author considers the so-called phenylsulphuric acid to be a sulphoacid, containing a residue of sulphuric acid in a similar manner as nitro compounds contain a residue of nitric acid. He gives it the formula

$$C_6H_4\left\{\begin{array}{l}OH\\ \left.\begin{array}{r}S''O_2\\ H\end{array}\right\}O\end{array}\right.$$

in place of the old one,

$$\left.\begin{array}{r}C_6H_5\\ S''O_2\end{array}\right\}O$$
$$H\Big\}O.$$

The action of sulphuric acid on phenol, however, gives rise to the formation of two isomeric bodies—phenolparasulphuric acid and phenolmetasulphuric acid The salts of the first being less soluble in water than those of the latter, their separation is easily accomplished by fractional crystallisation. If the phenolsulphuric acids still contain their original hydroxyl, as is assumed here, the hydrogen of the latter will be replaceable by an alcohol radical, without thereby causing a change in the general character of the sulphoacid. If methyl be introduced, sulphoacids will be formed, identical in composition with the acid obtained from anisol with sulphuric acid.

This substitution is effected by heating phenolpara- (or meta-) sulphuric acid with potassic hydrate, alcohol, and the iodide of the desired alcohol radical in sealed tubes.

The potassic salts of the phenolmeta- and phenolparasulphuric acid thus obtained resemble each other so closely that it is difficult to say to which of them the corresponding anisol compound belongs; but it is believed that there also exist two isomeric anisolsulphuric acids. The author also is of opinion that the action of sulphuric acid on aromatic compounds nearly always gives rise to substitution compounds.—(*Zeitschr. Chem.*, N.F., iii. 197.)

Monochlorphenyl (C=12).—Ed. Dubois has succeeded in obtaining monochlorphenol by passing a slow current of chlorine through cooled phenol. 500 grms. of phenol were taken, and the action of chlorine continued for twelve hours. On distillation, the portion going over between 212° and 225° C. was collected separately and rectified. He finds the compound thus prepared identical with the monochlorphenol obtained by the action of sulphurylchloride on phenol. Nitric acid converts it into dinitromonochlorphenol. Griess has obtained this body by treating phenol first with chlorine, and then, without previous purification of the chloro-compound, with nitric acid. The author believes he has observed differences between his compound and that of Griess, and it would, therefore, appear probable that in the latter chemist's case first di- or trichlorphenol was formed (vide *Ann. Chem. Pharm.* cxx. 286), which afterwards exchanged a portion of the chlorine against nitryl.—(*Zeitschr. Chem.*, N.F., iii. 205.)

Aluminic Sulphates, Basic(O=8).—H. Debray. Zinc in contact with platinum (or lead) is readily acted upon by a hot solution of alum. A precipitate is formed of the composition of Lowigit;

$$KOSO_3+3(Al_2O_3,SO_3)+9HO,$$

which is almost insoluble in strong chlorhydric or nitric acid, but attacked by sulphuric acid. Heated to low redness it loses nearly all its water; at a still higher temperature it decomposes into alumina and potassic sulphate. Finally powdered calcic carbonate digested in the cold with a solution of alum in excess forms a salt, readily soluble in dilute acids. Its composition is

$$4Al_2O_3,3SO_3+36HO\ .$$

A solution of aluminic sulphate in excess, boiled with zinc or platinum, forms a precipitate soluble in dilute acids; dried at 100°C., its composition is $SAl_2O_3,3SO_3+20HO$.—(*Bull. Soc. Chem.* 7, 9.)

Nitrotoluol.—A Kekulé, in a previous communication (*vide* Chemical News, No. 386), confirmed Jaworsky's statement, that the crystalline nitrotoluol is the pure compound, and the liquid not an isomeric modification of it, but simply a mixture of the pure with some other bodies. He now publishes some experiments he made with both the solid and the liquid nitrotoluol. If ordinary nitrotoluol is distilled, the greater portion goes over between 220° and 225° C.; the fraction distilling above 233° soon solidifies. The latter is purified by repeated distillation and crystallisation from alcohol or ether. The nitrotoluol thus prepared boils at 237° C., and crystallises readily. Nirtic and sulphuric acid convert it into dinitrotoluol. On oxidation with potassic bichromate and sulphuric acid, paranitrobenzoic acid (nitrodracylic acid) is formed; the oxidation proceeds more readily than it does with ordinary nitrotoluol, and a better result is attained, both as regards quantity and quality. Toluol prepared from toluolsulphuric acid yields by careful treatment a nitro compound which after the first distillation solidifies to a great extent. From the facts—1st, that on liquid nitrotoluol being reduced with tin and chlorhydric acid, no hydrocarbon is observed to be given off with the vapours of water: 2ndly, that the toluidine prepared from liquid nitrotoluol contains aniline; and 3rdly, that a mixture of pure nitrotoluol and nitrobenzol on distillation behaves in a very similar manner to ordinary nitrotoluol—the author believes that the liquid state of ordinary nitrotoluol is due solely to an admixture of nitrobenzol. As to the source of this benzol in those cases where it was not originally present in the toluol, the author thinks it possible that under favourable conditions nitric acid acting on toluol (methylbenzol) might destroy the methyl, and, substituting nitryl, form nitrobenzol.—(*Zeitschr. Chem.*, N. F., iii. 225.)

Hydrocarbons, Solid, from Coal-tar. Fritzsche (C=6).—The hydrocarbon $C_{28}H_{10}$, investigated by Anderson, and considered by that chemist to be the anthracene of Dumas and Laurent, the author believes to have been a mixture of the pure $C_{28}H_{10}$ with other hydrocarbons of lower melting points. The melting point given by Anderson is 180° C., by Dumas and Laurent 213° C., by Fritzsche (five years ago) 210° C.

The author has now succeeded in obtaining this body in a state of perfect purity, and, though unable as yet to give an exhaustive comparison of its properties with those of the accompanying hydrocarbons of coal-tar, he has established some characteristic reactions, which will suffice for their identification.

The body $C_{28}H_{10}$ separates from its solutions usually in thin, flat crystals, which are never twisted and always of a definite shape. The best mode of observing this is to dissolve a small quantity in a drop of ether on a glass plate; on evaporation a spot is left which appears transparent, but under the microscope is seen to consist of a mass of crystals which, especially along the edge, are well-formed hexagonal plates. The transparency and crystalline structure of the spot are peculiar to this body, which, by that means, can be distinguished from another closely related to it. This latter body dissolves more readily in ether, and leaves a spot less transparent, showing a crumpled appearance at the edges. The spot produced by a mixture of the two is white, perfectly opaque, and with the rim as before.

On cooling an alcoholic solution of the body $C_{28}H_{10}$, flat leaf-shaped crystals, either hexagonal or rhombic, appear, which grow on the sides of the vessel. The other body, under these conditions, forms a voluminous mass of thin twisted plates, which remain suspended in the liquid. A mixture of the two always shows the latter mode of crystallisation. It is then, however, easily seen, with a magnifying glass, that the crystals are not homogeneous.

The method employed by the author for the separation and purification of the body $C_{28}H_{10}$ consists in dissolving the raw material in coal-tar naphtha, redissolving again the first portions that are separated from the solution, and repeating this process several times, until the above-described characteristic phenomenon of crystallisation is observed. To free the body from a yellowish colour, caused by an admixture of chrysogene, its hot solution in coal-tar naphtha is exposed to direct sunlight. On cooling the bleached liquid, large crystals of the pure material are obtained. It is well to treat them finally with a boiling alcoholic solution of picric acid, which removes any admixture of a body of much higher melting-point.

The melting-point of the pure body $C_{28}H_{10}$ the author has never found higher than 207° C.

The behaviour of this body towards light is very remarkable. If a cold saturated solution of it is exposed to direct sunlight, microscopic crystals soon appear, varying in shape according to the nature of the solvent—elongated hexagonal plates from coal-tar naphtha, four-sided rhombic ones from alcohol. These crystals differ greatly from the mother substance. They are almost insoluble in any solvent, and are altogether very indifferent; but if they are melted (a higher temperature being required than for $C_{28}H_{10}$) they are converted again into the original body $C_{28}H_{10}$.

The action of nitric acid on $C_{28}H_{10}$ gives rise to a number of other compounds. The mode of operating was as follows: —The powdered substance was mixed with glacial acetic acid, and nitric acid added drop by drop. The liquid assumes a yellow colour, and, if heat be avoided, a clear dark yellow solution is obtained. On addition of water a resinous precipitate is formed; if the liquid is allowed to stand, evolution of gas gradually sets in, and crystalline bodies are precipitated. If it is heated to 50°—60° C., red vapours are given off, and crystals separate. Of these various derivatives the author describes three:—

1. A colourless body, soluble in alcohol and benzol, which separates from the latter in monoclinometric crystals; is decomposed by continued boiling with acetic acid; forms a precipitate, which combines with picric acid, on adding a basic substance to its alcoholic solution.

2. Large, colourless, prismatic crystals. An alkali added to their solution in alcohol colours the liquid dark orange, and an acid is formed, the potassic salt of which crystallises in red needles. A current of ammonia passed through its benzolic solution produces a dark red amorphous precipitate.

3. A body endowed with the property of combining with nearly all solid hydrocarbons from coal-tar as well as with retene and idrialine. The body is obtained in microscopic crystals, sparingly soluble in most solvents, but soluble to some extent in benzol.

This new nitro-body combines with $C_{28}H_{10}$, forming beautiful large violet crystals of the rhombic system. If they are heated to 170°—180°C., the body $C_{28}H_{10}$ is volatilised, and the nitro-compound remains behind. They also decompose on being dissolved in benzol, the nitro-compound crystallising first; or if treated with acetic or nitric acid, in which case the hydrocarbon is extracted.

Besides the body $C_{28}H_{10}$, the author has obtained from coal-tar three others (α, β, γ) melting at about 190° C., one (δ) melting at about 235° C., and another (ϵ) at about 100° C. Neither of them, however, has been obtained pure. They all combine with the new nitro-compound, which combination furnishes the chief means of distinguishing them from each other.

α, the body already alluded to as resembling closely $C_{28}H_{10}$, gives with the nitro-compound a dark red-brown substance, much less soluble than that produced by $C_{28}H_{10}$. Like the latter, it is converted, on exposure to light, into a "para-" compound.

β forms with the nitro-compound a dark green product.

γ, an orange-coloured, insoluble compound, crystallising from benzol in needles. This body is insoluble even in hot concentrated sulphuric acid.—(*Bull. Acad. Imp. Petersb.* vii.)

Toluol- and Benzol-sulphurous Acid—R. Otto (C=12, O=16). Toluolsulphurous acid, heated with water to 150° C. in sealed tubes, splits up into toluolsulphuric acid and a body of the composition C_7O_8SO, according to the following equation:—

$$2C_7H_8SO_2 = C_7H_8SO_3 + C_7H_8SO.$$

The new body is not acid, is insoluble in water, readily soluble in hot alcohol, from which it separates in rhombic crystals. Zinc and sulphuric acid convert it into metabenzylsulphhydrate.

The decomposition of benzolsulphurous acid takes place in a similar manner.—(*Zeitschr. Chem.*, N. F. iii. 262.)

Acids with Water at High Temperatures.—W. Markownikoff and Th. v. Purgold (C=12, O=16). Citric acid, when heated with water in sealed tubes to 160° C. splits up into carbonic anhydride and itaconic acid, according to the following equation:—

$$C_3H_4(HO)\left\{\begin{array}{l}CO.OH\\CO.OH\\CO.OH\end{array}\right. = C_3H_4\left\{\begin{array}{l}CO.OH\\CO.OH\end{array}\right. + CO_2 + H_2O.$$

Similar reactions are obtained with tartaric, quinic, lactic, anisic acid, and some substitution-compounds of benzoic acid.—(*Zeitschr. Chem.*, N. F., iii. 264)

MISCELLANEOUS.

Chemical Society.—The next meeting of the Society will be held on Thurday evening, at 8 o'clock, when a lecture "*On the Mode of Representation afforded by the Chemical Calculus, as contrasted with the Atomic Theory*," will be delivered by Sir B. C. Brodie, Bart. A large attendance of Fellows and visitors being anticipated on this occasion, the Royal Society have kindly placed their large meeting room at the disposal of the Chemical Society for that evening.

Dr. Odling's Lecture on the Absorption of Gases by Metals.—Our verbatim report of the above lecture was not returned corrected by the author in time for insertion in this number, but will certainly appear in our next.

The Paris Exhibition.—One of our earliest contributors, who is at the same time an eminent scientific chemist and a valued member of our editorial staff, is on the point of starting for Paris. His attention will be more especially directed to the English department of the Chemical Exhibition, and the results of his observations will be placed before our readers in a series of articles. These will be in addition to the usual articles forwarded weekly by our Exhibition correspondent.

British Association of Gas Managers.—The fourth annual general meeting of the members of this Association is announced to be held at Nottingham on the 11th, 12th, and 13th of June, when the following papers will be submitted:—"*The Utilisation of the Residual Products of the Manufacture of Coal Gas, with especial reference to the Production of Aniline Colours from Coal Tar*," by Dr. Letheby, M.B., M.A., etc., Professor of Chemistry in the College of the London Hospital, Gas Analyst and Medical Officer of Health for the City of London. "*On the Practical Working of the Liquor System of Purification;*" being a statement of results in continuation of the paper of last year on "An Improved Method of Purifying Coal Gas," by Mr. George T. Livesey, South Metropolitan Gasworks, London. "*On the Purification of Gas from Ammonia, and the Utilisation of the Product*," by Mr. George Anderson, London. "*Notes on the Manufacture of Sulphate of Ammonia*," by Mr. W. Esson, Gasworks, Cheltenham. "*On the Application of Liquid Hydrocarbons as a Substitute for Cannel in the Manufacture of Gas of High Illuminating Power*," by Mr. E. Goddard, Gasworks, Ipswich. "*Some Remarks on the Explosive Properties of Fire-damp and Coal Gas, with Particulars of Experiments made in Lighting Portions of the Oaks Colliery with Pit Gas*," by Mr. J. Hutchinson, Gasworks, Barnsley. "*On Leakage from Gas Mains*," by Mr. E. S. Cathels, Gasworks, Crystal Palace District. "*On the Valves of Gas Purifiers*," by Mr. W. J. Warner, Gasworks, South Shields.

Action of Charcoal in Removing Organic Matter from Water.—Mr. Edward Byrne has examined how far the statements generally made with regard to the action of charcoal in purifying water might be depended on. Nearly five pounds of new and freshly burned animal charcoal, of the degree of fineness used in sugar refineries, were packed in an ordinary stoneware filter. The water employed contained, in the gallon, organic matter, 10·80 grains; inorganic matter, 88·30 grains. The hardness before boiling, was found to be 50·50°, and after, 33°; and the oxygen required to oxidise the organic matter contained in one gallon amounted to 0·0116 grain. Several gallons of the water were allowed to percolate slowly through this charcoal, and, upon examination afterwards it was found that, of the *inorganic matter*, 52·60 grains were removed from the first gallon, but from each succeeding gallon less and less, so that, from the twelfth gallon that passed through the charcoal, only 8·80 grains of inorganic matter were removed. Of the *organic matter*, 4·80 grains were removed from the first gallon; but, with a gradual decrease, the charcoal ceased to remove any after the sixth gallon. In fact, immediately afterwards, it commenced to give back a portion of the organic matter removed in the first instance, the quantity returned to the twelfth gallon amounting to 1·55 grain. Thus, of the 13·54 grains of organic matter removed by the charcoal from the first six gallons of water, as much as 4·98 grains were given back to the next six gallons, from which the author concluded that, had this set of experiments been carried a little further, all the organic matter removed at first by the charcoal would have been given back again. The author, in conclusion, gave it as his opinion that, by chemical agency, bad water could be purified to a very limited extent only. [Mr. Byrne has overlooked the important fact that the efficacy of charcoal as a filtering agent for the removal of organic matter does not depend upon mechanical absorption, but upon oxidation; and from his drawing no distinction between the removal, by charcoal, of the inorganic and the organic impurities of the water, it would seem as if he were ignorant of this property. A few ounces of charcoal, used properly, will oxidise a pound or more of organic matter. Charcoal has not fair play in a filter unless it is occasionally allowed an opportunity of absorbing atmospheric oxygen.—Ed. C. N.]

Composition and Quality of the Metropolitan Waters in May, 1867.—The following are the returns of the Metropolitan Association of Medical Officers of Health:—

Names of Water Companies.	Total solid matter per gallon.	Loss by ignition.*	Oxidisable organic matter.†	Hardness.	
				Before boiling.	After boiling.
Thames Water Companies.	Grains.	Grns.	Grains.	Degs.	Degs.
Grand Junction	21·17	1·00	0·68	12·0	4·5
West Middlesex	19·00	0·45	0·40	11·5	3·5
Southwark & Vauxhall	18·43	0·50	0·46	12·0	4·0
Chelsea	20·83	0·75	0·69	12·0	4·0
Lambeth	20·77	0·50	0·64	12·0	4·0
Other Companies.					
Kent	26·62	0·20	0·18	15·0	8·0
New River	16·80	0·50	0·21	11·0	4·0
East London	18·74	0·51	0·39	11·0	4·0

Death of Pelouze.—We have to announce the death of one of the best and most celebrated French chemists, M. Pelouze, Master of the Mint in Paris, which took place on the 31st ult. The previous day he had been attacked by heart dropsy, and he expressed an urgent desire once more to breathe the pure air of the heights of Bellevue (near Meudon). No sooner was he in the carriage than a faintness came over him from which he recovered with much difficulty. His family yielded to his wish by taking him to the desired spot, where he arrived in the evening only to die on the following morning at 7. Since the sudden death of his excellent and distinguished wife he pined away, notwithstanding the affectionate attentions paid him by his son and three daughters. He was born at Valonges in the department of the Manche, in 1807, and was, at his first outset of life, a simple laboratory student. He became successively Professor at the Polytechnic School, Professor at the French College, Member of the Academy of Sciences, Verifier of the Mint Assays, Member of the Municipal Council of Paris, Director of the St. Gobain Glass Works, and, lastly, President of the Commission of the Mint, the highest post that a practical chemist can aspire to. M. Pelouze died with resignation, and in the faith of a Christian. He was buried at Montmartre Cemetery, in the family tomb, the corpse being followed by an immense *cortège*, composed of all the *élite* of society, the principal members of the Academy, six carriages of the Municipal Council, and the National Guard in full uniform. M. Fremy, the distinguished chemist of the *Conservatoire des Arts et Métiers*, delivered the usual funeral oration.

Action of Light on Chloroform.—The chloroform used for the experiments had a specific gravity of 1·492 at 70° F., was absolutely free from acid reaction, and imparted no coloration whatever to sulphuric acid. The diluted chloroform was made of eight ounces of the former, by the addition of one fluidrachm of strong alcohol. The bottles used for the occasion were made of flint glass, of uniform size and shape, and filled alike. The experiments lasted one week during the hot days in August. It was concluded from these experiments:—1. That in order to preserve pure chloroform of specific gravity 1·49, it should be kept totally excluded from the light. 2. That to keep chloroform in the daylight, it should be reduced in specific gravity by the addition of about two fluidrachms of 95 per cent. alcohol to one avordupois pound of chloroform, sp. gr. 1·492. During the repetition of some of these experiments, attention was drawn to the presence of moisture in some of the bottles, and it was determined to try its effects on chloroform; accordingly, chloroform of 1·492, dried by standing over chloride of calcium, was kept in absolutely dry bottles, and in bottles slightly moist, and both kinds exposed to diffused daylight and direct sunlight. The bottle containing the moisture always showed the presence of free chlorine much sooner than the dry one, though the entire absence of moisture would not be sufficient to preserve the chloroform unaltered. But, if the chloroform had been reduced in specific gravity to 1·475 or less, the presence of several drops of water in the bottle would not induce the liberation of chlorine after an exposure of two weeks to the direct sunlight. For medicinal purposes—that is, for inhalation—this amount of alcohol would be unobjectionable, since it amounts in one fluidounce only to about forty drops.—*J. M. Maisch, Proc. Am. Pharm. Assoc.*

Chemical Society.—The next meeting of the Society will be held on Thursday evening, at 8 o'clock, when the following papers will be read:—"*On Derivatives of Hydride of Salicyl,*" by Mr. Perkin; "*Analysis of Biliary Concretion,*" by Dr. Phipson; "*On Pyrophosphoric Acid,*" by Dr. Gladstone.

Magnesium.—We understand that the Magnesium Metal Company are progressing in the manufacture of magnesium wire and riband. They have found from experience that if magnesium riband is pressed broader and thinner, and by this means made to present a larger surface to the oxygen of the atmosphere for the same weight of metal, it burns much more steadily and surely. They are now supplying the metal in this form.

Extractum Conii.—Dr. John Harley has proved by experiment that the ext. conii is a very uncertain, if not an inert, preparation. He attributes this to the fact that the active principle of the plant is, to a certain extent, vaporisable, even at a natural temperature of 70° to 90° Fahr., and that a prolonged exposure to a high temperature is accompanied by a progressive diminution of the conia, the alkaloid being converted into ammonia and some other secondary product. It is, therefore, necessary, in order to obtain an extract

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* The loss by ignition represents a variety of volatile matters, as well as organic matter, as ammoniacal salts, moisture, and the volatile constituents of nitrates and nitrites.

† The oxidisable organic matter is determined by a standard solution of permanganate of potash, the available oxygen of which is to the organic matter as 1 is to 8; and the results are controlled by the examination of the colour of the water when seen through a glass tube two feet in length and two inches in diameter.

Hy. Letheby, M.B.

of full power, to expose the juice in shallow dishes to a rapid current of dry air having the temperature of 150° Fahr. By this process, an extract containing one per cent. of conia may be procured.—*Pharm. Journal.*

Sublimation of the Alkaloids.—Dr. Guy suggests the following mode of procedure:—Provide small crucible covers or slabs, or fragments of white porcelain, a few microscopic cell-glasses, with a thickness of about one-eighth of an inch and a diameter of circle of about two-thirds of an inch, and discs of window-glass about the size of a shilling. Place the porcelain slab on the ring of a retort-holder, then the glass cell, and upon the porcelain, in the centre of the cell, a minute portion of the alkaloid or other white powder or crystal reduced to powder. Then pass the clean glass disc through the flame of the spirit lamp till the moisture is driven off, and adjust it over the glass ring. Now apply the flame to the porcelain, underneath the powder, and continue to heat till the powder undergoes its characteristic change and gives off vapour. Watch the deposit of this vapour on the glass disc, and remove the spirit lamp, either directly or after a short interval, as experience may determine. By this process Dr. Guy has succeeded in getting very beautiful crystalline sublimates of morphia, strychnine, solanine, and cryptopia. He recommends it in preference to that of Dr. Helwig.—*Pharm. Journal.*

Cryptopia.—Messrs. T. and H. Smith have discovered a new alkaloid in opium, which they have named cryptopia. It is extracted from the weak spirituous washing of crude precipitated morphia, but the quantity yielded by opium is very small, Messrs. Smith having operated upon four or five tons of opium, and only obtained five ounces of muriate of cryptopia. They have prepared the sulphate, muriate, nitrate, thebolactate, and the acetate; these all crystallise in beautiful and distinct forms, but the alkaloid itself has much better defined crystalline forms than any of its compounds. Its primary form is a hexagonal prism, and it is obtained in this condition if crystallised slowly in a tube from its alcoholic solution. The formula of the new alkaloid is $C_{23}H_{25}NO_5$.—*Pharm. Journal.*

Preparation of Hydrate of Sodium from Sodium.—At the recent *soirée* of the Pharmaceutical Society, a large block of pure fused hydrate of sodium, prepared from metallic sodium, was exhibited, and excited much attention. It is thought that a short account of the means adopted to oxidise so explosive a substance may not be without interest to our readers. Sodium, as prepared for the market, is cast in moulds, which are well smeared with oil, which coats the metal and prevents it oxidising; but the sodium from which the hydrate is made is cast in bright clean moulds. When removed it is well rubbed with a clean linen cloth, in which it is encased to prevent contamination from the atmosphere. The bars of sodium are now cut into lumps about one inch square. One of these lumps is thrown into a perfectly clean silver dish, which floats on a stream of cold water. A few drops of distilled water are poured on the sodium, and the vessel is well agitated by hand, which prevents explosions. When the first lump of sodium is dissolved, another piece is thrown in, additional drops of water are added, and the vessel kept constantly agitated, and so on throughout the operation. After a deposit of soda forms at the bottom and around the sides of the vessel, and the liquid becomes completely saturated, the tendency to explosions seems much reduced. If the dish remains quiet, great amount of heat is generated, and the fusing sodium bursts out like a tiny volcano, scattering globules of fire—*i. e*, burning sodium—all around; but if the vessel is kept in constant motion, a fresh surface of cold water is brought into contact with the fusing sodium, its temperature is reduced, and explosions are almost prevented. The milky liquid thus prepared is now filtered, and then fused in a silver crucible, over a gas furnace to a dull red heat, or until all moisture is driven off and the liquid becomes perfectly transparent. The crucible is carefully covered up, and the contents allowed to cool; but as the hydrate of sodium is very deliquescent, absorbing moisture even when too hot to be handled with impunity; it is removed from the crucible whilst warm, quickly broken into lumps, and placed in well-stoppered bottles. The operation is, at the best, a slow and tedious one, accompanied with an unpleasant smell and some annoyance, as, with the utmost care, explosions cannot be entirely avoided. A steady workman will dissolve up, working with one dish, about 1½ lb. of sodium per day, but he could be trained to take charge of two dishes.

Preparation of Dilute Phosphoric Acid.—Introduce into a French glass tubulated retort, of capacity of forty-two parts, twelve parts of water and two parts of phosphorus. Place the retort on a sand bath, and introduce through a funnel tube, fixed in the tubulure by means of a cork and reaching half an inch below the level of the liquid, eight parts of nitric acid. Apply gentle heat, and watch the operation closely as soon as reaction commences. When the reaction slackens add more nitric acid in portions of about one-fourth part at a time. Should the reaction become violent, small quantities of warm water must be added until it is reduced to its ordinary action, which may be compared to the gentle boiling of water. The formation of frothy bubbles on the surface of the liquid is always the forerunner of violent reaction, and should be checked at once. I have found that if it was checked at this stage, a comparatively small amount of water would answer, but if allowed to react violently a much larger quantity of water was necessary. The evaporation of the acid, after the phosphorus is all oxidised, is conducted in a porcelain capsule; towards the end of this process it will froth up, owing to the rapid disengagement of nitric oxide. The dish must therefore have about three times the capacity of the acid when concentrated, and a little distilled water should be kept conveniently near, to add in case there is danger of frothing over. It is scarcely necessary to add that the operation should be conducted under a good furnace hood, or otherwise the beak of the retort should be introduced into a good flue.—*C. L. Diehl, Am. Pharm. Assoc.*

Determination of Arsenic in Sulphide of Arsenic.—To determine the arsenic contained in sulphide of arsenic—an operation often necessary for the estimation of arsenic—M. Graebe uses a standard solution of iodine, as in the estimation of arsenious acid. Suspend the sulphide of arsenic in water, add some carbonate of soda, then a little starch paste, and the standard solution of iodine. (It is evidently necessary that the sulphide of arsenic should be freed from sulphuretted hydrogen.) The reaction takes place according to the equation—

$$AsS_3 + 5I + 5HO = AsO_5 + 5HI + 3S.$$

—*Journal für praktische Chemie*, xcvi. 261.

Volumetric Estimation of Lead and Tin.—M. Graeger determines the lead by ferrocyanide of potassium; the decomposition takes place according to the equation—

$$2(PbO,NO_5) + 2(KCy),FeCy = 2(PbCy),FeCy + 2KO,NO_5.$$

Ferrocyanide of lead is almost insoluble in acids, and its precipitation is easy. The author employs a standard solution of ferrocyanide of potassium; when all the lead is precipitated, the liquid colours ferric salts blue, which may be tested in one drop of it. An excess of ferrocyanide may be added, and determined in the filtered liquid by permanganate of potash. As a control, the ferrocyanide of lead may be suspended in water, and titrated by permanganate. Tin, in the state of bichloride, may be estimated in the same way, but not when in the state of protochloride; in that case it must be first changed to bichloride.—*Journal für praktische Chemie*, xcvi. 330.

Larkin's Magnesium Powder Lamp.—It is a singular circumstance that England, so pre-eminently the home of mechanical invention and skill, should hitherto have failed to produce a good lamp for the combustion of magnesium. A lamp answering this designation must be simple in construction and certain in its operation, the light must be continuous and well under control, and mechanical skill should help to economise a light necessarily costly from the present limited scale of its production. Mechanical ingenuity ought also to assist the continuance of a flame liable to go out so long as it relies for its source upon one solitary wire or ribbon. The best magnesium lamps hitherto invented have come from America, which country has far distanced England in the applications of magnesium, and in the ingenious construction of apparatus for consuming it. In making these remarks we intend no disparagement to Solomon's lamp. It was constructed with the object of assisting photographers, on dull, wet, and foggy days, to follow their avocation. And as it was early found that from four to eight grains of magnesium were sufficient to take a single portrait, a lamp working from three to five minutes was amply sufficient, and this want Solomon's lamp supplied; but for those manifold uses where the light was required for a longer period, one or other of the American lamps had to be used. But this slur upon English mechanical ingenuity no longer exists. In Larkin's magnesium powder lamp we have a lamp adapted to many requirements. It is very simple in construction, and it need not be a very costly lamp. In it the light is continuous and absolutely under control; the combustion of the metal, and the light arising therefrom, is also greatly economised. The leading features of the lamp are these:—A reservoir, funnel-shaped, and attached to it a long narrow tube, at the end of which is placed a small spirit lamp—these, with mechanical details controlling and guiding each part, constitute the whole of the invention. And yet it will be easily seen that these principles are capable of a great variety of modifications of form and of manifold application. For instance, in the reservoir can be placed, according to the brightness of the flame required, either pure magnesium reduced to dust, or magnesium powder, mixed one-half, two-thirds, or three-fourths with sand, or, if a coloured flame be required, nitrate of strontia or other chemical substances can be added. These mixtures flow down the funnel-shaped tube—as sand flows down the common hourglass—and through a small spirit flame placed at the orifice of the tube, where the magnesium ignites into a continuous flame; the spirit flame insures the ignition of the magnesium, and prevents the fouling and blocking up of the tube by the oxide of magnesium, which would otherwise form around the exit tube. The mixture can be stopped or turned on, as required, by means of a small finger tap. In this way the combustion of the metal is economised, and the amount of light and its continuity are absolutely under control. The hand-lamp now before us, constructed on this principle, is light and portable, hardly exceeding in size an ordinary watchman's lantern. It is not easy to predict the uses opening out for so bright a light and so portable a lamp, but they must be very numerous. It seems admirably adapted for valuing mines and surveying underground workings. It might be used with great advantage in deep sea fishing, and as a decoy light by sportsmen and naturalists; it may also before long be used for lighthouse purposes, and for lighting the entrance to harbours and signalling at sea.

On the Decomposing Action of High Temperatures upon some Sulphates,[*] by M. Boussingault.—It appears from the experiments made that the sulphates of lime, magnesia, lead, etc., are decomposable at a white heat, and that, in consequence, in analytical researches, their calcination should be made only at a moderately elevated temperature; and, although the complete separation of the elements of the acid of the sulphates of strontia and baryta does not take place except at the melting heat of iron, precaution must be taken in their calcining, as it appears certain that the decomposition of these sulphates commences at a much lower temperature. As to the volatilisation of the alkaline sulphates, this must be taken into careful consideration when vegetable ashes are being examined and their proportions estimated; for, if the ashes are obtained at a high temperature, a perceptible loss of alkaline salts takes place, especially those of potassium, which are more volatile than those of sodium.

General Metallurgical Method of Messrs. Whelpley and Storer.—For the application of the chemical portion of this process, the ore must be in the state of very fine powder. The calcination of the ore in this state takes place in what is called a water-furnace. This consists of a fire tower about 20 feet high, 3 feet in diameter at the top, and 4 feet at the base, built of brick with double walls, and having a large water tank at the bottom. Around the upper portion are four fire boxes, opening into the tower, which is closed and connected with a large fan-blower. By this means, besides the supply of air heated by passing between the two walls of the tower, air and fuel, in the state of dust, are carried down into the furnace. The heating effects obtained are very surprising. In the calcination of sulphuretted ores, only a moderate temperature and a large supply of oxygen are required. The fire tower of the water furnace being heated to redness, the ore, with or without pulverised fuel, is driven by a small fan down the tower. The sulphur and base metals are rapidly oxidised, and the calcined ore falls into the water-tank below, the current of air being carried through successive chambers open to the tank beneath. In the case of sulphuretted ores of copper, the water-tank is filled with a solution of the chlorides of sodium and calcium, by which, with the aid of a spray wheel at the end where the air current has exit, the sulphurous acid is absorbed, and the oxide of copper converted into the dichloride. Mr. T. Sterry Hunt, F.R.S., finds the reaction to take place according to the following equation:—

$$\mathrm{CaCl + SO_2 + Cu_2O_2 = CaSO_4 + Cu_2Cl.}$$

The dichloride of copper is held in solution by the chloride of sodium. A small portion of protochloride of iron generally occurs in the solution, which is separated by addition of oxide of copper, according to this equation:—

$$\mathrm{Cu_3O_3 + Fe_2Cl_2 = Cu_2Cl + CuCl + Fe_2O_3.}$$

The addition of milk of lime precipitates the whole of the copper as hydrated oxide; simultaneously the bath is regenerated.

The British Association of Gas Managers held a very successful congress at Nottingham on the 11th, 12th, and 13th inst., under the presidency of Mr. Hawksley. About eighty members attended, and thirty-eight new members were elected, the total number now amounting to 178. Several interesting papers were read, including the continuation by Mr. G. T. Livesey of his communication "*On the Practical Working of the Liquor System of Purification;*" a paper by Mr. G. Anderson "*On the Extraction of Ammonia from Gas, and the Utilisation of the Product;*" a communication "*On the Application of Liquid Hydrocarbons as a Substitute for Cannel,*" by Mr. E. Goddard. Mr. E. S. Cathels read a paper "*On Leakage from Gas-mains;*" and papers "*On the Manufacture of Sulphate of Ammonia,*" by Mr. W. Esson; "*On the Explosive Properties of Firedamp,*" by Mr. J. Hutchinson; and "*On the Valves of Purifiers,*" by Mr. W. J. Warner, were also read. Dr. Letheby, as on former occasions, delivered a lecture to the Association, the title of it being "*The Utilisation of the Residual Products of the Manufacture of Coal Gas, with especial Reference to the Production of Aniline Colours from Coal Tar.*" The learned author is preparing a full report of this lecture for our columns. It was agreed to hold the congress in London next year, when a large accession to the number of members is expected to take place, as the metropolitan gas managers, who have hitherto kept aloof, will most probably take that opportunity of joining the Association.

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[*] Read before the Academy of Sciences, June 10.

NOTES AND QUERIES.

Manufacture of Sulphuric Acid.—Sir,—I see by the Chemical News of April 26 (just received), that you propose to aid manufacturers by placing them in communication with competent parties to get them out of their difficulties. Now, I am in connexion with a company who have been for some time past manufacturing sulphuric acid from sulphur, but now wish to use pyrites, and desire to know the best process for that purpose. Our chamber is of the ordinary construction, and of the capacity 29,000 cubic feet. We have already built four pyrites kilns; the internal capacity of each is about 1·51 m., and the distance these are placed from the chambers is 21 m., connected by an acid-proof earthenware tube of 0·50 m. diameter. Is this distance and tube sufficient to arrest any foreign matter the mineral may contain from getting into the chamber, and do you consider the kilns of good shape for burning Huelva or St. Domingo's ore freely? My object is to be placed in communication direct with a practical person who would give me all the information for working a chamber with pyrites on the best known plan, by correspondence and plans, if possible; and, if not, I would go or send a person to England for that purpose. Of course a fair remuneration would be given for the information. We concentrate our acid to 66 deg. Baumé in a platinum still. Would pyrites acid distil perfectly colourless, and as pure as that made from sulphur?—T. G. H., Lisbon.

Chinese Blue.—Sir,—In answer to the inquiry for the way to make Chinese Blue, I beg to say that I am quite able to show it for a consideration. I have been a colour-maker twenty years.—Samuel Johnson.

Estimation of Tartrates.—Sir,—In the Chemical News, No. 390, May 24, 1867, it is stated that a manufacturer of tartaric acid has expressed his willingness to give 100*l.* to any one who should succeed in discovering a correct and ready method for the estimation of tartaric acid in tartrates. Would you kindly give me the name of the manufacturer?—Chemicus.

[The statement is made on the authority of the authors of the book reviewed. Perhaps Mr. Watts will kindly give the required reference.—Ed. Chemical News.]

Palm Oil.—Reply to "Milo," May 24:—The quantity of chrome necessary to bleach 1 cwt. of palm oil varies with the kind of oil, of which there are several in the market. It may vary from 0·85 to 1·25 lb. Care should be taken to purify the oil from all foreign matter; else more chrome will be required.—"Bleach."

Chinese Blue.—Sir,—A substance called "Chinese Blue" has been referred to several times recently amongst your "Notes and Queries." I should feel much obliged to any of your correspondents who would tell me what is the composition of this substance.—"Learner," St. Helen's, Lancashire.

Naphthaline.—Sir,—Is there any process (heat excepted) by which naphthaline can be kept liquid?—W. B.

Carbon Biscuits.—Sir,—Can any of your readers kindly supply me with a recipe for making "carbon biscuits;" also where they are to be procured?—Sheff.

Dyeing Fancy Leather.—Sir,—I should be glad if you could inform me where I can purchase a work on dyeing and dressing fancy leathers, &c., and similar articles.—E. Church.

Repairing Looking-glass.—Sir,—I have a large looking-glass, with a patch of silvering about as large as a shilling rubbed off in the centre. Can I mend it? I am acquainted with the mode of making looking-glasses, and should think that with plenty of mercury and a piece of tinfoil somewhat larger than the bare spot, there would be no difficulty. Before I begin, I should, however, like to know what the experience of others may have been.—J. Thompson.

Santonine.—Sir,—In answer to your correspondent, I can recommend chloroform as a solvent for santonine for preparing good microscopic slides. Dissolve santonine in six or eight times its weight of chloroform, and allow a drop to fall on a glass slide and evaporate spontaneously. Beautiful rosettes of crystals will be left. They can be mounted with Canada balsam, or may be viewed dry.—F. R. W.

Sodium.—Sir,—If any one can give me a hint as to the best means of preserving sodium, I shall be greatly obliged.—N. Tyne.

Packing for Pumps for Corrosive Liquids.—Sir,—If one of your numerous correspondents would tell me what sort of packing I could use for a chemical pump, he would confer a great boon upon me. I want it for a plunger pump, to lift chloride of lime liquid. Yarn is eaten away in a few days. Vulcanised india-rubber is not affected by the "bleach," but there is a difficulty in screwing it tight.—M. James.

Carbon Biscuits.—Sir,—I cannot tell your correspondent "Sheff." how to make carbon biscuits, but I can strongly recommend highly carbonised bread as equally efficacious. Doubtless this mixed with flour and water would make good charcoal biscuits.

Dyeing Fancy Leather.—Sir,—If your correspondent, E. Church, will refer to O'Neill's "Chemistry of Calico Printing," or to the Dictionary of Calico Printing and Dyeing, by the same author, he will find a great deal of very useful information on this subject. Hunt's edition of Ure's Dictionary of Arts, article "Leather," also contains much that he will find useful.

Chinese Blue.—"Learner," St. Helen's, Lancashire, wishes to know the composition of Chinese blue. It is one of the synonyms of "Prussian blue," which is also called indifferently Paris, Berlin, and mineral blue. Full practical account in Watts's Dict., art. "Pruss. Blue," also under head "Ferricyanides," p. 245, subheading "Potassio-Ferrous Ferricyanide."

Repairing Looking-glass.—Sir,—J. Thompson will find that the plan he speaks of will repair the bare place; but there will generally be a mark at the outer circumference of the pool of mercury. I strongly advise him to scrape all the amalgam off, and deposit a coating of pure silver on the glass. By following Browning's excellent directions (quoted in your last volume) he will be able to do this in a few hours.—A. Milo.

Zinc Paper.—Sir.—Could you afford me information about buying or making zinc paper, covered on one side, as tin paper is?—J. C. J.

Packing for Pumps for Corrosive Liquids.—Sir,—Has Mr. James tried packing the piston with cork fibre? He will find this answer perfectly, and not be acted on by the chlorine.—F. W., Runcorn.

Production of Heat by Percussion.—Sir,—In many elementary works on science it is stated, as an illustration of the above, that blacksmiths are in the habit of hammering a cold nail on the anvil until it becomes red hot. Have any of your readers ever known of this being done? Can the heat rise to such intensity? Would not the cooling influence of the anvil be sufficient to keep the heat from rising to redness? and would not the iron be flattened out to foil first?—Scepti.

Preserving Sodium.—Sir,—The simplest and at the same time the most effective means of preserving sodium, is to immerse it completely in oil—Young's paraffin oil is the best—and then to place it in an empty air-tight jar or bottle. It ought to be immersed in oil after every time of using.—M.

Destroying Ants.—Sir,—My house is infected with a very small kind of ant, which at some seasons so swarm about the kitchen cupboards as to be a nuisance. Can you tell me how to get rid of them? They are much smaller and paler in color than the common garden ant.—E. M.

Use of Superheated Steam.—Sir,—I wish to try a plan of turning superheated steam through an ordinary evaporating pan. Now, I wish to know if any of your readers are aware if this plan has been tried, and with what result? Instead of passing the steam through the pipes of the pan direct from the steam-boiler, I propose to pass the steam through a superheater, and then through the pipes of an evaporating pan, such as may be used for boiling ale, alum liquor, etc. Information on this point will oblige.—S.

Gilding and Silvering.—Sir,—Can any of your correspondents inform me how to obtain dead or matt gilding, similar to what is seen on French clocks? Also, by whom are watch-dials gilt and silvered?—W. Smith.

Ants.—Sir.—I got rid of the ants in my house by adopting the suggestion you yourself made—viz., putting carbolic acid into the holes whence they were seen to issue. A few doses were effectual.—J. Th.

Wood Engraving.—Sir,—A Leipzig wood-engraver, particularly skilled in natural history illustrations, wishes to find a scope for his abilities among British publishers of illustrated scientific works. Perhaps some readers can tell whether there is a want of skilful artists of his kind felt in this country, or whether there is not much chance for a foreigner in this department.—G. L.

Preservation of Sodium.—Sir,—Permit me to observe the following:—Sodium is best of all kept and preserved by coating it with a layer of pure paraffin. I find that the well-known Professor Rud. Wagner, of Würzburg, recommended this mode of preserving sodium some time since, and that it is applied with perfect success. Of course some precautions are necessary, and first and foremost amongst these are, that only *pure* paraffin should be used; that, by being kept in a fused state previously to being applied for this purpose, it should be freed from water entirely; and then that it should be fused at its lowest melting point, 50 deg. Cels., and the sodium (melting point 95 deg.—96 deg. Cels.) immersed in it at no higher temperature than (say) from 55° to 60°. It is easy to remove the coating of paraffin when the sodium is wanted for use.—A.

ANSWERS TO CORRESPONDENTS.

⁂ All *Editorial Communications* are to be addressed to the Editor, and *Advertisements* and *Business Communications* to the Publisher, at the Office, 1, Wine Office Court, Fleet Street, London, E. C. *Private* letters for the Editor must be so marked.

⁂ In publishing letters from our correspondents we do not thereby adopt the views of the writers. Our intention to give both sides of a question will frequently oblige us to publish opinions with which we do not agree.

Articles by D. Forbes, F.R.S., T. Sherlock, and Dr. Phipson are in type, but are unavoidably postponed. Our report of last Monday's meeting of the Academy of Sciences arrived too late for insertion in this number.

F. S. J.—Evidently chalk and paraffin oil.

Studiosus.—You will be safe in following Dr. Mohr's directions. Dr. Hassall has written a work "On Urine," which will give you much information. You can also consult with advantage Lehmann's "Physiological Chemistry," Golding Bird's treatise "On Urinary Deposits," and Beale's treatise "On Urine, Urinary Deposits, and Calculi."

F. S. J.—To determine the sulphur in argillaceous iron ore, dissolve a weighed quantity of ore in nitro-hydrochloric acid, evaporate, etc., to separate the silica; dissolve the residue in acid; dilute, and precipitate the sulphuric acid with chloride of barium. The weight of sulphate of baryta obtained, multiplied by 0·13734, gives the sulphur.

X. Y. Z.—If you apply to our publisher, he will send the numbers you require, if they are in print. Occasional copies of our first and second volumes are to be met with; but they are worth far more than their publishing price.

A Miner.—It is not at all proved that our coal supply will run short for the next three or four hundred years, and in the meantime there will certainly be many discoveries made by which, if coal cannot be superseded altogether, its waste may be diminished. This is the most important problem; for nine-tenths of the coal raised is absolutely wasted, so far as the utilisation of the available force it is capable of exerting is concerned. Dr. Frankland, speaking of coal gas, says that physical science has yet scarcely attempted to estimate the true light-giving power of any sample of gas; but it can be proved, from the laws of conservation of force, that the light obtained by an argand burner is certainly less than the 1-265th of the light which could possibly be obtained from the same gas consumed at the same rate. Our problem now should be to try and get some of this enormously greater amount of light out of our gas. But we need not be dependent upon coal for gas. An ingenious Frenchman has lately propounded a brilliant idea. His theory (advanced through the medium of *La Gazette Médicale de Lyon*) is that all dead bodies of human beings are at present wasted, when they might as well be utilised by distillation into gas, to be used for illuminating purposes. He remarks:—"Coal is being exhausted, and since the human carcase is capable of supplying a gas of good illuminating power, why should it not be employed to this end? In India the idea is already realized. By a process of combustion in retorts a corpse of common dimensions may be made to yield twenty-five cubic metres of illuminating gas, which, at a cost of twenty-five centimes per cubic metre, would give a value of about eight francs for a body of ordinary size."

Communications have been received from C. Greville Williams; W. M. Bywater; H. Cross; J. W. Swindells; G. F. Bodwell; Sir B. C. Brodie, Bart.; S. Wilson; W. Odling; Rev. F. Souley Johnstone; S. Mordan; W. Briggs; C. H. Heaton; Dr. Adriani; T. A. Readwin; A. H. Church; T. Twining; Dr. W. S. Squire; A. C.; S. Meller; Runcorn Soap and Alkali Company; R. E. Bibbey; S. Muspratt, M.D.; Hadland and Co., with enclosure; E. Parnell; T. L. Phipson, Ph. D.; Cearnen; W. Lang; E. Stock; Joseph Thorley; Wm. L. Carpenter; Samuel Johnson; E. Church; Dr. Lish; G. Foord; P. Jessop; Dr. Price (with enclosure); W. Valentin; C. R. C. Tichborne; S. T. Tillman; Professor Joy; J. W. Swindells; G. Burditch; Johnson and Matthey; H. R. Marsden; Dr. W. T. Robertson; J. Bedford; R. H. Wilkinson; G. F. Rodwell; E. C. C. Stanford; Dr. W. Allen Miller; E. Church; Rev. J. L. Gordon; L. Mond; W. White; E. P. H. Vaughan; S. Hunt; S. Muspratt; H. L. Rayner; H. K. Bamber, F.C.S.; Geological Society; H. Sugg; C. R. Wright; R. Oxland; Mr. McDonnell; James Blackhouse; Gossage and Sons; A. E. Hawkes (with enclosure); W. Smith; Dr. H. Letheby; Charles Cochrane (with parcel); Henry Deane (with enclosure); John Wilkinson (with enclosure); J. Atkinson; Chas. U. Wright (with enclosure); Alex. Parkes (with enclosure); Dr. Odling; Rev. E. Smith; Dr. Odling; Johnson and Matthey (with enclosure); the Assistant-General Secretary to the British Association; Dr. Gladstone; F. Field; F. O. Ward; S. Mellor; Dr. R. Angus Smith (with enclosure); W. Schofield; J. Levincke; H. Walker; Dr. Letheby; H. Hofman; S. Baker; A. Watt; W. Millward; J.C. Todd; H. Henderson; E. Rothwell; Dr. A. Miller; John Bray, F.C.S.; Geological Society; H. H. Watson (with enclosure); Dr. Apjohn (with enclosure); Ritchie and Co.; Savory and Moore; Sir Henry James; Mr. Worthen; McDougall and Co.; Henry Hall; Sir B. C. Brodie, Bart.; John Pollard; August Stromeyer; Dr. Rohrig (with enclosure); Sir B. C. Brodie, Bart.; T. Sterry Hunt; E. Fairland, M.D.; Dr. S. Muspratt; S. Mellor; J. Culpar; Oates, Ingham, and Sons; Dr. Letheby; J. Morris, M.D.; F. Field (with enclosure); Avelino Aramayo; A. Sarle; F. Jerningham; C. Heisch; J. West; T. Grubb; J. Spiller (with enclosure); C. R. A. Wright, B. Sc. (with enclosure); J. Robbins; Thomas Reader; Dr. Frankland; Demuth and Co.; J. Turner; J. H. Blunt (with enclosure); T. B. Atkins (with enclosure); Runcorn Soap and Alkali Company; Thomas Hill; W. A. Johnson; J. Thudichum; Thomas Blair; R. Wilding; Joseph Davis; Dr. H. M. Noad; J. H. Swindells; E. P. H. Vaughan; Wm. Bywater; H. James; Jesse Fisher; Dr. Adriani; Dr. Rohrig; Capel H. Berger; Dr. R. Angus Smith, F.R.S.; C. R. C. Tichborne; Prof. Williamson, F.R.S.; Dr. Odling, F.R.S.; G. Gore, F.R.S; H. M. Jenkins; E. P. H. Vaughan; Dr. Lunge; C. Greville Williams, F.R.S.; J. Lawrence Smith (Vice-President of Jury, Group 4, Paris, 1867); Dr. Wm. Allen Miller, F.R.S.; the Board of Trade: Burnard, Lack, and Co. (with enclosure); May and Baker; J. S. Parker; J. Swain Window; John Parry; W. Browning; Samuel Collins.

Books Received.—"Gas Manipulation," by U. T. Sugg; "An Index to Mineralogy: being an Alphabetical List of about 2500 Minerals, with concise References to their Composition, Synonyms, and Place in the British Museum," by T. Allison Readwin, F.G.S.; "A Guide to the Chemical Department of the Museum of the Royal Agricultural College, Cirencester. Part I.—The Mineral Collection," by A. H. Church, M.A.; "Science made Easy," by Thomas Twining, Esq.; "New Theories of the Universe," by James Bedford, Ph. D.; "The Technologist," June; Transactions of the American Institute for 1865-6; "Photographs of Eminent Medical men," by W. T. Robertson, M.D., etc.; "Natural Philosophy," by C. Brooke, M.A., F.R.S., etc.; "The Mixture-book;" "Germinal Matter and the Contact Theory," by James Morris, M.D. Lond.; "Journal of Materia Medica," No. 5; "Chemical Physics," by Dr. Wm. Allen Miller, F.R.S; "Arithmetic Simplified for General Use," by Neil Arnott, M D., etc.

Ferrum.—Add a little caustic soda to the water, and you will find that the iron axles and shaft will not rust so readily.

Jehu.—Apply to Professor Tuson, at the Veterinary College, Camden Town. We cannot answer such questions.

S. K. Rawfolds.—Miller's "Chemistry," Bloxam's "Chemistry," Watts's "Dictionary of Chemistry," Fresenius' "Qualitative and Quantitative Analysis," and Sutton's "Volumetric Analysis," will form a good foundation for the chemical library of your Mechanics' Institute.

J. T. Gordon.—The name and address of the inventor of the ice-making machine are given in the article describing the machine. See page 29 of this volume, No. 372.

Fire-damp Indicator.—A few weeks ago the *Scientific American* advised a correspondent, who had applied for Mr. Ansell's address, to forward the letter to the "care of W. Crookes, England." Our American contemporary will be glad to know that the advice was taken, and was perfectly successful. The letter has been forwarded to Mr. Ansell.

Glycerin Soap.—We have received from the manager of Price's Patent Candle Company samples of a new preparation of toilet soap. It contains half its weight of pure distilled glycerin, and is apparently free from cocoanut oil or excess of alkali. The glycerin is solidified by dissolving in it an equal weight of fine toilet soap. This article, from its containing so large a proportion of glycerin, if exposed to the air, attracts moisture and becomes sticky. It has been found to lather and wear well, and has a fragrant perfume. The value of glycerin as a remedy for chapped or irritated skins is now well known.

Iron Eggs.—A correspondent draws our attention to the following statement in an American paper, and asks if any one can throw light upon the subject:—"Eggs with iron shells will be a fact at the Paris Exposition. A Berlin chemist caused his hens to produce them by feeding them on a preparation in which iron was made to take the place of lime." We should be sorry to throw doubts on the authenticity of the statement, but we pity the poor chickens! Probably, however, they were *canards*, not hens, which were experimented on. Would the iron eggs be thick enough to be used against armour plating? Boiled hard, they would serve as solid shot, and, if sucked and filled with powder, they would make capital shells, as effective against our iron-clads as any other foreign-made shells would probably be.

Errata.—Dr. Miller has called our attention to two misprints in our report of his first lecture. Page 260, twenty-two lines from bottom, *for* "magnesium," *read* "magnesia;" page 262, seventeen lines from bottom, *for* "light," *read* "eye."

Q.—Spelter is the commercial name for zinc.

F. Stephens.—Artificial ivory, for billiard-balls, has been made from a mixture of paper pulp, sulphate of baryta, and gelatine. They are said to be quite equal to ivory balls.

C. L.—Apply to Mr. Sutton, Eastern Counties Laboratory, Norwich. He will supply you with all the requisites for volumetric analysis.

Dr. Adriani wishes us to state that his last letter was forwarded, together with a former letter on the same subject, and was intended as a private communication. That being the case, Dr. Adriani should have given some intimation that his letter was not intended for publication.

A. B. C.—1. Amongst aqueous solutions having powerful affinity for oxygen, you may find the following useful:—A solution of pyrogallic acid in potash; an ammoniacal solution of subchloride of copper; a mixture of potash and solution of ferrous sulphate. You may, perhaps, find it useful to absorb oxygen by shaking together copper turnings and very dilute sulphuric acid. 2. The mixture explodes rather violently, and has more than once occasioned serious injury. 3. In taking photographs of the spectrum, we did not find that the point of maximum intensity of deposit was in the slightest degree influenced by the base of the bromide, iodide, etc., used in conjunction with the silver salt.

E. Fairland.—Speaking generally, it is correct to say that the heat from the sun will pass through glass, whilst the rays from a dark source of heat are intercepted. The reason for this is that glass is opaque to heat-rays of the refrangibility of those proceeding from a dark source of heat, whilst it is transparent to those coming from a body so hot as to be brightly luminous.

Physicus.—It is recorded that galena is slightly volatile in the vapour of water, at a high temperature. This might, therefore, account for what you have observed.

J. Bynner.—A gallon of water is required to be converted into steam to drive a railway train a little more than 100 feet. Each puff of steam ejects about half a pint.

F. Carte.—Phosphoric acid solution will take the enamel off a glazed iron dish, but it will not attack good Berlin porcelain.

Still another New Material for Gas Making.—*Apropos* to the suggestion we published recently for turning bodies into gas, a correspondent tells us that, according to a Swiss journal, a means has been discovered of utilising cockchafers. The *Estafette* of Lausanne states that between four and five millions of those insects were recently sent to Friburg for the manufacture of gas, and the residue was found to form an excellent carriage-grease.

J. G. T.—We think that the new iron and sulphuric acid pile of M. Monthier differs in some respect from Callan's battery, although it might be considered a "colourable imitation" of it.

Carbo.—Pure caramel may be obtained by placing a solution of crude caramel on a dialyser. The undecomposed sugar and some other matters pass through into the water, whilst the caramel remains behind. It forms when dry a black, highly lustrous mass, soluble in water when it has been evaporated without heat, but insoluble when it has been dried over a water bath.

W. S.—The process of depositing silver on glass, which we quoted in full in the review of Browning's "Plea for Reflectors," is the best for your purpose.

R. H. M.—You will find Dr. Roscoe's "Treatise on Chemistry" give you full information.

C. H.—The Act compels the condensation of 95 per cent. of muriatic acid gas, but in most works the condensation is more than 99·5 per cent. The average for this year has been 99·27 per cent.

May.—Cyanide of potassium will remove stains caused by nitrate of silver. Remember that it is very poisonous.

Physicus.—If matter be not infinitely divisible, it is, at all events, sufficiently so for all purposes of experiment, or even reasoning. Schiaparelli concludes that the *matter* in a sphere of meteoric cloud 100 miles in diameter weighs about 15 grains; if that be not infinite divisibility, it is a very near approach to it.

THE CHEMICAL NEWS.
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ON THE APPLICATION OF THE BLOWPIPE TO THE QUANTITATIVE DETERMINATION, OR ASSAY OF CERTAIN METALS.

BY DAVID FORBES, F.R.S., ETC.

(Continued from Vol. XV., p. 282.)

Silver Assay. Cupellation Loss.—This term is applied to indicate a minute loss of silver, unavoidably sustained in the process of cupellation, which arises from a small portion of that metal being mechanically carried along with the litharge into the body of the cupel. The amount of this loss increases with the quantity of lead present in the assay (whether contained originally in the assay or added subsequently for the purpose of slagging off the copper, etc.); it is relatively greater, as the silver globule is larger, but represents a larger percentage of the silver actually contained in the assay, in proportion as the silver globule obtained diminishes in size. It has, however, been experimentally proved that in assays of like richness in silver, this loss remains constant when the same temperature has been employed, and similar weights of lead been oxidised in the operation.

In the blowpipe assay this loss is not confined to the ultimate operation of cupellation, but occurs, though in a less degree, in the concentration of the silver-lead, and in the previous scorification of the assay, had such operation preceded the concentration. The total loss in the blowpipe assay is found, however, to be less than in the ordinary muffle assay, since in the latter case the whole of the oxidised lead is directly absorbed by the cupel.

In mercantile assays of ore it is not customary to pay attention to the cupellation loss, and the results are usually stated in the weight of silver actually obtained. Where, however, great accuracy is required, especially when the substances are very rich in silver, the cupellation loss is added to the weight of the silver globule obtained, in order to arrive at the true percentage.

The amount to be added for this purpose is shown in the annexed table, which is slightly modified from Plattner's.

The use of the table is best explained by an example, as the following:—An assay to which there had been added, in all, five times its weight of assay lead, gave a globule of silver equivalent to six per cent. Upon referring to the table, it will be seen that the cupellation loss for this would be 0·07; consequently the true percentage of silver contained in the assay would be 6·07. This table is only extended to whole numbers, but fractional parts can easily be calculated from the same.

When the globules of silver are so minute that they cannot be weighed, but must be measured upon the scale, the cupellation loss should not be added, since, as a rule, it would be less than the difference which might arise from errors of observation likely to occur when measuring their diameters upon the scale.

In the case of beginners, it will be found that the cupellation is usually carried on at too high a temperature, and that thereby a greater loss is occasioned than would be accounted for by the annexed table. After some trials the necessary experience will be acquired in keeping up the proper temperature at which this operation should be effected.

Actual percentage of silver found by assay.	Cupellation loss, or per-centage of Silver to be added to the actual per-centage found by assay in order to show the true per-centage of silver contained in the same. The entire amount of lead in or added to the assay being the following multiples of the original weight of assay:—									
	1	2	3	4	5	6	8	11	13	16
99·75 99·5 ….	0·25	0·32	0·39	0·45	0·50	—	—	—	—	—
90………	0·22	0·29	0·36	0·42	0·47	0·69	0·83	—	—	—
80………	0·20	0·26	0·33	0·39	0·44	0·64	0·75	—	—	—
70………	0·18	0·23	0·29	0·35	0·40	0·58	0·68	0·82	—	—
60………	0.16	0·20	0·26	0·30	0·36	0·52	0·61	0·74	—	—
50………	0·14	0·17	0·23	0·26	0·32	0·46	0·54	0·65	—	—
40………	0·12	0·15	0·20	0·22	0·27	0·39	0·46	0·55	0·62	—
35………	0·11	0·13	0·18	0·18	0·25	0·36	0·42	0·50	0·57	—
30………	0·10	0·12	0·16	0·16	0·22	0·32	0·38	0·45	0·51	—
25………	0·09	0·10	0·14	0·14	0·20	0·29	0·34	0·40	0·45	—
20………	0·08	0·09	0·12	0·12	0·17	0·25	0·29	0·35	0·39	0·45
15………	0·07	0·08	0·10	0·11	0·15	0·20	0·23	0·28	0·32	0·37
12………	0·06	0·07	0·09	0·10	0·13	0·17	0·19	0·23	0·26	0·32
10………	0·05	0·06	0·08	0·09	0·11	0·15	0·17	0·20	0·23	0·27
9………	0·04	0·05	0·07	0·08	0·10	0·14	0·16	0·18	0·21	0·25
8………	0·03	0·04	0·06	0·07	0·09	0·13	0·15	0·16	0·18	0·22
7………	0·02	0·03	0·05	0·06	0·08	0·12	0·13	0·14	0·16	0·20
6………	0·01	0·02	0·04	0·05	0·07	0·10	0·11	0·12	0·14	0·17
5………		0·01	0·03	0·04	0·06	0·09	0·10	0·11	0·12	0·14
4………			0·02	0·03	0·05	0·07	0·08	0·09	0·10	0·11
3………			0·01	0·02	0·04	0·05	5·06	0·07	0·08	0·09
2………				0·01	0·03	0·04	0·04	0·05	0·06	0·07
1………					0·01	0·03	0·03	0·04	0·04	0·05

Silver Assay.

It now becomes necessary to consider in detail the processes requisite for extracting the silver contents (in combination with lead) from the various silver ores, and other argentiferous compounds, which are met with in nature or produced in the arts.

In considering these, the following classification of the substances will be found convenient:—

I. METALLIC ALLOYS.

A. Capable of direct cupellation.

- *a.* Consisting chiefly of lead or bismuth: silver lead and argentiferous bismuth, native bismuthic silver.
- *b.* Consisting chiefly of silver: native silver, bar silver, test silver, precipitated silver, retorted silver amalgam, standard silver, alloys of silver with gold and copper.
- *c.* Consisting chiefly of copper: native copper, copper ingot, sheet or wire, cement copper, copper coins, copper-nickel alloys.

B. Incapable of direct cupellation.

- *a.* Containing much copper or nickel, with more or less sulphur, arsenic, zinc, etc.; unrefined or black copper, brass, German silver.
- *b.* Containing tin; argentiferous tin, bronze, bell metal, gun metal, bronze coinage.
- *c.* Containing antimony, tellurium, or zinc.
- *d.* Containing mercury: amalgams.
- *e.* Containing much iron: argentiferous steel, bears from smelting furnaces.

II. MINERALISED COMPOUNDS.

- *a.* Silver and other ores, furnace products, sweeps, and products of the arts containing sulphides, arsenides, and other compounds of the metals in combination with more or less earthy matter.
- *b.* Argentiferous sulphide of molybdenum.

c. Substances nearly free from sulphides or arsenides, but containing chlorine, iodine, or bromine.

d. Argentiferous litharge, and other easily reducible oxides.

I. A. Metallic Alloys capable of direct Cupellation.

a. Consisting chiefly of Lead or Bismuth.—In determining the silver contained in these alloys, it is only requisite to place a clean piece of the same, weighing about from one to ten grains according to its probable richness in silver, upon a cupel of coarse bone ash, and proceed by concentration and cupellation exactly as has been already described under these heads.

Should the substance be not altogether metallic, or not free from adherent slag, earthy matter, or other extraneous matter, it should previously be fused on charcoal with a little borax in the reducing blowpipe flame, and the clean metallic globule then removed from the charcoal, and treated as before. In order to remove the globule from the inherent borax-glass, it may be allowed to cool, and then detached; or, after a little practice, it will be found easy, by a quick movement of the charcoal, to cause the globule still melted to detach itself completely, and drop on the anvil in the form of a single somewhat flattened globule, without suffering any loss of lead adhering to the charcoal.

In the case of argentiferous bismuth alloys the process is carried on in all respects the same as if silver-lead were being treated. As, however, the bismuth globule is very brittle, care must be taken when separating the concentrated globule from the litharge, as, if not carefully done, a loss may easily be sustained from a portion of the globule remaining behind adherent to the litharge. It is better, therefore, to remove the litharge by degrees from the globule with the aid of the forceps.

Argentiferous bismuth, free from lead, when cupelled alone, invariably leaves a globule of silver, having a dull frosted surface. If, however, at the end of the operation a small quantity of lead (¼ to ⅓ a grain) be added, and fused along with it, the silver globule then obtained will be perfectly bright and free from all bismuth.

In the case of native bismuthic silver it is advisable to fuse the previously weighed mineral with a little lead and borax-glass on charcoal in the reducing flame, so as to free it from any adherent earthy matter, and then proceed by concentration and cupellation, as before described.

Silver Assay.

I. A. Metallic Alloys capable of direct Cupellation.

b. Consisting chiefly of silver: native silver, bar, test, and precipitated silver, retorted silver amalgam, standard silver, silver coin, and other alloys of silver with gold and copper.

These alloys may be at once fused with lead on the cupel itself, and the operation finished as before described. In general, however, it is better to fuse the weighed assay previously with the requisite amount of pure lead and a little borax-glass, say from a quarter to half the weight of assay, in the reducing flame at a low heat on charcoal until the globule commences to rotate. This ensures having a perfectly clean button of silver-lead, which is then cupelled in the ordinary manner.

In most cases the quantity of lead to be added need not exceed that of the weight of the alloy, but when several percentages of copper are present in the assay, as in case of many coins, etc., the lead should be increased to some three, or even five times the weight of the assay in proportion to the amount of copper actually contained in the substance under examination, and which will be treated of more at length under the head of copper-silver alloys.

When no more lead has been added to the assay than its own weight, the cupellation may be concluded in one operation by inclining the stand, and so moving the globule on to a clean part of the cupel; but when more copper is present, it is preferable to concentrate first and cupel subsequently, in order thereby to reduce the cupellation loss to its minimum.

In the concentration as much copper as possible should be slagged off with the lead, which is effected by inclining the cupel somewhat more than usual, so that its surface may be less covered up with the litharge and exposed as much as possible to oxidation, by which means the litharge, as it forms, is enabled to carry off more of the copper contained in the silver lead.

Should the silver globule after cupellation show indications of still containing copper, as before noticed, when treating of cupellation, a small quantity of lead must be fused along with it, and the cupellation finished as usual.

As at the present time no means are known by which silver can be separated from gold by the use of the blowpipe, in all cases of alloys containing gold, this metal remains to the last along with the silver, and the result in such cases always indicates the combined weight of both these metals contained in the alloy under examination. The employment of the humid assay must be resorted to for effecting their separation:—

c. Containing chiefly copper: native copper, ingot, wire, or sheet copper, cement copper, copper coins, copper-nickel alloys.

Under the most favourable conditions in cupellation, the amount of lead requisite when converted into litharge to slag off one part of copper along with it as oxide, amounts to between seventeen and eighteen parts its weight. In the blowpipe assay it is usual to add to any cupriferous alloy an amount of pure lead equal to twenty times the amount of copper contained in the alloy, in order to ensure the whole of the copper being separated in the litharge. In the case of nickel the amount of lead required is somewhat less than with copper, but in practice the same amount of lead may be employed.

When the copper is quite clean the requisite amount of lead may be added to it in a single piece on the cupel, fused and cupelled as usual, after previous concentration of the silver-lead to a small-sized globule.

It is generally found, however, that traces of iron, slag, gangue, or other foreign matter is present; and, consequently, it is usually advisable to fuse the assay along with the requisite amount of lead, and about one-half its own weight of borax-glass in the reducing flame, until the whole of the substance is seen to have perfectly combined or alloyed with the lead, and the globule has come into brisk rotation, whilst at the same time no detached metallic globules are seen in the borax-glass.

The concentration of the silver-lead and cupellation are then conducted as usual, taking care when concentrating to incline the cupel-stand so as to expose as much of the metallic surface of the melted globule to the oxidising action of the air as possible, with a view of enabling the litharge whilst forming to carry off as much copper along with it as possible.

Should the silver globule obtained after cupellation spread out, or appear to the eye more flattened than usual with globules of pure silver, it indicates that some copper still remains, and a small piece of assay lead (½ to 1 grain weight) should be placed alongside it whilst still on the cupel, fused together, and the cupellation finished on a clean part of the same cupel as usual.

Precipitated or cement copper, especially that which is in the crude state, and has not been melted and run into ingots, is often very impure, containing so much iron, lead, arsenic, earthy matter, etc., as not to admit of direct cupellation, and in such case should be treated as pertaining to class B. *a.*:—

B. Metallic Alloys incapable of direct Cupellation.

a Containing much copper or nickel, with frequently some little sulphur, arsenic, zinc, iron, cobalt, etc., as unrefined or black copper, brass, German silver, etc.

As the presence of these extraneous matters would interfere with the cupellation either by causing a loss of silver-lead projected from the cupel upon the evolution of the volatile substances present, or by forming oxides which could not be absorbed by the cupel, it is necessary to eliminate such substances by a scorification with borax on charcoal previous to concentration or cupellation.

In the case of unrefined and black copper, the portion used in the examination is placed in the scoop with twenty times its weight of assay lead, and its own weight of powdered borax-glass, mixed with the spatula, and transferred to a soda-paper cornette. It is then fused on charcoal in the reducing flame, which should be constant and uninterrupted, until all particles have completely united, and a brisk rotation sets in, which is kept up for a short time, when the silver-lead globule, which should appear bright on the surface after cooling, is concentrated and cupelled precisely as is directed under A. *c*. By this preliminary scorification the sulphur, arsenic, and zinc are volatilised, and any lead, cobalt, or iron slagged off into the borax-glass.

In the assay of brass and German silver the quantity employed is fluxed with its own weight of borax-glass, but only requires ten times its weight of assay lead. The operation is commenced as before, but the globule is kept somewhat longer in rotation (always keeping the flame directed only on to the borax-glass), so as to allow the zinc present to be completely volatilised, which is evident when the surface of the silver-lead becomes bright, on which the heat is increased for a few moments to expel the last traces of that metal, and the silver lead thus obtained is concentrated and cupelled as before.

The silver globule obtained from the cupellation of substances rich in copper generally requires the addition of a small quantity of lead and re-cupellation (as before described), in order to ensure its freedom from copper.

Behaviour of Lime when Burned.—Dorlhar and Saminn. Two cylinders rormed out of the same piece of limestone measured 27 millimètres in length and 17 millimètres in diameter. After being completely burned their volume had increased nearly 1-10th—viz., to 28 millim. and 17·7 millim.—*Berg. und huttimm, Zeitung*, 1867.

NOTE ON THE CALCULUS OF CHEMICAL OPERATIONS.

BY PROFESSOR WILLIAMSON, F.R.S.

The remarkable memoir of Sir Benjamin Brodie, respecting which these remarks are made, is the first consistent attempt to introduce analytical reasoning into the body of the science of chemistry.

One fundamentally important question of method is raised by the memoir; and as it may be considered apart from the rest of the subject, and is, in fact, a preliminary to any discussion upon it, the author wishes to draw attention to some considerations relating to it.

Sir B. Brodie defines a chemical operation as an operation performed upon the unit of space, of which the result is a weight. The unit of matter (or molecule) adopted is the weight of matter of a specified kind, which occupies in the state of perfect gas the volume of one litre at 0°C. and a pressure of 760 millimetres of mercury.

This absolute definition is intended to supersede the prevailing theory that the molecule of each compound is the smallest proportional weight in which we can, consistently with its other properties, represent it as taking part in any reaction, or in which we can suppose it to exist by itself.

In some cases the vapour-densities of many compounds have confirmed the molecular weights assigned to them by a comparison of their reactions; but in other cases, many of which are too familiar to need mention here, the vapour-density contradicts the above evidence of the molecular weight. What is the result in such a case of conflict? Uniformly this: that if the vapour-density and reactions are irreconcileable, we know that the vapour-density must have given wrong advice, and it only remains to be seen by an examination of the anomalous vapour how the molecule broke up on evaporation.

Perhaps the best way to judge of the working of the new definition is to see the manner in which Sir B. Brodie himself applies his principle. Thus, at page 817 of his memoir, the units of thirteen substances are given, and opposite each formula is given the "absolute weight in grammes" of a litre of the vapour, and in another column the "relative weights" of each. Of these fundamental statements four only—viz.: the numbers for sulphur, sulphuretted hydrogen, sulphurous acid, SO_2, and sulphuric acid, SO_3, are the records of observations. The numbers for three other substances are at variance with observation, for SO_3H_2 breaks up on evaporation into SO_2 and H_2O, forming a mixed vapour of about half the density given. SO_4H_2 breaks up similarly, forming a vapour of about half the specific gravity assumed, and Nordhausen acid first breaks up into SO_3 and SO_4H_2, and this hydrate decomposes at a higher temperature, as above mentioned. The vapour from Nordhausen acid has, therefore, a specific gravity vastly below that assumed. No doubt there are good reasons derived from a study of other facts for believing that these three compounds, *if they were capable of evaporating undecomposed*, would have the vapour densities assigned to them by Sir B. Brodie; but taking the simple definition as given, we are led to molecular weights, which the author, in common with all chemists, considers inadmissible, and which he very properly corrects.

The other six substances of the table are more unstable compounds, which have never been evaporated without even more permanent and fundamental changes in their composition. It would be easy to multiply instances, but enough has been said to illustrate the fact that molecular formulæ are not deduced from mere vapour densities, and that we should only be justified in taking a fixed vapour volume, as the definition of molecules, if we could show that from the vapour densities of compounds their molecular weights could be inferred with certainty.

There are strong reasons against the supposition that the molecules of chemistry can all, or nearly all, exist in the state of vapour. If we were to speculate upon the probable distribution of the elements at a temperature sufficiently high for the volatilisation of all known substances, it would be safe to assume that a very great if not the greatest, number of known molecules would be resolved into others containing a smaller number of atoms.

Numberless known molecules are notoriously broken up at temperatures within easy reach of every operator, and there is reason to anticipate that the progress of research will gradually give us more insight into the conditions of condensation which distinguish volatile from non-volatile molecules, so as to explain why the latter cannot exist in the gaseous state.

It is, moreover, not only undesirable on the ground of accuracy, but also exceedingly inconvenient, to define chemical molecules as being one litre of vapour, for the artifice necessitates the establishment of a new scale of atomic weights and molecular weights.

The molecule of hydrogen in this scale weighs				0·089
"	"	"	oxygen	1·430
"	"	"	nitrogen	1·251
"	"	"	chlorine	3·173
"	"	"	steam	0·805
"	"	"	ammonia	0·760
"	"	"	hydrochloric acid	1·631

The numbers represent in grammes the weight of a litre of the respective substances.

If we retain the ordinary molecular weight—viz.:

Hydrogen	2	=	H_2
Oxygen	32	=	O_2
Nitrogen	28	=	N_2
Steam	18	=	H_2O
Ammonia	17	=	NH_3
Hydrochloric acid	73	=	ClH

we have numbers which are easily remembered and easily used, and which answer all the purposes of the other numbers, in a far more convenient manner. Relations between these short numbers are seen at a glance, and computations are rapidly made with them in the head, which, with the numbers of the *crith* series of the litre, are comparatively slow and difficult. Calculations relating to any absolute measure are most easily made upon the basis of the common molecular weight. One constant has to be used for the reduction in absolute volume—viz.: 11·2 litres, or the volume, at the normal temperature and pressure, of one gramme of hydrogen. Every formula of a volatile molecule represents 22·4 litres of the vapour weighing as many grammes as there are units in the molecular weight.

Some interesting considerations are suggested by the proposal to consider the molecule of hydrogen as containing only one atom, and the molecules of chlorine, bromine, iodine, nitrogen, phosphorus, arsenic, etc., as each compounded of one atom of hydrogen with two atoms of elements at present unknown. The formula for hydrochloric acid is $\alpha\chi$, which we may define, $\alpha\chi = \mathrm{HCl}$, and this combined with the fundamental assumption $\alpha = \mathrm{H}_2$, gives us

$\mathrm{H}_2\chi = \mathrm{HCl}$, whence $\chi = \frac{\mathrm{Cl}}{\mathrm{H}}$. In like manner the symbol of chlorine, $\alpha\chi_2$, may be translated into ordinary symbols by the aid of the equation $\alpha\chi_2 = \mathrm{Cl}_2$, in which we replace α and χ by their values as above.

$$\mathrm{Cl}_2 = \mathrm{H}_2.\frac{\mathrm{Cl}_2}{\mathrm{H}_2}$$

In explanation of the symbol $\frac{\mathrm{Cl}}{\mathrm{H}}$ it is merely stated that it represents an atom of chlorine from which an atom of hydrogen is removed. Chemists employ the sign of multiplication to denote combination, as in HCl, and the sign of division is here employed to indicate decomposition.

The assumption that two molecules of hydrochloric acid decompose so as to form a single atom of hydrogen necessarily involves for a molecule of the acid a formula like $\alpha\chi$, representing it as one atom of hydrogen to which another atom $\frac{\mathrm{Cl}}{\mathrm{H}} = 34{\cdot}5$ is united.

$\mathrm{H}_2\frac{\mathrm{Cl}}{\mathrm{H}}$ is the translation of the formula of hydrochloric acid; and when two such molecules are decomposed into hydrogen and chlorine, the process consists in removing H_2 from one of them, and combining the residue $\frac{\mathrm{Cl}}{\mathrm{H}}$ with the other molecule of acid, forming $\mathrm{H}_2\frac{\mathrm{Cl}_2}{\mathrm{H}_2}$

In like manner hydrobromic acid is described by the symbol $\mathrm{H}_2.\frac{\mathrm{Br}}{\mathrm{H}}$.

Bromine ..... $= \mathrm{H}_2.\frac{\mathrm{Br}_2}{\mathrm{H}_2}$

Hydriodic acid $= \mathrm{H}_2.\frac{\mathrm{I}}{\mathrm{H}}$

Iodine........ $= \mathrm{H}_2.\frac{\mathrm{I}_2}{\mathrm{H}_2}$

Ammonia ..... $= \mathrm{H}_4.\frac{\mathrm{N}}{\mathrm{H}}$

Nitrogen...... $= \mathrm{H}_2.\frac{\mathrm{N}_2}{\mathrm{H}_2}$

The author is inclined to think that the hypothesis adopted respecting the constitution of chlorine, nitrogen, etc., might, with advantage, be extended to oxygen and other atoms of even equivalence, if adopted at all, so as to represent those elements as containing hydrogen. Oxygen can be supposed to be built up from water by the addition of an increment of weight, analogous to the increment which transforms hydrochloric acid into chlorine. Water would thus be represented by a symbol of the usual form (say $\alpha_2\phi$), and the decomposition of the unit of water into two units of hydrogen and one unit of oxygen would be represented in a manner analogous to that adopted in the case of hydrochloric acid. One unit of water would lose ϕ, liberating two units of hydrogen, which ϕ would enter into combination with the other unit of water, thereby forming $\alpha_2\phi_2$. $\alpha_2\phi_2$ is thus the symbol of oxygen, and ϕ is an element which is contained in oxygen, and which leaves it to unite with two atoms of hydrogen.

Let α represent	2	grammes of hydrogen	=	22·4	litres.
$\alpha_2\phi_2$ "	32	grammes of oxygen..	=	22·4	"
$\alpha_2\phi$ "	18	grammes of steam ..	=	22·4	"

Then ϕ is the symbol of an imaginary element, representing the increment to be added to 4 grammes of hydrogen, when they are converted into 18 grammes of water. The weight of ϕ is, therefore, 14. It may be translated thus:

$$\phi = \frac{O}{H}, \text{ and if } a = H_2, \text{ we have } 2\ H_2 + H_4\frac{O_2}{H_4} = 2H_4\frac{O}{H_2}.$$

The following equation represents the process:

$$2a + a_2\phi_2 = 2a_2\phi.$$

It is interesting and important to observe that Sir B. Brodie's fundamental hypothesis is an atomic hypothesis, viz., that the molecule of hydrogen consists of one atom instead of containing two atoms, as we have been accustomed to see it; and that, in constructing each molecular formula, he limits himself to some integral number of atomic weights.

In weighing the arguments which will, no doubt, be brought forward in favour of the proposed change, chemists will need to compare the whole of the new system to the old one. Such comparison can only be made when the new systems are before us. The author hopes, before long, to bring forward various considerations relating to the interpretation and application of our present system of chemical facts and theories. He wishes now to point out that the proposed system would be at an unnecessary disadvantage in the comparison or contest, if it were left to rest on the quicksands of conjecture respecting vapour densities.

CHEMICAL COMPOSITION OF STREET MUD.

BY C. R. C. TICHBORNE, F.C.S.

The communication on the above subject, from Dr. Letheby, which appeared in the last number of the CHEMICAL NEWS, possesses to my mind considerable interest from two causes. First—from its important sanitary aspect, and second—from the fact that last year, I endeavoured to bring prominently before the authorities of the city of Dublin, the highly deleterious nature of street dust, or mud, which from their hygienic point of view was vulgarly looked upon as harmless. In a letter published in the *Irish Times*, Tuesday, October 9, 1866, I gave the analysis of the mud taken from one of our narrowest streets, but which street was, and is to the present day, our greatest thoroughfare. The letter was written at the commencement of the severe attack of cholera with which Dublin was visited last autumn, and was published mainly with a view to advocate the use of carbolic acid for watering the roads. On referring to my notes, I find that the street dust in Dublin contained on an average 24 per cent. of organic matter, which is lower than the average given by Dr. Letheby for the London mud, even allowing for difference of moisture. This lower percentage is probably owing to the much larger percentage of animals working upon the same extent of roads, or also because many of our streets are macadamized, or constructed in a similar manner. From the continual state of steeping (if I may use the expression), and re-drying that this comminuted manure is constantly undergoing, I am of opinion that we are hardly alive to the mischief that it is capable of perpetuating. Antiseptics, such as carbolic acid, from their expense, are inadmissible for general use in the ordinary course of events. In seaport towns an efficient antiseptic, and harmless friend, would be found in the sea-water. This should be used freely for watering the streets. In dry and hot weather tbe saline substance very soon accumulates, and the sea-water leaves a perceptible hard crust of the briny matter. From its slightly deliquescent nature it answers its mechanical requirements admirably, and during the usual dry weather adds no expense to the work ordinarily done to restrain the dust. The three and a half per cent. of chloride of sodium, etc., contained in sea-water collects rapidly when exposed to superficial evaporation, even in damp weather, and it would be next to impossible that fermentable changes could take place in the presence of so large a proportion of sea-salts.

Such a project would perhaps be hardly feasible in a city like London, which is situated so far from the mouth of the river, but it could be easily adopted in in such towns as Liverpool, Portsmouth, Dublin, etc. The following analyses taken from my note-book of last year, may possess some interest in connection with my remarks.

Moist Dust from Grafton-street, Dublin, October, 1866.

Moisture	33·3
Organic matter	25·1
Inorganic matter	41·6
	100·0

Street Dust, October, 1866.

Soluble salts	1·3 per cent.
Organic matter	25.1

Soil from a well made road upon which sea-water had been used.

Soluble salts	7.5 per cent.
Organic matter	21·1

Here it will be seen that the salts are about ⅓ the weight af the total organic matters present.

ANALYSIS OF TINKALCITE—$NaO,2BO_3+2(CaO,2BO_3)+18HO$*—DETECTION OF BORON AND FLUORINE IN MINERALS

BY PROFESSOR F. WÖHLER.

After estimating the water of crystallisation, dissolve the mineral in hydrochloric acid, and after neutralising with ammonia, precipitate the lime with oxalate of ammonia. Concentrate the filtrate, and estimate the boracic acid in the state of double fluoride of boron and potassium.

To estimate the soda dissolve another portion of the mineral, precipitate the lime as above with oxalate of ammonia, evaporate the filtrate to dryness, and drive off the ammoniacal salt by heat. Then digest with concentrated hydrofluoric acid, evaporate to dryness, and digest with strong sulphuric acid; all the boracic acid will be carried away in this operation in the form of gaseous fluoride of boron. The sulphate of soda may then be heated to redness in a crucible with a fragment of carbonate of soda.

There are some minerals which contain a rather large quantity of fluorine, silica, alumina and alkalies (principally potash): some also contain soda and lithia, micas for example. Micas being sufficiently heated to disengage fluorine, it must be noticed if they only give off fluoride of silicium, since they contain considerable quantities of silica; the coloration of the blowpipe flame, however, will suffice to indicate the volatilisation

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* O—8.

of alkaline fluorides. If this occurs, the best plan to adopt is to perform the experiment just described, replacing the lime by silica; all the fluoride of silicium traverses the silica undecomposed, but the alkaline fluorides are arrested and changed into fluoride of silicium, which is evolved, and into silicates of potash, soda, and lithia, which remain in contact with an excess of silica. Take up the silica by distilled hydrofluoric acid; this will change it into fluoride of silicium, whilst alkaline fluorides remain behind. The alkaline fluorides are treated with sulphuric acid, and changed into sulphates of potash, soda, or lithia, and in this mixture the base is to be sought for.

When the object is therefore to determine the fluorine, one can always estimate the water and the fluoride of silicium alone or mixed.

In the case of fluoride of boron the question is not so simple; on heating, some is disengaged from tourmalines, which contain fluorine and boron. The volatile matters contained in a tourmaline may be determined by a process analogous to that employed in the case of fluoride of silicium; the fluoride of boron being changed into a mixture of fluoride of calcium and boracic acid. Unfortunately we do not know a method of estimating boron in mineral substances, especially if associated with fluorine and silicium; so that if we have fluoride of boron, fluoride of silicium, and alkaline fluorides, we can estimate the alkalies, but not the boron, silicium, and fluorine. In the absence of a quantitative method for estimating fluorine, boron, and silicium, we have an excessively delicate qualitative test. The best method of recognising the presence of fluorine consists in mixing the substance with potassic bisulphate, grinding the whole together in a small mortar, and introducing a small quantity of the paste slightly moistened into a glass tube, open at each end; place the substance at the lower part of the tube, and direct a blowpipe flame on to it so as to heat the fluoride.

Under the combined influence of the water in the bisulphate of potash, and that resulting from the combustion of the gas, there are formed alkaline sulphates, with disengagement of hydrofluoric acid. It is true that if there is a sufficient quantity of silica, fluoride of silicium may be formed, but the result will be the same. This gas condenses with the globules of water a little beyond the part heated; if hydrofluoric acid has been formed the glass is attacked, and the same effect is produced if fluoride of silicium has been formed; in the latter case this gas changes in contact with the aqueous vapour into hydrofluosilicic acid and silica; if then the drop of water condensed with the acid is heated, the latter attacks the glass and becomes changed into fluoride of silicium. The glass tube should have been washed and dried carefully, and ought to be transparent; the same precautions must be taken after the operation.

In the case of boron the process is quite different. If no fluorine is present, mix the substance with a small quantity of fluoride of calcium and bisulphate of potash, having previously ascertained that the two reagents do not contain boron; two experiments must therefore be made, one on the reagents, and the other on the substance mixed with the reagents. The mixture is slightly moistened, and held on the extremity of a perfectly clean platinum wire. Direct the reducing flame of the blowpipe on to the paste: at the moment when the mixture appears to boil the flame assumes a vivid green colour, characteristic of boron. When but little boron is present this must not be tried in full daylight, and it should be viewed against a dead black background; the colour of the flame will then be easily detected.

VAPOUR DENSITY OF WATER.

The following remarks, by Mr. F. O. Ward, occur in a private letter recently addressed by that gentleman to a chemical friend. It will be perceived, by the familiarity of the style, and the cursory character of the statements, that this passage was not intended for publication. It contains, however, several indications of interest bearing upon theoretical questions which are at present strongly attracting the attention of chemical philosophers; and we believe that, in submitting it to our readers, we shall not unpardonably overstep the limits of editorial discretion.

"Can you tell me on whose authority rests the assertion that water expands 1,696-fold in becoming steam at 212° F.? It is repeated from book to book, and I have repeated it myself in print (in one of the chapters of Hofmann's "Introduction to Chemistry.") It does not, however, by my reckoning, tally with the vapour-density of water, for

	Gramme.
2 litres of H at 0·0896=..........	0·1792
1 litre of O at 16 × 0·0896=.......	14·3360
	14·5152

The 3 litres being condensed into 2 we have.......................... $\frac{14·5152}{2}=7·2576$

as the vapour density of steam at ordinary pressure and temperature. A litre of water weighs 1000 grammes, and

$\frac{1000}{7·2576}=1377·8$ for the expansion. Reducing the alleged 1,696 for temperature (by $\frac{1}{460}$ per degree F.) from boiling point to 60°=152° we have the ratio

$$\frac{208}{460} \times 1696 = 1266$$ as the expansion

so deduced, against....... .1377·8 as shown above.

Difference.................110·8 (over 8 per cent.)

This is a large discrepancy, and (unless I blunder in my reckoning) it shows error in one or other of the figures compared, which ought not, therefore, *both* to appear (as they do, the 1696 in so many figures, the other *implicitly*,) in the same books (*vide passim* "Fownes' Chemistry," the only one I have with me in my trunk). One question this suggests to my mind is, whether we are not going a little too fast in accepting, as we are all disposed to do, the volumetric relations of bodies as so perfectly symmetrical? Just so, some years back, we were all seduced into the pretty belief that the ponderal relations of bodies were in Prout's simple multiple ratios. Stas stemmed and turned back that false current of chemical philosophy, and we may want a Stas to keep our volumetric opinions in the path of truth!

"The differences as to volume-ratios imputed *en masse* to 'errors of observation,' may, very possibly, have in some cases, an *actual existence*.

"It may well be, indeed, that as, in music, certain minute secondary vibrations give to musical tones their vowel modifications and all the admirable varieties of *timbre* which make up our orchestral wealth; so the small deviations from symmetry in the ponderal and volumetric relations of matter, may be the very conditions of the infinite variety we observe in the modulations of chemical phenomena in those unnumbered surprises of harmony and discord to which we owe the richness and beauty of our chemical orchestra."

F. O. W.

ON A POSSIBLE CAUSE OF VARIATION IN THE WEIGHTS, ATOMIC AND OTHERWISE, OF ELEMENTS AND COMPOUNDS.

BY JOHN A. R. NEWLANDS, F.C.S.

M. Stas has lately remarked that "hitherto nothing has proved that the differences found in certain analyses between experiment and calculation must be *wholly* owing to error in the operation; a certain part may be due to the inexactitude of the law of definite proportions."* It is open for us then to inquire whether any cause exists which should induce the atomic weights of two or more elements to vary in different compounds, or which should make the atomic weight of a compound greater or less than the sum of the atomic weights of its constituents.

If the attraction of gravitation were the sole force concerned in the question, and if the force of gravity be assumed to be always the same for all bodies and at all temperatures, it would seem reasonable enough to believe that the atomic weight of each element would be expressible by an absolutely invariable number, and that the atomic weights of compounds would be absolutely the sum of the atomic weights of their constituents. I of course take it for granted, in this case, that the weighings should be performed in vacuo, and neglect the almost infinitesimal error which might possibly be caused by the amount of the ether of space, displaced by a given weight of matter, being greater in certain forms of combination than in others.

It must not be forgotten, however, that we are living on the surface of an immense magnet, and that all, or almost all, the constituents of the earth's surface are capable of being either attracted or apparently repelled by a magnet with a force which varies with the temperature, the state of combination, etc.; the magnetic attraction of the earth being most powerfully manifested at its two colder points, viz., the two poles.

Among the theoretical consequences of this state of things the following may be mentioned:—1st. A given weight of a magnetic substance would weigh more the lower the temperature, being under the influence of an increasing attraction. Thus, the oxygen of the atmosphere would, during the winter, be drawn to the earth with more force than during the summer; and also, as a general rule, be attracted more in the night than in the day.

2nd. A given quantity of an element would weigh more when in the state in which it was attracted by the magnet than it would in another state. Thus, a given quantity of iron would weigh more in the state of black oxide than in the state of potassium ferrocyanide. The weight of a compound might, therefore, be less than the sum of the weights of its constituents; thus, the weight of ferric oxide, which is but slightly magnetic, would be less than the sum total of the weights of the iron and oxygen it contains, weighed separately, both of these being highly magnetic. By the reverse of this operation, it is possible that the weight of some compounds of certain elements, apparently repelled by the magnet, such as bismuth and thallium, might be greater than the sum of the weights of their constituents.

3rd. The magnetic attraction of the oxygen of the air above any substance might possibly reduce or augment its weight, according as it might be of a magnetic or diamagnetic character.

4th. A portion of the increase of weight which takes place when a given quantity of matter is transferred from the neighbourhood of the earth's equator to that of its poles, may be due to the increased magnetic attraction of the latter. If such be not the case, we should find the gain in weight of iron, under these circumstances, to be no greater than the gain in weight of bismuth.

5th. Admitting the existence of a variety of small bodies, or fragments of planets, meteorites, etc., of all kinds of composition, travelling through space, and approaching from time to time the earth's path in their respective orbits, we should expect to find if they were drawn to the earth by the action of gravity alone, pure and simple, that the substances so falling would be of all shades of composition. On the contrary, if they were drawn to the earth by virtue of its magnetic powers, we should find that the substances so falling would consist entirely, or at least partially, of bodies of a highly magnetic character, such as iron and nickel, and never of diamagnetic substances, such as bismuth and antimony. This latter view is corroborated, to a great extent, by the analysis of meteorites. Those who maintain that the fall of meteorites is due to gravity alone, must also believe that the proportions of iron and nickel to other kinds of matter in surrounding space is far greater than we know it to be on the earth's crust, and that the earth attracts only iron and nickel because it has nothing else to attract. The presence of hydrogen gas in some specimens of meteoric iron may be explained by supposing that the iron in question originally contained within its substance a minute amount of water, and that as it became heated by passing into the earth's atmosphere, this water was decomposed. The oxygen, under these circumstances, would unite with the iron to form black oxide of iron, and the hydrogen thus set free would be retained within the pores of the softened iron in a highly compressed state, as was the case with that found by Graham in the meteoric iron of Lenarto.

If the amount of hydrogen so liberated were very great, the heated mass would explode and split up into a number of smaller meteorites. This explosion of meteorites has also been observed.

6th. As far as the magnetic attraction of the earth is concerned, the fall of meteorites would occur mostly in the vicinity of the north and south poles. A large quantity of finely divided meteoric dust descending in this manner into the earth's atmosphere, and becoming ignited therein, might contribute to the phenomena of aurora borealis and australis.

7th. If the magnetic intensity of the earth be liable to secular variation, we should find the ordinary weights, and also the atomic weights, of substances to vary likewise.

8th. If the real atomic weights of the elements, after eliminating the magnetic action of the earth, could be obtained, we should probably be able to observe numerical relations between them of a simpler character

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* Chemical News, June 14th, 1867.

than can at present be found. They might then prove to be multiples of a unit, without any fractions whatever, in accordance with the idea of Prout, as modified by M. Dumas.

9th. The length of a pendulum vibrating seconds would be different if constructed of a magnetic or of a diamagnetic substance.

ON THE ACTION OF

NITROGEN ON THE SILICIDES OF MAGNESIUM AND CALCIUM,

AND ON A NEW DEGREE OF OXIDATION OF SILICIUM.

BY M. A. GAUTHER.

Silicium possesses but a small affinity for nitrogen, which affinity only becomes manifest near its fusing point. The author considered that the combination might be more easily effected by making nitrogen act on silicide of calcium, and especially on silicide of magnesium, magnesium having itself a strong affinity for nitrogen.

Wöhler's silicide of calcium, heated to a red white heat in a current of nitrogen, increased 5·2 per cent. in weight; but the surface only of the silicide was attacked, a small quantity of nitride of calcium was formed, and the silicium separated. The crystallised silicides of magnesium, when treated in the same way, gives a black mass, which behaves like a mixture of silicide and nitride of magnesium; by the action of water it gives ammonia and magnesia, which dissolves on addition of hydrochloric acid, leaving the silicium pure; thus again, the nitrogen was only fused on the metal.

The author prepared silicide of magnesium by the action of magnesium on fluosilicate of sodium. Place at the bottom of a Hessian crucible a layer of chloride of sodium, melted and pulverised; then a mixture of 7 grammes of fluosilicate of sodium with 2½ grammes of chloride of sodium, and over this mixture 2½ grammes of magnesium in lumps. Cover the whole with melted chloride of sodium, heat the crucible quickly in a good wind furnace; when the reaction, which is lively, abates, leave the crucible in the fire five minutes, then take it away and stir the mass with a clay spatula, cover the crucible, and let it cool. Whilst the crucible is open, part of the magnesium burns in the air, giving magnesia and nitride of magnesium; the metallic button contained in the crucible is more or less rich in silicide of magnesium; when it contains an excess of magnesium, which occurs when the temperature is raised too slowly, this button may be considered as magnesium, and submitted to a new operation. First treat it with water to remove the dross, and then with a cold dilute solution of sal ammoniac, which dissolves the magnesium. Metallic crystals are easily obtained, and the silica may be mechanically separated by rubbing: they generally represent 10 per cent. of the weight of the magnesium used: they are leaden grey, and seem to be regular octahedra. A hot solution of sal ammoniac attacks them, disengaging hydrogen, accompanied by silicated hydrogen, leaving a residue of silica. Hydrochloric acid completely attacks them in the cold, giving hydrogen, silicated hydrogen, and a white residue having the form of crystals, and constituting an oxide of silicium which will be noticed later. The composition of silicide of magnesium is explained by the formula,

Mg_5Si_2 (Si, 41·2; Mg, 58·8 per cent.)

The author takes Si=21; Mg=12. In taking Si=28 and Mg=24 this formula becomes Mg_5Si_3.

The author considers that the silicide to which M. Wöhler assigned the formula,

Mg_2Si (Si, 30·6; Mg, 52·9 per cent.),

was impure and contained silica.

The dross formed in the preparation of this silicide contains another product, crystallised in cubes, insoluble in water, and which is a double fluoride of sodium and magnesium:

NaFl,2MgFl,

which may also be obtained by melting chloride of magnesium with fluoride of sodium in excess, and chloride of sodium. The crystals of silicium which accompany them may be removed by treating them with a mixture of hydrofluoric and nitric acids.

Oxide of Silicium.—This oxide, which is formed by the action of hydrochloric acid on silicide of magnesium, has already been indicated by Wöhler, who had obtained it by the action of hydrochloric acid on the silicide, but he did not give its composition. Prepared with pure silicide, it is perfectly white, and possesses all the qualities indicated by Wöhler. Treated with potash, it disengages hydrogen; heated in a tube, it leaves amorphous silicium, and gives a gas which smokes in the air. It is not attacked by concentrated and boiling sulphuric acid: nitric acid attacks it slowly. Its composition is—

$3SiO_2,2HO$, or $4SiO_2,3HO$.

By operating with the greatest precaution, and by taking care that the temperature does not pass 0° when attacking the silicide of magnesium by hydrochloric acid, hydrated oxide may be obtained, having this composition:

$2SiO_2,HO$.

In taking Si=28,O=16, these formulæ become $3Si_2O_4,4H_2O$; $2Si_2O_4$ and Si_2O_4,H_2O.

According to the author these bodies are very different from the *leucon* of M. Wöhler. Leucon is richer in hydrogen and silicium than the preceding compound; according to MM. Geuther and Sheerer it is a hydrate;

SiO,HO.

Thus it is $Si_2O_2,2H_2O$.

After a discussion, which it would be difficult to resume here, the author concluded that there were four oxides of silicium,

Si_2O, SiO, SiO_2, SiO_3;

Or with Si=28, and O=16: Si_2O; Si_2O_2: Si_2O_4; SiO_2; and for the silicated hydrogen, SiH_2; the silicide, Mg_5Si_2, becomes $Mg_5Si_3=2Mg_2SiMgSi$. He admits that the silicated hydrogen is H_3Si_2, and that the silicide of magnesium, Mg_5Si_2, ought to be regarded as a combination of two silicides,

$$3Mg_5Si_2=4Mg_2Si+Mg_3Si_2,$$

only the silicide, Mg_3Si_2 taking part in the reaction which produces the silicated hydrogen, whilst the silicide, Mg_2Si, gives rise to the hydrate, $_2SiO_2,HO$.

ON THE LOSS OF SULPHURIC ACID IN SALT-CAKE MANUFACTURE.

BY CHARLES R. A. WRIGHT, B. SC.

More or less sulphuric acid is always lost in the process of conversion of sodium chloride into sulphate, through the mechanical effect of the escaping gases in carrying off vesicles of acid and sulphate spirted up;

in the roasters also a portion of sulphuric acid is volatilized as such, especially if so much acid have been added as would be necessary to convert the whole of the sodium chloride into sulphate. The total amount thus lost necessarily varies in different instances, being dependent on the form of pot and roaster used, the mode of applying heat, etc.; and especially on the amount of salt left undecomposed in the salt-cake. When ordinary salt-cake is withdrawn from the roaster, a white cloud or vapour is emitted from it; when the amount of acid originally added is sufficient to decompose all the sodium chloride present, and when the roasting has been carried so far as to decompose from 98 to 99 per cent. of the chloride, the escaping vapour contains mostly sulphuric acid with but little hydrochloric; if more sulphuric acid have been originally added, and the roasting carried so far as to leave only 1 to 1·5 per cent. of "free acid," a good deal of sulphuric acid is given off from the salt-cake on withdrawal from the roaster; whilst, if less than the requisite amount of sulphuric acid have been originally added, so that 4 to 6 per cent. of the sodium chloride remain undecomposed, the vapours emitted consist chiefly of hydrochloric acid.

Some experiments made on this subject by the writer, together with some similar details kindly furnished by different manufacturers, yielded the following results:—

Nature of Furnace, etc., used.	Average per centage of undecomposed salt in the salt-cake.	Approximate amount of sulphuric acid lost out of 100 parts originally used.
1) Open Furnace	0·60	12·6
2) Blind furnace: one pot.		About 6
3) Ditto ditto	3·40	Less than 1
4) Ditto ditto	5·00	1·0
5) Ditto two pots	3·00	2·4

As might be anticipated, there seems to be a greater amount lost with an open furnace than with a blind one; apparently also when salt-cake containing 95 per cent. of available sodium sulphate (*i. e.*, 5 per cent. of sodium chloride, "free acid," ferric oxide, moisture, etc.) is manufactured in blind furnaces, the average amount of sulphuric acid lost is about 2 parts in 100. Such salt-cake will contain about 54·5 per cent of SO_3, and consequently 100 parts of salt-cake will require for its production about $54.5 + 0·02 \times 54·5$ or 55·6 parts of SO_3. Acid of specific gravity 1·591 at 15°C. contains 55·6 per cent. of SO_3 (Bineau); hence a kilogramme of such salt-cake represents a kilogramme of sulphuric acid of sp. gr. 1·591; or 0·68 kilogramme of SO_4H_2.

It may be noticed that in (4), the SO_3 present in the shape of sodium sulphate in the condensed muriatic acid was only about one-sixth of the total amount: hence indicating that the majority was derived from the roaster where the heat would be sufficient to volatilize SO_4H_2 as such; the mechanical transfer of vesicles spirted up, is however evidenced by the presence of perceptible amounts of sodium compounds in the gases passing off from the pot.

ON THE RECOVERY OF SULPHUR FROM ALKALI WASTE.

BY LUDWIG MOND.

Alkali waste, black ash waste, tank, vat, or blue waste, are the different names of the insoluble residue obtained by the lixiviation of artificial crude soda, or black ash, produced by Leblanc's celebrated process for the manufacture of alkali, which, in spite of innumerable attempts to supersede it, still furnishes almost alone the very large quantities of alkali at present consumed, and has undergone hardly any essential change since its illustrious inventor introduced it about eighty years ago. This superiority to all other known processes is undoubtedly, to a great extent, due to the production of the above named waste, on account of the valuable property it possesses of separating completely and easily from the alkali in the black ash by lixiviation. Nevertheless this waste has been always regarded as the greatest drawback to this important manufacture. Every ton of alkali produces no less than 1½ tons of dry waste, and the enormous quantities thus obtained are generally deposited in the neighbourhood of the works, often forming hills of considerable height. In damp weather especially this waste evolves large quantities of sulphuretted hydrogen, that most noxious and most disagreeable of all gases, sadly annoying the surrounding population; and, moreover, the rain and ground-water coming into contact with it dissolve out considerable quantities of yellow liquor containing hydrosulphide and polysulphide of calcium, which poisons the water of all wells and rivers to which it has access. These evil results are altogether due to the sulphur contained in the waste, which amounts to no less than 80 per cent. of all the sulphur used in the manufacture of alkali, and which represents, of course, a very considerable value. All efforts to recover this sulphur by a cheap and simple method, and thus also to do away with the nuisance of the waste, have until lately failed in their object, though a great many distinguished chemists and intelligent manufacturers have for the last thirty to forty years devoted much time and expense to this important task, amongst whom Mr. William Gossage (to whose well known labours we owe so many valuable improvements in the manufacture of alkali) takes again the first place. A considerable number of methods have been described and patented, none of which, however, have overcome the principal practical difficulty of the question,—to treat the large quantities of waste without employing too much labour and too large a plant.

It is only within the last few years that sulphur has for the first time been regularly manufactured from alkali waste, by a process of the author's invention, and so rapid has since then been the progress of this new industry, that at this year's Paris Exhibition no less than seven works exhibit sulphur recovered from waste by three different methods, all of which have been patented in England as follows:—L. Mond, 8th September, 1863; M. Schaffner, 23rd September, 1865; P. W. Hofmann, 9th April, 1866. All these processes are based on the same principle—viz., conversion of the insoluble sulphide of calcium in the waste into soluble compounds by bringing the waste into contact with air in order to oxidise it. Lixiviation of the oxidised mass and precipitation of the sulphur in these liquors by a strong acid, in practice of course muriatic acid.

This principle has already been described by W. H. Leighton, in his patent for improvements in converting sulphate of soda into sub-carbonate of soda, dated October, 1863. He proposes to allow waste to remain in the vats until it heats and gives off smoke, then to lixiviate and to precipitate the sulphur from the liquor thus obtained by muriatic acid. It is, however, not probable that he ever worked his process out, no account of it being found, except that in the Register of Patents. In 1852 W. S. Losh took out a patent for obtaining hyposulphite of soda by exposing waste in heaps to the atmosphere, lixiviating it, adding carbonate of soda to the liquor, and crystallising. This process has ever since been worked very successfully at the Walker Alkali Works, near Newcastle, where about six tons of hyposulphite of soda per week are produced. Being engaged in researches on the different processes for sulphur recovery by Mr. Gossage and others in the summer of 1860, my attention was drawn to Mr. Losh's patent, and I at once started a series of experiments in order to ascertain whether, and under what conditions, a quantity of hyposulphite of lime could be obtained by oxidation of the waste, which would render practicable the extraction of sulphur on a large scale, and its recovery by means of muriatic acid. I soon found out that the formation of soluble sulphur compounds in the waste increased only up to a certain maximum, when sulphur to the extent of about 5 per cent. of the weight of the dry waste could be extracted by lixiviation, and that this quantity decreased by exposing the waste any longer. When these soluble compounds, however, were washed out, the waste oxidised quite as well a second time, a similar quantity of sulphur being obtained again, and this treatment could be advantageously repeated even a third time.

The waste I used for these experiments being lixiviated by a singular method, since abandoned, was, however, so dense that all efforts to oxidise it in heaps, or by forcing air through it, failed, so that I had to expose it in shallow layers on shelves. This process was patented in France in December, 1861, and in England in August, 1862, and sulphur to the extent of 12 per cent. of the waste has been obtained by it in considerable quantities in a German alkali works.

Coming to England, in the autumn of 1863, I very soon found, however, that the enormous quantities of waste to be treated, and the high rate of wages, made this process quite impracticable here, and that the waste produced by the excellent process of lixiviating black ash, in general use in this country, was very likely to allow of a much more simple treatment. I tried again to oxidise it by forcing air through it, and succeeded so well that the time necessary to oxidise and lixiviate the waste, which had previously been six to eight weeks, was soon reduced to 60 or 80 hours, and that manual labour was almost altogether avoided by performing these operations in the same vat in which the waste was produced, without moving the latter. These facts led to a new process, which was patented the 8th of September, 1863, since when there have been no alterations in the main features of the process described in my specifications of that date. In place of the set of four vats generally in use for lixiviating black ash, I employ a set of ten or twelve. All of these are connected by pipes in the usual way, so that the soda liquor runs from the bottom of one vat to the top of the next one, and by special pipes and taps which allow the sulphur liquor to run out of the bottom of each vat to the top of any other vat in the set. Besides this, they are provided with extra taps and shoots to convey the sulphur liquor to wells or setters. The lower parts of all the vats are connected with a fan, capable of producing a a pressure of about 7 inches of water by pipes with dampers, which regulate the quantity of air passing through. A silent fan of Schiele's construction, 20 inches diameter, price £10, propels a sufficient quantity of air for the treatment of the waste resulting from 100 tons of salt-cake per week. Four of the vats are always filled with black ash in the course of lixiviation, the other six or eight with waste to be treated according to my invention. As soon as the black ash is completely spent and the weak liquor well drained off, the connection with the fan is opened. The waste soon begins to heat, the temperature gradually rising above 200° F., and gives off quantities of steam, becoming greenish and afterwards yellow on the top, gets more and more dry, and would take fire if the air was passed through long enough. The period at which oxidation should be stopped, and the passing of air discontinued, so as to give the best results, must be ascertained in each works by experiment, and varies according as much or little hyposulphite in the liquors is desirable. In the beginning of the action, hydrosulphide and bisulphide of calcium are formed, which are afterwards oxidised into hyposulphite. A part of the hyposulphite is again decomposed into sulphur and sulphite, which is very insoluble and cannot be extracted by lixiviation. Carrying the oxidation too far would therefore entail a serious loss. On an average, the time of exposure will be limited to between 12 and 24 hours. The waste is now lixiviated sytematically with cold water, the weaker liquors passing from one vat to the next one in course of lixiviation, so as to obtain only strong liquors, which operation can be easily performed in six to eight hours. When this lixiviation is finished, air is again passed through the waste in exactly the same way as before; the waste is again lixiviated, and the same treatment repeated a third time. The vat is then ready to be cast, and is again filled with black ask. When the operations have been conducted well sulphur equal to about 12 per cent. of the weight of the salt-cakes used in making black ash is obtained in solution from the waste. The waste contains only traces of sulphide of calcium, and is principally composed of carbonate of lime, sulphite and sulphate of lime, which, far from being noxious, make the waste, on the contrary, a valuable manure. In separating the sulphur from the liquors thus obtained, by adding muriatic acid, I met with much more difficulty than I had anticipated from apparently so simple a reaction.

Firstly, I wanted an easy and rapid method of determining the quantity of acid necessary for the decomposition of a given quantity of liquor, which always contains hyposulphite, polysulphide, and hydrosulphide of calcium and sodium. For this end I availed myself of the following method:—

1. The hyposulphite is determined as usual by a standard solution of iodine and starch, after having first separated the polysulphide and hydrosulphide by adding an excess of chloride of zinc, and filtering.

2. To a certain quantity, say 3·2 c.c. of the original liquor, and starch, is added a standard solution of iodine until it turns blue, the liquor is then again decoloured by a drop of hyposulphite of soda solution, and litmus, and a standard solution of caustic soda are added until the liquid is neutral.

The following reactions take place:—

$$
\begin{aligned}
&2CaO,S_2O_2+I=CaI+CaO,S_4O_5,\\
&CaSx+I=CaI+xS,\\
&CaS,HS+2I=CaI+S+HI,\\
&HI+NaO=NaI+HO.
\end{aligned}
$$

Thus the caustic soda corresponds to the sulphuretted hydrogen, the iodine used in the first titration to the hyposulphite, and from the iodine used in the second titration and the two former numbers the calcium present in the form of sulphide is easily calculated.

Using for both titrations 3·2 c.c. of liquor, and standard solutions containing one-tenth of an equivalent per litre, and presuming that the polysulphide is bisulphide only, we have simply to add the measures of iodine used in both determination, to subtract the measures of caustic soda, and divide this number by ten, in order to find the total percentage of sulphur in the liquor, from which the muriatic acid is easily calculated, every 32 of sulphur requiring 36·5 of hydrochloric acid.

Generally, the polysulphide contains very little more than 2 equivalents of S to 1 of Ca, so that this method is also sufficiently exact for the determination of the sulphur in the liquors, for practical purposes.

Though this method has been proved to be perfectly correct by a number of accurate experiments, the muriatic acid, as calculated by it, was still more than was actually required to effect a complete decomposition of the liquor. A number of careful investigations made with the view of explaining this fact, have shown that, contrary to the assertions of all chemical handbooks, the products of the decomposition of hyposulphite of lime by muriatic acid are, comparatively little sulphur and very little sulphurous acid, but principal trithionic acid, and a small quantity of pentathionic acid. The reaction was proved to take place principally according to the following equation:—

$$5CaOS_2O_2 + 3HCl = 3CaCl + 3HO + 2CaOS_3O_5 + 4S.$$

On boiling, the trithionate of lime is decomposed to sulphate of lime, sulphur, and sulphurous acid. The latter transforms a portion of the hyposulphite, which is still in the liquor, again into trithionate, according to the well-known equation:—

$$2CaOS_2O_2 + 3S_2 = 2CaOS_3O_5 + S.$$

The newly-formed trithionate is again decomposed, and so on. In consequence of these reactions it is possible to decompose a solution of hyposulphite of lime completely into sulphur, sulphate of lime, and very little sulphurous acid, by adding to it when boiling a quantity of muriatic acid sufficient to neutralise about one-half of the lime in solution.

In places where hydrochloric acid has a comparatively high value, these facts may be taken advantage of. As, however, at the present moment, fully one-half of the acid produced by the decomposition of salt is run into the rivers, or passes into the air, and as, besides, the above-quoted reactions involve a very heavy loss of sulphur in the form of sulphate of lime, and produce also a very impure sulphur, I prefer the following plan, which avoids these inconveniences.

The oxidation of the waste is regulated so as to obtain a liquor, which contains as nearly as possible to every equivalent of hyposulphite, two equivalents of sulphide. This liquor is decomposed by first adding to a certain small quantity of acid an excess of liquor, until there is a trace of sulphide in the mixture; then a quantity of acid sufficient to neutralise the whole of the calcium is poured in, a new quantity of liquor equivalent to this last quantity of acid is added, and then acid again, and liquor again, and so on, until the vessel is nearly filled. To the last liquor only one-half of the required acid is added, and steam introduced, until the liquid shows a temperature of about 140° F. Practically speaking, the liquor and the acid are poured at the same time into the decomposing vessel in nearly equivalent proportions, the workmen taking care to keep a small excess of liquor up to the end of the operation. This part of the process is carried on in wooden tanks covered in and connected to a chimney, in order to carry off any sulphuretted hydrogen which may be evolved by mistake of the workman. If properly carried out there should be, however, no appreciable quantity of that gas evolved.

The practical result of this mode of working is simply precipitation of nearly the whole of the sulphur in a pure state.

$$CaO,S_2O_2 + 2CaSx + 3HCl = 3CaCl + 3HO + (2 + x)S$$

The details of the reaction, are, however, very complicated, almost all the different acids of sulphur being probably formed during the process.

In the first place, by adding liquor to acid, some sulphuretted hydrogen is given off (which may be avoided by starting the operation with liquor rich in hyposulphite) and hyposulphurous acid is set free, which will give rise to the formation of sulphurous and several thionic acids. All these are, however, again converted into hyposulphite by the sulphide of calcium in the liquor, then added in excess.

$$3SxO_2 + 2CaSy = 2CaO.S_2O_2 + (3x + 2y + 4)S.$$
$$3SxO_5 + 5CaSy = 5CaO,S_2O_2 + (3x + 5y + 10)S.$$

The muriatic acid entering next, thus only produces hyposulphurous acid, and its products of decomposition, which are again converted into sulphur and hyposulphite without the formation of any gaseous product, and so on. At the end there is a certain quantity of hyposulphite left in the liquors, which is decomposed into sulphate and sulphur by adding an insufficient amount of muriatic acid. In practice about 90 per cent. of the muriatic acid, calculated according to the above described method, are required to effect thus the complete decomposition of a well-proportioned liquor. If it contains more hyposulphite than above indicated, less acid is, of course, to be used. About 90 per cent. of the sulphur contained in the liquor is precipitated in an almost pure state, and settles exceedingly well within two hours. The supernatant clear solution of chloride of calcium is then drawn off, and another operation directly commenced in the same vessel. As soon as a sufficient quantity of sulphur is collected in it, which will depend on the size of the vessel and on the strength of the liquor (varying from 4 per cent. to 7 per cent. of sulphur), it is drawn out by means of a door at the lower part of the vessel into a wooden tank with a double floor, where the chloride of calcium is washed out by water, and the sulphur then simply melted down in an iron pot. The product thus obtained contains only from $\frac{1}{10}$ per cent. to 1 per cent. of impurities, and is thus by far superior to any sort of brimstone in the market, though it has sometimes a rather darker colour caused by traces of sulphide of iron, or a little coal dust, which latter may have been suspended in the muriatic acid.

The total yield of sulphur obtained by the process amounts thus to 10 or 11 per cent. of the weight of the salt-cake used in making black ash, or about $\frac{1}{4}$ of the sulphur therein contained, and to about 60 per cent. of the sulphate contained in the waste. I still hope, however, to be able to increase this quantity considerably by some more years' experience. The cost of production, as well as that of the plant, are inconsiderable. In the different continental and English works, where the process has now been working for years, the expense for wages, fuel, and maintenance amounts only to £1 per ton of sulphur, and the outlay for the plant has been more than covered by the net profits of the first year.

- *c.* Substances nearly free from sulphides or arsenides, but containing chlorine, iodine, or bromine.
- *d.* Argentiferous litharge, and other easily reducible oxides.

I. A. Metallic Alloys capable of direct Cupellation.

a. Consisting chiefly of Lead or Bismuth.—In determining the silver contained in these alloys, it is only requisite to place a clean piece of the same, weighing about from one to ten grains according to its probable richness in silver, upon a cupel of coarse bone ash, and proceed by concentration and cupellation exactly as has been already described under these heads.

Should the substance be not altogether metallic, or not free from adherent slag, earthy matter, or other extraneous matter, it should previously be fused on charcoal with a little borax in the reducing blowpipe flame, and the clean metallic globule then removed from the charcoal, and treated as before. In order to remove the globule from the inherent borax-glass, it may be allowed to cool, and then detached; or, after a little practice, it will be found easy, by a quick movement of the charcoal, to cause the globule still melted to detach itself completely, and drop on the anvil in the form of a single somewhat flattened globule, without suffering any loss of lead adhering to the charcoal.

In the case of argentiferous bismuth alloys the process is carried on in all respects the same as if silver-lead were being treated. As, however, the bismuth globule is very brittle, care must be taken when separating the concentrated globule from the litharge, as, if not carefully done, a loss may easily be sustained from a portion of the globule remaining behind adherent to the litharge. It is better, therefore, to remove the litharge by degrees from the globule with the aid of the forceps.

Argentiferous bismuth, free from lead, when cupelled alone, invariably leaves a globule of silver, having a dull frosted surface. If, however, at the end of the operation a small quantity of lead (¼ to ½ a grain) be added, and fused along with it, the silver globule then obtained will be perfectly bright and free from all bismuth.

In the case of native bismuthic silver it is advisable to fuse the previously weighed mineral with a little lead and borax-glass on charcoal in the reducing flame, so as to free it from any adherent earthy matter, and then proceed by concentration and cupellation, as before described.

Silver Assay.

I. A. Metallic Alloys capable of direct Cupellation.

- *b.* Consisting chiefly of silver: native silver, bar, test, and precipitated silver. retorted silver amalgam, standard silver, silver coin, and other alloys of silver with gold and copper.

These alloys may be at once fused with lead on the cupel itself, and the operation finished as before described. In general, however, it is better to fuse the weighed assay previously with the requisite amount of pure lead and a little borax-glass, say from a quarter to half the weight of assay, in the reducing flame at a low heat on charcoal until the globule commences to rotate. This ensures having a perfectly clean button of silver-lead, which is then cupelled in the ordinary manner.

In most cases the quantity of lead to be added need not exceed that of the weight of the alloy, but when several percentages of copper are present in the assay, as in case of many coins, etc., the lead should be increased to some three, or even five times the weight of the assay in proportion to the amount of copper actually contained in the substance under examination, and which will be treated of more at length under the head of copper-silver alloys.

When no more lead has been added to the assay than its own weight, the cupellation may be concluded in one operation by inclining the stand, and so moving the globule on to a clean part of the cupel; but when more copper is present, it is preferable to concentrate first and cupel subsequently, in order thereby to reduce the cupellation loss to its minimum.

In the concentration as much copper as possible should be slagged off with the lead, which is effected by inclining the cupel somewhat more than usual. so that its surface may be less covered up with the litharge and exposed as much as possible to oxidation, by which means the litharge, as it forms, is enabled to carry off more of the copper contained in the silver lead.

Should the silver globule after cupellation show indications of still containing copper, as before noticed, when treating of cupellation, a small quantity of lead must be fused along with it, and the cupellation finished as usual.

As at the present time no means are known by which silver can be separated from gold by the use of the blowpipe, in all cases of alloys containing gold, this metal remains to the last along with the silver, and the result in such cases always indicates the combined weight of both these metals contained in the alloy under examination. The employment of the humid assay must be resorted to for effecting their separation:—

- *c.* Containing chiefly copper: native copper, ingot, wire, or sheet copper, cement copper, copper coins, copper-nickel alloys.

Under the most favourable conditions in cupellation, the amount of lead requisite when converted into litharge to slag off one part of copper along with it as oxide, amounts to between seventeen and eighteen parts its weight. In the blowpipe assay it is usual to add to any cupriferous alloy an amount of pure lead equal to twenty-times the amount of copper contained in the alloy, in order to ensure the whole of the copper being separated in the litharge. In the case of nickel the amount of lead required is somewhat less than with copper, but in practice the same amount of lead may be employed.

When the copper is quite clean the requisite amount of lead may be added to it in a single piece on the cupel, fused and cupelled as usual, after previous concentration of the silver-lead to a small-sized globule.

It is generally found, however, that traces of iron, slag, gangue, or other foreign matter is present; and, consequently, it is usually advisable to fuse the assay along with the requisite amount of lead, and about one-half its own weight of borax-glass in the reducing flame, until the whole of the substance is seen to have perfectly combined or alloyed with the lead, and the globule has come into brisk rotation, whilst at the same time no detached metallic globules are seen in the borax-glass.

The concentration of the silver-lead and cupellation are then conducted as usual, taking care when concentrating to incline the cupel-stand so as to expose as much of the metallic surface of the melted globule to the oxidising action of the air as possible, with a view of enabling the litharge whilst forming to carry off as much copper along with it as possible.

Should the silver globule obtained after cupellation spread out, or appear to the eye more flattened than usual with globules of pure silver, it indicates that some copper still remains, and a small piece of assay lead (½ to 1 grain weight) should be placed alongside it whilst still on the cupel, fused together, and the cupellation finished on a clean part of the same cupel as usual.

Precipitated or cement copper, especially that which is in the crude state, and has not been melted and run into ingots, is often very impure, containing so much iron, lead, arsenic, earthy matter, etc., as not to admit of direct cupellation, and in such case should be treated as pertaining to class B. *a.*:—

B. Metallic Alloys incapable of direct Cupellation.

a Containing much copper or nickel, with frequently some little sulphur, arsenic, zinc, iron, cobalt, etc., as unrefined or black copper, brass, German silver, etc.

As the presence of these extraneous matters would interfere with the cupellation either by causing a loss of silver-lead projected from the cupel upon the evolution of the volatile substances present, or by forming oxides which could not be absorbed by the cupel, it is necessary to eliminate such substances by a scorification with borax on charcoal previous to concentration or cupellation.

In the case of unrefined and black copper, the portion used in the examination is placed in the scoop with twenty times its weight of assay lead, and its own weight of powdered borax-glass, mixed with the spatula, and transferred to a soda-paper cornette. It is then fused on charcoal in the reducing flame, which should be constant and uninterrupted, until all particles have completely united, and a brisk rotation sets in, which is kept up for a short time, when the silver-lead globule, which should appear bright on the surface after cooling, is concentrated and cupelled precisely as is directed under A. *c.* By this preliminary scorification the sulphur, arsenic, and zinc are volatilised, and any lead, cobalt, or iron slagged off into the borax-glass.

In the assay of brass and German silver the quantity employed is fluxed with its own weight of borax-glass, but only requires ten times its weight of assay lead. The operation is commenced as before, but the globule is kept somewhat longer in rotation (always keeping the flame directed only on to the borax-glass), so as to allow the zinc present to be completely volatilised, which is evident when the surface of the silver-lead becomes bright, on which the heat is increased for a few moments to expel the last traces of that metal, and the silver lead thus obtained is concentrated and cupelled as before.

The silver globule obtained from the cupellation of substances rich in copper generally requires the addition of a small quantity of lead and re-cupellation (as before described), in order to ensure its freedom from copper.

Behaviour of Lime when Burned.—Dorlhar and Saminn. Two cylinders formed out of the same piece of limestone measured 27 millimètres in length and 17 millimètres in diameter. After being completely burned their volume had increased nearly 1-10th—viz., to 28 millim. and 17·7 millim.—*Berg. und huttimm, Zeitung*, 1867.

NOTE ON THE CALCULUS OF CHEMICAL OPERATIONS.

BY PROFESSOR WILLIAMSON, F.R.S.

The remarkable memoir of Sir Benjamin Brodie, respecting which these remarks are made, is the first consistent attempt to introduce analytical reasoning into the body of the science of chemistry.

One fundamentally important question of method is raised by the memoir; and as it may be considered apart from the rest of the subject, and is, in fact, a preliminary to any discussion upon it, the author wishes to draw attention to some considerations relating to it.

Sir B. Brodie defines a chemical operation as an operation performed upon the unit of space, of which the result is a weight. The unit of matter (or molecule) adopted is the weight of matter of a specified kind, which occupies in the state of perfect gas the volume of one litre at 0°C. and a pressure of 760 millimetres of mercury.

This absolute definition is intended to supersede the prevailing theory that the molecule of each compound is the smallest proportional weight in which we can, consistently with its other properties, represent it as taking part in any reaction, or in which we can suppose it to exist by itself.

In some cases the vapour-densities of many compounds have confirmed the molecular weights assigned to them by a comparison of their reactions; but in other cases, many of which are too familiar to need mention here, the vapour-density contradicts the above evidence of the molecular weight. What is the result in such a case of conflict? Uniformly this: that if the vapour-density and reactions are irreconcileable, we know that the vapour-density must have given wrong advice, and it only remains to be seen by an examination of the anomalous vapour how the molecule broke up on evaporation.

Perhaps the best way to judge of the working of the new definition is to see the manner in which Sir B. Brodie himself applies his principle. Thus, at page 817 of his memoir, the units of thirteen substances are given, and opposite each formula is given the "absolute weight in grammes" of a litre of the vapour, and in another column the "relative weights" of each. Of these fundamental statements four only—viz.: the numbers for sulphur, sulphuretted hydrogen, sulphurous acid, SO_2, and sulphuric acid, SO_3, are the records of observations. The numbers for three other substances are at variance with observation, for SO_3H_2 breaks up on evaporation into SO_2 and H_2O, forming a mixed vapour of about half the density given. SO_4H_2 breaks up similarly, forming a vapour of about half the specific gravity assumed, and Nordhausen acid first breaks up into SO_3 and SO_4H_2, and this hydrate decomposes at a higher temperature, as above mentioned. The vapour from Nordhausen acid has, therefore, a specific gravity vastly below that assumed. No doubt there are good reasons derived from a study of other facts for believing that these three compounds, *if they were capable of evaporating undecomposed*, would have the vapour densities assigned to them by Sir B. Brodie; but taking the simple definition as given, we are led to molecular weights, which the author, in common with all chemists, considers inadmissible, and which he very properly corrects.

seems to be every reason to believe was constructed in 1675. Papin became Boyle's amanuensis in 1676, and remained for some length of time in his service, during which he conducted a number of experiments at Boyle's suggestion, chiefly with a view of testing the accuracy of previously made experiments: these were published, together with a description of the new air-pump, in 1680, in a work written by Papin,[*] but read over and revised by Boyle, whose name alone appears on the title-page. It was written in French, and translated into Latin before printing; a translation into English was subsequently published.

It commences with a description of Papin's double-barrelled air-pump, which consisted of two vertical cylinders of brass, in each of which worked a piston fitted with a valve opening upwards; at the bottom of each cylinder there was also a valve opening upwards; indeed the pump was precisely similar to that used in the present day as regards the fitting with self-acting valves, and only differed in the mode of working the pistons. At the top of each piston rod a metal stirrup was fixed, and these were connected by a cord passing over a pulley. The pump was worked by a man who, putting one foot into each of the stirrups, threw the weight of his body first upon one piston and then upon the other; thus the action somewhat resembled that which is practised on a tread-mill. The advantages of the double pump barrel over the single barrel are considerable: in all the previous air-pumps the piston had to be drawn from one end of the barrel to the other against the whole pressure of the atmosphere, but by the introduction of two barrels, and the connection of the piston rods, so that the descent of one piston caused the ascent of the other, it will be perceived that the pistons balance each other, for the downward pressure of the atmosphere upon one, balances the pressure tending to press down the other. Thus a double-barrelled air-pump not only exhausts in half the time required by a single-barrelled pump, but requires less force to work it.

The degree of rarefaction which obtained in the receiver was measured by a mercury gauge enclosed within the receiver. It consisted of a graduated tube sealed at one end and filled with mercury, with the exception of a small space occupied by a bubble of air, by the expansion of which the degree of rarefaction was ascertained.

The first experiment is dated July 11th, 1676, and relates to "several waies used to help the production of air." The work, for the most part, treats of the preservation of edibles in vacuo, and the amount of air produced by various substances when kept for a length of time in closed vessels. There is nothing of interest or utility among the experiments, which much resembles those described in Papin's former treatise.

Among other things, we find an account of Papin's wind-gun and of some experiments made upon animals in compressed air. A mouse was placed in the receiver of the gun, and the air rapidly compressed to one-twentieth of its former volume, the gun was then discharged, the receiver opened, and the mouse was found to be dead, at which Papin expresses great surprise, as he expected to find it "only a little convulsive." A second mouse was then placed in the receiver, and the air compressed to one-fourth its original volume. On discharging the gun and opening the receiver, the mouse was found to be alive and well. Finally, a mouse was left in air compressed to one-seventh of its volume for 24 minutes, and the gun then discharged. When the mouse was taken out it was observed "to fetch many deep groans," and soon after it died, from all of which experiments he deduces the corollary, "That a great compression of air is noxious, yea mortiferous to animals."

In 1684, Papin was appointed curator to the Royal Society with a salary of £30 a year, for which he was to show at least one experiment at each meeting of the society. It will be borne in mind that this office had previously been held by Hooke, now one of the secretaries to the Society.[*]

In the *Philosophical Transactions* for 1686, we find a paper entitled, "An Account of an Experiment shown before the R.S., of Shooting by the Rarefaction of the Air," by Dr. Denys Papin, R.S.S. Otto Von Guericke had previously described a similar invention, but Papin's gun was considered more effective. It consisted of a long tube fitted with suitable valves. It was exhausted by an air-pump, and the external air was suddenly admitted. By this means a bullet weighing two ounces was propelled with great velocity to a considerable distance.

In the *Philosophical Transactions* for October, in the same year, Papin gives "A demonstration of the velocity wherewith the air rushes into a vacuum." The Academie des Sciences had previously endeavoured to determine the relative velocities of air and water under similar conditions, by filling a bladder first with water, then with air, applying a like pressure, and noting the time necessary to empty the bladder respectively. It was thus found that the bladder of air could be emptied in one twenty-fifth the time necessary to empty the bladder of water, and it was hence concluded that with equal orifices and pressures the velocity of air is twenty-five times greater than that of water. But for many reasons stated by Papin this method is most erroneous. The following mode of demonstration was adopted by Papin. It had been proved that the heights to which dissimilar liquids will be driven by the same pressure will be reciprocally as the specific gravity of the liquids; thus a pressure which causes mercury to rise to a height of one foot will cause water under the same conditions to rise to a height of $1 \times 13{\cdot}5$ feet. From Galileo's demonstration that the velocities of bodies are as the square roots of the heights to which they would ascend, it follows that the velocities of two dissimilar liquids is as the square root of their respective specific gravities. Now the pressure of the air is equal to that of a column of water 32 feet high, which would rush out with a velocity of 45 feet per second. The specific gravities are as 840 to 1, and the roots as 29 to 1; therefore the velocity of air is 29 times greater than that of water under similar conditions; hence he concludes that the velocity of air driven by the whole pressure of the atmosphere, or in other words the velocity of air entering a vacuum, would be $45 \times 29 = 1,305$ feet per second.

Papin continued to act as Curator to the Royal Society till the year 1687, when he was appointed Professor of Mathematics in the University of Marburg, an appointment which he held until his death, in 1714.

Although Papin did not do much by direct means to

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[*] This work is entitled "*A Continuation of New Experiments physico-mechanical, touching the spring and weight of the air, and their effects.*" The second part. By the Hon. Robert Boyle. London, 1680. The first part of the continuation was published in 1669; an account of it will be found in the thirteenth of these papers, Chemical News, Vol. 12, p. 62.

[*] See the tenth of these papers, Chem. News, vol. ii. p. 38.

further pneumatics, he did much indirect service to the science by the invention and improvement of apparatus. It is strange that the invention of the double-barrelled air-pump should be so frequently attributed either to Boyle or Hauksbee; the error is probably due, on the one side, to the fact that the first account of it was published in a work which claimed Boyle for its author; and in the second place because it was improved and first perfectly figured by Hauksbee, and by him brought into general use. Winkler* of Leipsic, while he is most cautious to mention the slight improvements introduced by Sengwerdus, Wolfius, Leupold, S'Gravesande, and Musschenbroek, does not so much as mention Papin. The air-pump as Papin found it possessed but one barrel; one at least of its valves was worked by hand; and the receiver had to be cemented to the pump-plate before the commencement of each experiment. As he left it, it possessed two pump-barrels, fitted with self-acting valves; a ground pump-plate upon which a receiver with ground edges could remain air-tight without the necessity of cement; and a connection was established between the pistons, so that the descent of one effected the elevation of the other.

We can scarcely be surprised that Papin did not greatly extend our knowledge of the nature and properties of the air, for the above extracts from his works clearly show us that his object was to apply the air-pump to useful purposes, rather than to extend pure pneumatical research. He was prouder of his "Digester of Bones?" than of his double-barrelled air-pump; and he preferred the experiments on the preservation of fruits to the "demonstration of the velocity wherewith the air rushes into a vacuum."

We may mention *en passant*, that Papin was one of the first to adopt the important theory of combustion (since completely verified), of "Ce très subtil Anglois M. Robert Hook," as he calls him.

ON THE

UTILISATION OF THE WASTE PRODUCTS OF THE MANUFACTURE OF COAL GAS.

BY DR. LETHEBY.†

As you are aware, the residual products of gas-making are six in number—namely, *coke*, *ammoniacal liquor*, *coal tar*, and the three waste products from the purifiers, as the *spent oxide of iron*, the *refuse lime*, and the *acid or other matters used for absorbing ammonia*, each of which has its special value on account of its technical uses.

I.—Coke.

This need not occupy much of our attention, as its practical value and uses are pretty well known to you. I may say, however, that it was the opinion of the late Dr. Fyfe, and is still the opinion of many chemists who have examined the power of coal under steam-boilers, that the heat actually made available *in practice* is very nearly the same as ought to be produced *according to theory* by the quantity of coke which the coal yields. He found that a pound of Scotch coal from Trenant would boil away 5·61 lbs. of water, and that the coke of it, which amounted to 0·525 of a pound, produced 3·9 lbs. of steam; so that the practical loss was 5·61 − 3·9 = 1·71 lbs., but the theoretical value of the coke was about 5·5 lbs. of steam. Here is a table of the relative heating power of different fuels, expressed in the number of pounds of water which 1 lb. of the substance will boil away when the water has been heated to its boiling point:—

Dry wood (average of many specimens)	4·51 lbs.
Derbyshire coal (ditto)...............	7·58 "
Scotch coal (ditto)...................	7·70 "
Lancashire coal (ditto)...............	7·94 "
Newcastle coal (ditto)................	8·37 "
Welsh coal (ditto)....................	9·05 "
Good coke (ditto).....................	10·00 "

If all these numbers are multiplied by 5·5, they give the quantity of water which a pound of the fuel will in each case raise from 32° to 212°, and the results show that the thermotic power of coke is very high.

II.—Ammoniacal Liquor.

This is the aqueous portion of the condensed products of the gas. It floats upon the tar, and is a watery solution of carbonate, sulphide, and sulphocyanide of ammonium, with certain carbohydrogens of no value.

If all the nitrogen contained in coal were converted into ammonia, so as to make a liquor of 8-oz. strength (4° Twaddle), it would yield from 142 to 226 gallons per ton of coal. This will be evident from the table which is before you, and which represents the average amounts of nitrogen in certain well-known coals in a dry condition:—

	Nitrogen per cent. in Coal.	Ammonia per cent. from Coal.	Gallons of Liquor of 4° Twaddle p. ton of Coal.
Welsh coal (average)	0·91	1·10	142
Lancashire coal (ditto)	1 25	1·52	196
Newcastle coal (ditto)	1·32	1·60	206
Scotch coal (ditto)	1·44	1·75	226

But by far the largest portion of nitrogen is not converted into ammonia, for by combining with sulphur and carbon it forms the sulphocyanides which are so abundant in ammoniacal liquor and in spent lime, and much of it also unites with carbon and hydrogen to produce the alkaloids which exist in the tar. In practice, therefore, you get but a comparatively small proportion of the nitrogen as ammonia in the ammoniacal liquor. The quantity of liquor rarely exceeds 45 gallons of 8-oz. strength per ton of coals; and to obtain this quantity you must condense well, and also wash the gas with water. I have already explained to you how this is done at the Birmingham and Staffordshire Gas Works by Mr. Hugh Young, who obtains 44 gallons of liquor per ton of Staveley coal in his yearly working. In ordinary practice the yield is about 25 gallons per ton, and in London it is not above 13 gallons—calculated in every case as 8-oz. liquor. You will see from this how largely the production of ammoniacal liquor may be increased; and I will call to your recollection the valuable advice of your president, Mr. Hawksley, with respect to the copious washing of raw gas with ammoniacal liquor, for this practice has a two-fold advantage—it not only purifies the gas by removing tarry matter and sulphur compounds, but it also strengthens the liquor and renders it a more valuable product. By using the liquor as a shower or in a scrubber, in the proportion of 1 volume of liquor to 16 of gas, the liquor may easily be raised to 10° or 11° of Twaddle, which are equivalent to from 20 to 22 ounces of acid; and considering that the price of liquor rises

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* Vide his *Anfangsgründe der Physic*. Leipsic, 1754, translated into English in 1757.

† A lecture delivered before the British Association of Gas Managers. Corrected and communicated to this paper by the Author.

about 4d. or 6d. per butt for every degree of Twaddle, it is manifestly of the greatest importance that the liquor should be sent away from the works as strong as possible. It ought, in fact, never to be under 6° of Twaddle, or of less than 12-oz. strength; and, with proper condensation and judicious washing, there should be from 29 to 30 gallons of such liquor produced from every ton of coals. The average price of ammoniacal liquor of 8-oz. strength, in eleven towns of England, is at the present time 2s. 7d. per butt of 108 gallons. It ranges from 1s. 9d. to 4s. 6d. per butt, and in London it fetches 2s., with an increase of 4d. per butt on every additional ounce of acid strength. Below 3° of Twaddle or 5oz. of acid it does not pay for working, whereas at 10° or 11° of Twaddle it is a valuable product. The strength of the liquor may be estimated either by the hydrometer or by the quantity of strong sulphuric acid (sp. gr. ·1845) required to neutral ise it; and it will be found that every degree of Twaddle is equal to about 2 ounces of acid per gallon of liquor.

The method of converting the liquid into a salt of ammonia varies in different places according to the facilities for working. In some places the liquor is at once saturated either with sulphuric or muriatic acid, in a close tank, and the evolved gases (sulphuretted hydrogen and carbonic acid) are carried to a furnace or to a furnace shaft. The saturated liquor is then evaporated and crystallised in open troughs. This, however, produces a dark-coloured salt which is not very saleable. The liquor, therefore, is either distilled alone from a steam-boiler, or it is mixed with lime in the boiler, so as to fix the sulphuretted hydrogen and carbonic acid, and is then distilled. In many works the liquor is heated in an apparatus called a Coffey's still, which is a tall vessel containing a number of transverse divisions (from 20 to 30) which alternately pass to nearly the opposite sides of the vessel. The liquor is run in at the top, and as it flows from side to side over the alternate divisions, in its way downwards it meets a rush of steam, which is admitted at the bottom of the vessel, at a pressure of from 20 to 30 lbs. upon the inch, and thus the carbonate and sulphide of ammonium are volatilised. In all these cases the ammonia is distilled into a close vessel containing sulphuric acid, diluted with enough water to prevent the salt from crystallising (equal parts of brown chamber acid of commerce and water are good proportions); and the evolved gas (carbonic acid and sulphuretted hydrogen) is conveyed to the furnace fire, or is used for the production of oil of vitriol. When the ammoniacal liquid is evaporated by blowing steam into it, it is necessary to have a worm, or other cooling apparatus, to condense the water from the gases before they are carried to the furnace, or they will perhaps extinguish the fire. While the distillation is going on the acid in the saturating vessel is frequently examined, and when it is thoroughly neutralized, it is run out into a proper receiver, and is then transferred to shallow pans or troughs, where it is evaporated to the crystallising point.

The residual liquor from the stills is not completely exhausted of ammonia, but contains from 3 to 5 ounces of sulphocyanide of ammonium per gallon. It is, therefore, treated with lime, and again distilled, whereby more ammonia is obtained.

If there were a large demand for the sulphocyanide, it might perhaps be worth while to recover it from the spent liquor by evaporation, especially where it could be done by waste heat. Here is some of the residual liquor, and you will notice that when I add to it a very acid solution of a persalt of iron it produces a deep blood-red colour of the ferric-sulphocyanide. Here also is some of the salt obtained from the liquor by evaporation, and it is well suited for the preparation of this white powder, the mercuric sulphocyanide, which is the sole constituent of the little toys called Pharaoh's serpents. Sulphocyanide of ammonium is also used to some extent by photographers. I may here mention that the watery solution which runs from the hydraulic mains with the tar, when the temperature is not below 150° Fahr., contains this salt, without any carbonate or sulphide of ammonium; there is no reason, therefore, why this solution may not be collected, apart from the true ammoniacal liquor which is found in the condensers, for even if it be not of much value on its own account, it might be kept from diluting the liquor in the first stages of condensation, and be afterwards used instead of water for finally washing the raw gas.

In country gas-works, where there is little or no sale for ammoniacal liquor, it would not be difficult to convert it into sulphate of ammonia, by transferring it to an old boiler, then blowing steam into it, and carrying the vapors into a properly constructed vessel, charged with the brown sulphuric acid of commerce, diluted with the mother liquor of a previous crystallisation. In this way every ton of coals should yield about 30 lbs. of sulphate of ammonia.

This sulphate is worth from £12 to £14 per ton, and it is not merely used for agricultural purposes, but it is the salt from which all other preparations of ammonia are obtained. Distilled with quick-lime it yields pure ammonia, which by condensation in water forms the liquor ammoniæ of commerce; distilled with chalk it makes carbonate of ammonia; and it has other applications. There are good reasons, therefore, why great pains should be taken to recover all the ammonia of gas-making. We shall presently see how this may be further accomplished by means of absorbent agents placed at the end of the purifiers.

III.—Coal Tar.

This is a very complex liquid, for it contains at least three classes of compounds—viz., acids, neutral bodies, and alkaloids, the composition and leading properties of which are as follows:—

Acids of Coal Tar.

Names.	Formulæ.	Specific Gravities.	Boiling-points (Fahr.)
Acetic..........	$C_4\,H_4\,O_4$	1062	243°
Butyric.........	$C_8\,H_8\,O_4$	973	314
Carbolic........	$C_{12}H_6\,O_2$	1065	370
Cresylic........	$C_{14}\,H\,\,O_2$	—	397
Phlorylic... ...	$C_{16}H_{10}O_2$	—	424
Rosolic.........	$C_{24}H_{12}O_6$	—	—
Brunolic.......	?	—	—
Neutral Bodies of Coal Tar.			
Alliatous oils....	?	?	?
Benzole.........	$C_{12}\,H_6$	850	177
Toluole.........	$C_{14}\,H_8$	870	230
Xylole..........	$C_{16}\,H_{10}$	867	164
Cumole.........	$C_{18}\,H_{12}$	870	299
Cymole.........	$C_{20}\,H_{14}$	861	341
Naphthaline	$C_{20}\,H_8$	1153	428
Anthracine	$C_{28}\,H_{10}$	1147	572
Pyrene..........	$C_{30}\,H_{12}$	—	?
Chrysene........	$C_{24}\,H_8$	—	?

Basic or Alkaline Bodies of Coal Tar.

Pyridine	$C_{10}H_5\,N$	986	242
Pyrrol	$C_8\,H_5\,N$	1077	272
Picoline........	$C_{12}H_7\,N$	961	271
Lutidine........	$C_{14}H_9\,N$	946	310
Collidine.......	$C_{16}H_{11}N$	937	354
Parvoline......	$C_{18}H_{13}N$	—	370
Aniline.........	$C_{12}H_7\,N$	1080	360
Toluidine......	$C_{14}H_9\,N$	—	388
Xylidine.......	$C_{16}H_{11}N$	—	418
Cumidine	$C_{18}H_{13}N$	952	437
Cymidine	$C_{20}H_{15}N$	—	482
Chinoline	$C_{18}H_7\,N$	1081	462
Lepidine	$C_{20}H_9\,N$	1072	510
Cryptidine	$C_{22}H_{11}N$	—	525

The general properties of coal tar, as well as the proportions of its several constituents, vary with the quality of the coal used, and with the temperature at which it is distilled or carbonized. The tar which is produced from common gas coals at rather high temperature is always heavier than water (sp. gr. 1120 to 1150). It dries freely in the air, and its hydrocarbons are so rich in carbon that the tar cannot be burnt in an ordinary lamp. But the tar which is produced from cannel coal at lower temperatures is lighter than water, and does not readily dry when it is exposed to the air. Besides which, its hydrocarbons are comparatively poor in carbon, and may be burnt in lamps. There is almost every variety of coal tar from these two extremes, but the tars of commerce are chiefly of three kinds—viz., the rich cannel coal tar of Scotland; the tar which is produced from common coal in country gas-works, where the temperatures are generally low; and the still heavier tar of the London gas-works, which is produced at excessively high temperatures. The yield of tar per ton of coals is from 9 to 15 gallons—the latter being the average at country works; and the former, or from that to 10 gallons, is the yield in London, where the tar is undoubtedly affected by the high temperature of the retorts, for it is not only small in quantity, but it is deficient of naphtha, and contains more pitch than country tar; besides which, the dead oil from it is always overloaded with naphthaline.

In London the distillation of coal tar is always effected in stills, which are placed over a fire, and the products are collected at different stages of the distillation. Up to a temperature of from 160° to 190° Fahr. little or nothing flows over, but at that temperature ammoniacal liquor, with *crude naphtha* of a gravity of 850, begins to distil. These continue to flow until the thermometer rises to from 310° to 340°, when a heavier naphtha of a gravity of about 920 is carried over. This is called *light oil*, and it is collected separately until the temperature rises to from 370° to 400°, and then the oil begins to have the gravity of water; after that, and up to the temperature of from 690° to 700°, the oil which is collected is heavier than water, and is therefore called *heavy oil* or *dead oil*—the last runnings having a gravity of about 1060 or thereabouts. If a soft pitch is wanted the process of distillation is stopped at this stage, but if a harder pitch is required it is pushed a little further, and the *green oil* which flows over is rich in neutral oils, which are well suited for making railway grease.

A still containing 2,500 gallons of coal tar will in this way yield about the following proportions of the several products:—

Ammoniacal liquor from	50 to	70 gals.	(average) 60 gals.
Crude naphtha........	30 "	50 "	" 40 "
Light oil.............	12 "	35 "	" 30 "
Creosote or dead oil...	689 "	740 "	" 720 "
Pitch	8 "	10 tons	9 tons.

Each of these products has its commercial value, the naphtha and light oil being used for the production of benzole and toluole of commerce—naphthas which are largely in demand for the manufacture of coal-tar colours.

Formerly the greatest value was attached to the naphtha or benzole which had a low boiling-point, and the contracts, especially with the French, were for a benzole or naphtha which yielded 90 per cent. of volatile oil at a temperature not exceeding 212°, and I have examined thousands of gallons of this quality for the French market. Even at the present time there is a demand for this, which is called 90 per cent. benzole, for making certain aniline reds; and to obtain it the crude naphtha, or the first runnings from the tars, were distilled alone. At present, however, there being a demand for a less volatile oil, the practice is to mix together the crude naphtha and the light oil, and to subject them to fractional distillation, thus:—Steam is blown into them at a pressure of from 20 to 30 lbs. on the inch, and the naphtha which comes over with the steam is called *once run naphtha.* This is purified by shaking it with strong sulphuric acid (sp. gr. 1845), using the acid in small proportions at a time, for fear of injuring the naphtha, and washing with water between each operation. In this manner, after using about 5 per cent. of acid (or ½ lb. to each gallon of naphtha), the brown colouring matter of the naphtha and all basic compounds are either destroyed or removed, and the brown naphtha, after being well washed with water, is again distilled by blowing high-pressure steam into it, and the products are collected at three stages; that which comes over first is called *crude benzole of* 80 *per cent. strength*, the second runnings are a naphtha containing 50 *per cent. of benzole*, and the third is a naphtha which is used for solvent purposes. With the view of strengthening the 50 per cent. benzole, and making it 80 per cent., it is redistilled from a vessel with a steam jacket, whereby the temperature can be regulated. That which flows over at a temperature up to 210° is set aside as 80 *per cent. benzole;* that which distils between 210° and 260° is called 30 *per cent. naphtha;* and the residuum, on being treated with high-pressure steam, yields *solvent naphtha.* Once more the 30 per cent. naphtha, or that which has flowed over at from 210° to 260°, is distilled with a dry steam heat, and when the thermometer has risen to 106° there is obtained a little more 80 per cent. benzole; after which, and up to 234°, there flows over what is called 40 per cent. naphtha, and from 234° to 260° a little of the 30 per cent. Steam is then blown into it, and it yields a little more of the solvent naphtha.

In this way, by a series of fractional distillations, the washed naphtha is made to yield at each successive operation a quantity of 80 and 40 per cent. naphtha. All the 80 per cents. are then mixed together, and are once more distilled by a dry steam heat. The naphtha which flows over at a temperature up to 204° is called 90 per cent. benzole; that which flows between 204° and 210° is called 80 per cent. benzole, and is again fractionally distilled up to 204°; while the residue, on being treated with high pressure steam, yields a quantity of 40 per cent. naphtha.

Five separate products are thus obtained—namely, 90 per cent. benzole, 40 per cent. benzole, solvent naphtha, the last runnings of the first operation, and the residuum of each distillation. Operating in this manner with a charge of 1,587 gallons of crude naphtha and light oil, there is first obtained 897 gallons of once run naphtha and 56 gallons of the last runnings, the remainder (634 gallons) being a residuum of no value except for mixture with dead oil; and the 897 gallons of once run naphtha yields, after it has been purified with sulphuric acid, 301 gallons of 90 per cent. benzole, 195 gallons of 40 per cent., 237 gallons of solvent naphtha, 12 gallons of last runnings, and 152 gallons of residuum.

The 40 per cent. benzole contains also 50 per cent. of volatile oil, chiefly toluole, which distils over between 212° and 248°. This is the oil which is preferred at the present time for the manufacture of coal-tar colours. The several products which are thus obtained in the distillation of coal tar are upon the table before you, and roughly speaking, the proportions per 10,000 gallons of crude tar and their commercial values are as follows:—

40 per cent. benzole	34·4	gals.,	worth	2s. 4d.	per gal.
90 per cent "	53·1	"	"	2s. 0d.	"
Solvent naphtha	41·8	"		1s. 9d. to 2s.	"
Last runnings	12·0	"	"	0s. 9d.	"
Dead oil	3018·7	"	"	0s. 1d.	"
Pitch	36	tons	"	45s. 0d.	per ton

Before rectification the crude naphtha is worth about 1s. per gallon, and the light oil about 6d., the two together fetching 9d. or 10d. a gallon; and once run naphtha is worth 1s. 6d. a gallon. Two samples of this oil from different distillers yielded by fractional distillation the following percentage of proportions of oil at different temperatures:—

	Sample 1.	Sample 2.
Up to 212° Fahr.	15·0	17·5
From 212° to 248°	44·0	42·0
" 248° to 264°	8·0	8·5
" 264° to 300°	13·0	13·0
" 300° to 320°	5·5	4·5
Residuum	14·5	14·5
	100·0	100·0

The samples therefore in commerce from good markets may be regarded as of pretty uniform quality.

In Scotland the method of distilling coal tar is a little different from what it is in England, and this arises from the circumstance that the Scotch cannels yield a tar which is so rich in the volatile naphthas that it is not altogether safe to distil the tar from a still with a naked fire. The tar, therefore, is first submitted to the action of high pressure steam, which is blown into it until the more volatile products are passed off. In this way from 7 to 13 per cent. of *crude or rough naphtha* is obtained with a gravity of about 930. The residuum is called boiled tar, and is distilled with a naked fire. It thus yields from 6 to 7½ per cent. of a *light oil* called *pitch oil* or *torch oil*, which has a specific gravity of from 973 to 976. The next runnings, which amount to from 27 to 30 per cent. of the boiled tar, are generally heavier than water, and are called *heavy pitch oil*, and they constitute the great bulk of the product.

The several products of coal tar are thus used in the arts:—

Coal tar is itself employed as a rough varnish for iron, and in Scotland the *boiled tar* is extensively used for covering woodwork, etc.

Light oil and crude naphtha are either redistilled for procuring benzole and toluole, as I have already explained, or they are employed for making common black varnish or for burning in naphtha lamps. In this country they are for the most part distilled, but in Scotland they are largely used in a lamp called the *foundry lamp*. It is an enlarged form of a lamp which was patented many years ago by Mr Beale, and it consists of a chamber supplied with naphtha, and having a nozzle or jet for directing a blast of air through it. The chamber is covered with a bell with a large hole in the top of it. When the naphtha is lighted and the bell put upon it, the blast of air forces the vapour of the burning naphtha through the hole in the top of the bell, and thus produces an enormously large volume of flame. The light is equal to at least a dozen gas jets, and the cost of it is said to be a penny a night. It is very generally used in the foundries, the ship yards, and other large workshops of Scotland.

Solvent naphtha is a colourless spirit which is chiefly employed for dissolving india-rubber for waterproofing, and resins or pitch for varnishes.

The *last runnings* are also used for varnishes, for making a superior lamp black called *spirit black*, and for burning in Holliday's lamp, which is the common naphtha lamp of the streets. It is an ingenious contrivance for converting the naphtha into vapour by means of a mass of heated metal, and spreading it out in a star-like form.

I have already alluded to the use of coal naphtha as a means of increasing the illuminating power of common 12 or 14 candle gas, and have shown that with a moderately good naphtha, which yields about seven grains of vapour to every cubic foot of gas, the illuminating power may be increased about 60 per cent. Considering that naphtha is now becoming a drug in the market, from the waning of fashion in respect of coal-tar colours, it may be worth while to encourage its use as a naphthaliser, rather than to yield to the public clamour for cannel gas. I have long thought that gas, as well as water, should be dealt with at the consumers' houses, when in either case it is required to be of unusual quality.

The creosote, or dead oil of coal-tar, is used almost entirely for the preservation of timber, and at the present moment, in the stagnant condition of railway business, it is almost unsaleable. I apprehend, however, that it is valuable as a fuel, and that it will ere long be used in steam furnaces. Already there are several patents for its application in this manner, and experiments are now being conducted at Woolwich with the view of ascertaining its practical and economical capabilities. The contrivances which appear to offer the largest prospects of success, are those which deliver the oil into the furnace in the form of a spray or vapour, by means of a jet of steam or blast of hot air; and it is said that the heating power of the oil is from 2½ to 3 times that of a similar weight of coal.

In applying the oil to the preservation of timber, it is necessary that it should be forced deeply into the tissue of the wood. The method employed by the best operators is to place the timber in large wrought-iron cylinders, and then to exhaust it of air and moisture as completely as possible by creating a vacuum. After a time the dead oil, heated to a temperature of 120° Fahr., and thus made as fluid as possible, is let into the cylinder. Pressure is then put upon it until the oil is forced into the wood with a power of 150 lbs. upon the inch. In about three hours the wood absorbs the prescribed amount of creosote, which, with the best houses, is never less than from 30 lbs. to 50 lbs. of creosote to a load of 50 cubic feet of timber; every cubic foot of timber has, therefore, taken up from 6 lbs. to 10 lbs. of oil.

The preservative power of the dead oil is partly due to the antiseptic properties of the creosote, and partly to its filling up the pores of the wood with an oil which gradually resinifies and excludes air and moisture. Different views are entertained of the quality of creosote which is best suited for this purpose. In the contracts which I have prepared for the Indian railway works, I have prescribed that the creosote should have the following properties:—"It should have a density between 1,045 and 1,055; it should not deposit any crystalline matter at a temperature of 40° Fahr.; it should yield not less than 5 per cent. of crude carbolic acid to a solution of caustic potash of the density of 1,070 (14° Twaddle); and it should furnish 90 per cent. of liquid oil when distilled to the temperature of 600° Fahr." The contracts, which I have lately seen, for the Dutch Government, prescribe that the creosote shall be clear, and shall not deposit more than 40 per cent of naphthaline when cooled to the temperature of 32°, and kept at that temperature for 24 hours. Here are specimens of creosote from country tar which fully realise those properties; but this sample from London tar is almost solid at 32°.

Another use to which dead oil has lately been put is the preparation of a dip for washing sheep. It was patented by Mr. McDougall in 1860, and is made by heating together two parts by weight of dead oil with one of a solution of caustic soda of 50° Twaddle (sp.gr. 1,250) which contains about 15 per cent. of soda; and to this is added one part of tallow, fat, or other saponifiable substance. The mixture which is thus obtained has the appearance of a very dark soft soap, and it is either smeared upon the skin of the animal, or dissolved in water and used as a wash.

The *greasy matter or green oil*, which follows the dead oil in the distillation of coal tar, is used for making railway grease, with resin, oil, etc.; and the pitch which is the residual product of the distillation is largely employed for all sorts of purposes.

(To be continued.)

CHEMICAL PRIZES.

The custom of offering prizes for the successful solution of problems in chemistry, is one which deserves some attention in this country. On the Continent it is not rare to find chemical manufacturers, or directors of large works where a particular operation in testing or analysis has to be performed many times a day, resort to the expedient of publicly offering a prize for the discovery of a process which shall fulfil certain prescribed conditions. This plan has several advantages. In most large chemical works there is one operation which has to be performed almost hourly, and on the accuracy and dispatch of which much money is risked. It is impossible for any chemist attached to the establishment to be acquainted with all the improvements which are being made in a particular process, and his time is generally too much occupied in routine work, to admit of his carrying out the experiments necessary for the working out of minor details. A publication of the difficulty through the agency of a scientific press, is often sufficient to bring forward many good suggestions, and we can point to our own "Notes and Queries" column in illustration of the readiness with which chemists will assist each other in the elucidation of a technical problem.

This plan, however, of asking for advice is obviously of limited use. Although any of our readers would, we doubt not, be willing to give a querist information when he could do so by simply writing a letter to our columns, few would feel inclined to enter gratuitously into an investigation, and occupy their time for some weeks, to solve a technical problem simply for the benefit of a manufacturing firm. Hence the system of offering prizes appears especially appropriate, and in many instances the expenditure of £50 or £100 has secured to the manufacturer an analytical process which has saved the outlay many times in the year.

A prize of 300 thalers has just been offered by the Mansfield Copper Mining Company, Eisleben, for the discovery of a process for the estimation of copper in the Mansfield schist, the following conditions being fulfilled:—The process must not occupy more than five or six hours, including all operations. One person must be able, without much exertion, to finish at least eighteen analyses daily. The differences between the analyses must fall within narrow limits. All the claims for the prize, with full particulars, are to be sent to the Company before the end of December next. The decision will be announced on June 30, 1868, and if any of the processes sent in fulfil the requisite conditions the prize will be forthwith paid, but should several be found which appear eligible, to the best will be awarded 200 and to the second 100 thalers. If, however, no satisfactory process is discovered, it is intended that the prize shall be divided amongst those who have sent in the best investigations on the subject. The successful processes are to remain the property of the Company.

A few months ago we drew attention to the fact that one of the largest and most extensive manufacturers of tartaric acid would give £100 as a reward to any one who would discover a satisfactory method of determining, directly, the quantity of crystallisable tartaric acid present in tartars, in a sufficiently ready manner to be applicable to commercial analysis; the name of this firm has not been made public, but should any of our readers wish to know more accurately the conditions of the prize, they can address a private letter to our office.

It is, perhaps, not premature to mention that in all probability a money prize will shortly be offered through the Chemical News, for the discovery of a somewhat similar process of technical analysis, connected with an important branch of manufacture. The details are now under consideration.

ON THE QUANTIVALENCE OF CHLORINE AND OTHER REPUTED MONADS.

BY JOHN A. R. NEWLANDS, F.C.S.

The difficulties with regard to the formation of certain compounds which have been lately pointed out by Dr. Odling and others, seem to indicate that chlorine and other reputed monads must really possess latent bonds, and be capable of acting as triads, etc., under peculiar circumstances. Thus, in the union of ammonia and hydrochloric acid, the nitrogen acts as a pentad, and, if we regard the hydrogen and chlorine in hydrochloric acid as monads, the hydrochloric acid must be decomposed in order that its hydrogen and chlorine may unite with nitrogen, an element for which they have but slight affinity. On the contrary, if we admit that chlorine may act as a triad (just as iodine does in ICl_3), we may suppose that two of its bonds are latent in HCl, and that on bringing HCl in contact with NH_3 the two latent bonds of the Cl unite with the two latent bonds of the N. Or, we may suppose that the Cl in HCl remains a monad, and that the H acts as a triad having two latent bonds, which on the approach of NH_3 serve to unite it with the two latent bonds of the N. This latter view of the constitution of sal-ammoniac, and similar bodies, is, perhaps, preferable, as it represents the H of the HCl as directly united to the N of the NH_3.

It may be said that if this view holds good regarding the union of HCl and NH_3, it ought to hold good with regard to the union of ethylic iodide, C_2H_5I, with NH_3, and that either the I or one of the five atoms of H in the ethylic iodide acts by virtue of two latent bonds, and so unites itself with the two latent bonds of N in NH_3.

For the purpose of the present communication, I admit, in accordance with the views of Frankland and others, that multivalent elements may have their apparent quantivalence reduced by successive pairs of bonds becoming latent. Thus N is a pentad in NH_4Cl, a triad in NH_3, and a monad in ON_2, and C is a tetrad in CH_4 and a dyad in CO, still N is always a perissad and C always remains an artiad.

These views, however, may be extended by considering it possible for a monad, by the development of two or more pairs of latent bonds, to become a triad, a pentad, a heptad, etc., still, however, remaining a perissad. In like manner we may consider it possible for a dyad, by the development of two or more pairs of latent bonds, to become a tetrad, a hexad, an octad, etc., still, however, remaining an artiad.

So that all perissads may really have the same number of bonds (that number being an odd number), and may differ only by the number of pairs of latent bonds which their atoms respectively possess under ordinary circumstances. Again, all artiads may really have the same number of bonds (that number being an even number) and may differ only by the number of pairs of latent bonds which their atoms respectively possess under ordinary circumstances.

Thus I, which is a monad in KI, becomes a triad in in ICl_3; B, which is a triad in BF_3, becomes a pentad in BKF_4; Pt, which is a dyad in $PtCl_2$, becomes a tetrad in $PtCl_4$, and an octad in PtK_2Cl_6; and Si, which is a tetrad in SiF_4, becomes an octad in SiK_2F_6.

In the. following table certain compounds of the monads with O, etc., are given, which are analogous to the compounds of triads and pentads placed alongside of them. I am aware that too great stress must not be laid upon such facts, inasmuch as the number of atoms of O combining with an element, affords us no absolute proof of its quantivalence:—

H_2O	Cl_2O	K_2O	N_2O
H_2O_2	K_2O_2	N_2O_2	
Cl_2O_3	N_2O_3	P_2O_3	
Cl_2O_4	K_2O_4	N_2O_4	
I_2O_5	N_2O_5	P_2O_5	
$KClO_2$.....	KNO_2		
$HClO_3$.....	HIO_3	HNO_3.....	HPO_3
ICl_3	PCl_3		

This table might easily be enlarged to a considerable extent, but I think the above will be sufficient to render the view probable that reputed monads may, under certain circumstances, play the part of triads and pentads. This is certainly the case with I in ICl_3, and the privilege we accord to I we cannot well refuse to Cl, Br, or even to H itself.

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FOREIGN SCIENCE.

(From our own Correspondent.)

Baron Liebig's Artificial Milk; Death of the Patients.—Liquid Diffusion applied to the Extraction of Cane Sugar.—Effect of Tobacco and Snuff in Impairing Memory.

Paris, July 3d, 1867.

Artificial Milk.—At the last meeting of the Academy of Medicine, M. Giboust, Professor at the School of Pharmacy, read a paper which we cannot help noticing. He called the attention of the medical world to the description given of the artificial milk invented by Baron Von Liebig, and regretted very much being obliged to enter into a controversy with him. After having reminded the assembly of the composition of this milk, and insisting upon the difficulties attending the preparation of such aliments in places where it might be most necessary, such as with wetnurses or small families, M. Giboust added that we have at our disposal a natural product which more nearly resembles human milk than does a mixture of cow's milk, flour, malt, lactate and butyrate of potash. It is cow's milk itself. On an average, human milk contains a little more water, more sugar of milk, less butter and caseine than cow's milk. Thus, by taking the latter, and adding a little sugar and a fifth of its weight of water, we have an aliment, at the disposal of everybody, forming a better substitute for human milk than any artificial compound.

M. Depaul, on his part, declared that he undertook experiments on new-born children, to examine the effects of this artificial milk, the taste of which was, by the bye, less agreeable than that of natural milk. Four children were tried. The first two were twins, and born prematurely. In spite of the care bestowed on them, and the nourishment by the artificial milk, they died in two days. The third, born at full time, weighed 3 kilogs. 370 grammes; the mother was ill. The nourishment given was that of artificial milk. At the end of two days, the dejections became green, and on this day the child perished. The fourth infant, born under the same conditions, and nourished with the same aliment, died after four days. M. Wurtz promised to write to Baron Von Liebig, to obtain more precise details on the preparation of this milk.

Diffusion, by M. Robert of Seelswitz, applied in the East Indies for the extraction of cane-sugar.—Previous to a voyage to the East Indies, he traversed Germany and Austria, with the intention of studying the progress made in the manufacture of beet-root sugar in those countries, and to see how far the method could be introduced into the East Indies, for the manufacture of cane-sugar. This process of extracting the juice of the beet-root by means of diffusion was invented by M. Jules Robert, of the firm of Robert and Co., proprietors of works situated in Moravia. Struck with the simplicity of this process, which with cheap machinery and scarcely any wear and tear, gives a finer and more abundant juice at less cost than the usual process, he consulted M. Jules Robert on the possibility of applying this method to the sugar in the East Indies, and the only difficulty was, that of having a machine to cut the cane into the required lengths, inasmuch as cane, from its hardness, presented greater resistance. A cane cutting machine was constructed at Seelswitz, which was found to work successfully on maize stalks. Four engines were sent to the East Indies, along with a triple evaporator. Up to the time of sending off the samples to the Champ de Mars, 1,500 tons of sugar-cane had been prepared. A steam engine of 12-horse power, four cane cutters, and diffusing apparatus in wood will suffice for 70 tons of sugar-cane. The advantages have been found to be:—1. Less force is necessary for equal weights of cane, to cut the cane into slices than to drive rolling machines. 2. That diffusion extracts a juice purer than that obtained by mills. 3. That diffusion extracts from the same quantity of cane 20 per cent. more juice than by mills, as is proved by the comparison of the weight of the stalks and the residues of the diffusion. 4. As the cane contains less pectose than the beet-root, the temperature can be allowed, without inconvenience, to exceed 50°C. and even to amount to 70°C. This is important, because diffusion is quicker at a higher temperature.

Tobacco.—The Abbé Migne has just addressed a letter to a very honourable director of one of the great seminaries of Paris, condemning the use of tobacco and snuff. This letter furnishes us with an opportunity of relating a fact that is personal to us. Several times in our youth and riper age we have taken up and discarded the use of the snuffbox. In 1861, when writing our mathematical treatises, during our labours with M. Lindelof, for the calculation of variations, and when we commenced the editing of our lectures on analytic mechanics, we used snuff to excess, taking 20 to 25 grammes per day, incessantly having recourse to the fatal box and snuffing up the dangerous stimulant. The effect of this was, on the one hand, the stiffening of the nervous system, which we could not account for; on the other hand, a rapid loss of memory, not only of the present but of the past. We had learned several languages by their roots, and our memory was often at a loss for a word. Frightened at this considerable loss, we resolved in September, 1861, to renounce the use of snuff and cigars for ever. This resolution was the commencement of a veritable restoration to health and spirits, and our memory recovered all its sensibility and force. The same thing happened to M. Dubrunfaut, the celebrated chemist, in renouncing the use of tobacco. We do not hesitate in saying that for one moderate snuff-taker or smoker there are 99 who use tobacco to excess.

F. Moigno.

Artificial Milk; Manufacture of it in England; Analysis Disputed.—Report of the Weights, Measures, and Coins Committee; Official Recommendations.—New process of Evaporation.—Improvements in Electrotyping Copper.

Paris, July 10, 1867.

At the last meeting of the Academy of Medicine two protestations were again made against the artificial milk of Baron Liebig. The first is by M. Boudet. The second objection was raised by M. Poggiale, in the following terms:—Baron Liebig tells us that his artificial milk is manufactured on a large scale in England; that there is an Industrial Company which carries on the business on a large scale. The important point to be considered is, whether this new product differs widely in composition from human milk. Baron Liebig has taken as the basis of his preparation that of a milk analysed by a German chemist, named Haideln. This analysis is very old, and when it was made scientific men were not provided with the means which are now at their disposal; and the results have, therefore, been contested.

Nevertheless, in accepting, and taking for granted the truth of Haidelu's analysis, we observe that in the formula of M. Liebig the plastic elements are represented by 10, and the respiratory elements by 38, instead of 24 required by the analysis of M. Haideln. The composition of the artificial milk thus differs much from the true proportions, and in this respect it is inferior in quality to natural milk. In it the fatty matters are replaced by starch. But glycose, it is well known, gives less heat, and therefore cannot replace advantageously the substances we have mentioned. We may also ask why does Baron Liebig add bicarbonate of potash? this new element gives to the milk a taste far from agreeable. In a word, this artificial milk differs from the natural, by its odour, taste, colour, and chemical composition. We should certainly recommend, in preference to it, milk which is most similar in composition to human milk, namely, cows', goats', and asses' milk.

The Committee of Weights, Measures, and Coins, at the Universal Exhibition of Paris, 1867, have made their official report relative to units of measure and weight. The commission advance the following measures:—The prompt substitution, in all its integrity, of the metric system, for the old systems of weights and measures, as it is practically adopted in several other countries in the west of Europe. This system, introduced and legalised optionally, cannot be at once rendered imperative to the exclusion of every other system. A certain delay is necessary for the change; and the different nations are alone capable of fixing its duration. Let us observe in the meantime that experience in several countries has proved that a too long delay does not have the effect of sensibly facilitating the accomplishment of this task. Thus it is desirable that Governments take, henceforth, the following measures, viz:—

1. To order the teaching of the metric system in public schools, and to require that it should form part of the public examinations.

2. To introduce its use into scientific publications, in public statistics, in postal arrangements, in the custom houses, and other branches of Government administration.

3. The commission does not consider, as appertaining to its mission, the duty of making standards the exact prototypes of those of Paris. The Government of each country will take upon itself the verification of each of these standards.

The commission declares that the present report contains the expression of its deliberations and conclusions. It expresses a wish that different nations will yield to the solicitations of science and the manifestations of opinion.

A new evaporating process is carried on by M. E. Parion, at Wardrecques, St. Omer. This process is carried on essentially by the renewal of the surface of the liquid exposed, in the state of fine division, in contact with the air, or the products of the combustion according as the evaporation should take place, with or without the aid of artificial heat. When the evaporation takes place by the aid of the temperature of the air alone, the liquids are divided into a small shower exposed to the wind and sun. By maintaining them in this state, we obtain in a small space, and one easy to cover when necessary, the same results as the concentrating basins and the graduating buildings give, at great cost, and with a vast extent of land.

In the case of the employment of artificial heat, the waste heat from chimneys of factories is utilised in preference, and in the absence of this any source of heat is employed if the products obtained have a sufficient value to pay the expenses.

The reduced model of the apparatus for evaporating by the aid of artificial heat, is exhibited at the Universal Exhibition class 50, group VI.; moreover, a small apparatus is at work in the annexe at Billancourt.

M. Bouillet has discovered a remedy for a defect in the electro deposition of copper, which is sometimes seriously injurious, viz., its brittleness. He has found that a very small quantity of gelatin dissolved in the bath gives a copper of nearly equal malleability to rolled copper, whereas the pure bath only gives a porous defective metal like cast copper. The relative specific gravities of copper in different states are: cast copper, 8·78; laminated copper, 8·95; galvanic copper, 8·86. Gutta-percha moulds are exclusively used by the firm of Christophle and Co. They are applied, either cold by pressure with a lever, or by the hand. The mould is rendered conductive either by black-lead or silver, reduced from the nitrate by nascent hydrogen.

F. Moigno.

Manufacture of starch, utilisation of the waste.—Phenic acid, its manufacture and properties.

Paris, July 17, 1867.

More than 100 tons of wheat are annually employed in the fabrication of starch in France. M. L. Maiche, of Paris, now proposes to utilise the waste as an aliment.

The best wheat only contains 55 per cent. of starch, while rice of the most ordinary description contains 85 per cent.; maize and buck-wheat also contain a considerable proportion. The difficulty consists in the separation of foreign matters, such as bran, cellulose, gluten, etc., contained in the pulp of the grains. Having isolated small quantities of cellular tissue and other substances, the author found that the specific gravity of these bodies was much less than that of starch.

If raw starch is placed in water, a small quantity of almost pure starch is deposited, but the bulk only falls mixed with the different substances above mentioned; these, although specifically lighter, are relatively more heavy, being much larger than the starch grains. M. Maiche takes advantage of difference of specific gravity, in order to obtain a complete separation; he makes use of centrifugal force, by which the specifically heavier bodies are thrown farthest off. The mode of operation is the following:—A mixture of raw starch and two parts of water is introduced into a sort of drum of copper, turning on its axis at the rate of 1,000 to 1,200 revolutions per minute; as soon as the velocity attains 450 turns, the starch commences to be separated, and collects in a compact mass, adhering to the sides of the vessel; all the impurities remain in the water, in the centre, which is easily drawn off, while the perfectly white and pure starch can be removed in lumps. All amylaceous matters can be treated by this method, and the extraction of the starch which formerly required several weeks, now takes place in a few minutes. The return is much greater, for 100 kilogs. of rice, costing less than 100 kilogs. of wheat, give more than twenty francs worth of starch. There is then no reason for employing, in the manufacture of starch, wheat which gives the best and most nutritious flour, and the chief principle of nutrition of which, the gluten, is almost entirely lost by the process actually employed.

We give an extract from the excellent lecture given at the Society of Encouragement for National Industry, on *Phenic Acid and its Compounds*, by Dr. Crace-Calvert. It is well known that when coal is heated to a low degree in retorts or distillatory apparatus it gives off substances that can be classed into four groups.

1. Gaseous products furnishing light, heat, and motive power.
2. Water containing ammoniacal salts, which can be purified by well-known chemical means, and utilised in agriculture and in the industrial and medical arts.
3. A thick, black, sticky mass of a repulsive odour, to which the name of *tar* has been given, and which passes over along with the above-named products.
4. A solid porous body, known by everybody as "coke," which remains in the retorts.

When tar is submitted to distillation, first water is obtained, then products which pass over with this liquid, but which, lighter than it, float on the surface, and are therefore termed the *light coal oils*. Lastly, there is distilled a compound heavier than water, and consequently called *heavy oil*.

It was about the year 1837 that these heavy oils were first used for the preservation of sleepers according to Bethell's process. M. Farestier, engineer-in-chief of the department of the Vendée, conjointly with M. Marin, engineer, published a very remarkable and very complete work on the creosoting of wood and its preservation for twelve, fifteen, or twenty years from decay and the ravages of water and the teredo. There remains in the retort a substance fusible at the high temperature attained after the oils have passed over. This is asphalte or bitumen, which hardens on cooling. The distinguished lecturer then proceeded to "phenic acid," stating that M. Laurent, the great French chemist, was the first to indicate the method of extracting phenic acid from tar. It consisted in submitting the light coal oils to a partial distillation, and treating by a concentrated solution of potash, the products distilling at a temperature between 160° and 200° C.

In 1847 Mr. Mansfield indicated another method of treating the heavy oils by caustic alkalies, and towards 1856 M. Bobœuf made known his modified process of M. Laurent. This consists chiefly in the use of caustic soda instead of potash, and treating the whole of the light oils, instead of a portion, by Laurent's method; but this only gave an impure acid, yet, in a commercial point of view, it was a progress. Of a similar nature were the products manufactured by Mr. John Bethel, since 1847, under the direction of Mr. Calvert. They were used for several purposes, either for the production of picric acid, or for transforming tannic acid into gallic acid, or for preserving organic substances from putrefaction. M. Bobœuf used it also very extensively for this purpose.

In 1859, M. Marmas, of the firm of Guinon, Marmas, and Bonnet, of Lyons, came to Manchester, and requested Mr. Calvert to furnish a purer picric acid than that hitherto made, showing him, at the same time, a product white and crystalline, which they furnished as a type. Mr. Calvert made new researches, and discovered that the most favourable mode of preparation was not to treat the coal oils with concentrated alkalies; but, on the contrary, to treat impure benzine of commerce, or naphtha, by weak alkaline solutions.

By this means, a blackish semi-fluid product was obtained, a little heavier than water, having a sp. gr. of 1·06, containing 50 per cent. of real phenic acid, and which acid he separated partly by the aid of distillation. After further researches, Mr. Calvert produced white phenic acid in detached crystals, melting between 26° and 27° C. Towards the end of last year he discovered a process by which he produces phenic acid free from all unpleasant taste; and what deserves remark is, that it is as pure, though it is made from coal tar, as if it had been artificially produced by the aid of reactions recently discovered by MM. Wurtz and Kékulé.

F. Moigno.

PARIS EXHIBITION OF 1867.

(From our Special Correspondent.)

Copyright of the Medals.—An Explanation Wanted.—Awards of Gold and Silver Medals.—Extract of Meat.

In our last communication we felt ourselves bound to express our opinion somewhat strongly regarding some of the more salient and least agreeable of the features of the Paris Exhibition. Grumbling comes so naturally to us English, and is therefore so completely to be expected from a "Special Correspondent," that, feeling a little ashamed to think we had fallen so easily into the conventional groove, we determined to complain no more, but proceed like Hotspur in the *Telegraph* to point out the probable winners. At this moment the following circular was placed before us:—

"*Notice to Exhibitors.—The Medals.*

"The Jurors' Awards, and Medals, and Honourable Mentions will be published in course of a few days, and duly notified to the successful Exhibitors.

"The copyright of the Medal being invested in us by the Imperial Commission, and fully secured for the United Kingdom (13 and 14 Vic., cap. 104, and at Stationers' Hall), we have the exclusive right to publish fac-simile copies of the Medal, as well as to supply all reproductions of the design by every process of printing, photography, embossing, electrotyping, or otherwise.

"Exhibitors, desirous of introducing the medal on their show cards, labels, or other printing, should favour us with instructions immediately on receipt of notice of award.

"J. M. Johnson and Sons,
Sole Concessionaires."

This precious document (precious alike in style and spirit), in one sense is to be received with enthusiasm. Henceforth who shall dare to call us "a nation of shop keepers?" The writer contends that to sell any firm such a monopoly is eminently disgraceful. Every exhibitor then who obtains a Medal or Honourable Mention is to be forced to wait as long for his bill heads, blank invoices, or headed papers, as "MM. the Concessionaires" please, and will also be compelled to pay any price for them they may choose to demand. "MM. the Concessionaires" may, for auhgt we know or care, be absolutely incapable of presuming unduly on their exceptional position; but, if they do not, it will solely be due to their good feeling and moderation. It is more than ever evident that everything connected with the Exhibition is "sold," especially the "Exhibitors." It is generally believed, however, that "MM. the Concessionaires" will not have the resources of their establishment too heavily taxed. "In things gone by we see the archetypes of things that are to come." *Verbum sap.*

One word more; the readers of this Journal will have observed that in its advertisement columns it is not unusual to have illustrations of Exhibition Medals. Is it then to be understood that if any English Firm, after having won a medal, should wish to have a cut of it as a heading to their advertisement, such firm will have to pay a tax to "MM. the Concessionaires"? And are we to understand that in default they will have infringed the rights ". . . . fully secured for the United Kingdom (13 and 14 Vic., cap. 104, *and at Stationers' Hall*)"? We call upon the authors of this wonderful parenthesis to define distinctly the position of advertisers in this matter. If this tax is intended to be enforced it is to be hoped that the English Medalists will refuse to lend themselves to what can only be characterised as a most odious monopoly.

It is with pain that your correspondent has felt called upon, by an absolute sense of duty, to make some of the remarks to be found in this and the last letter. It is desired, therefore, before proceeding with the criticisms upon the contents of the Exhibition, to state that, despite what has been said, the writer is deeply and most favourably impressed with the vast majority of his French experience. Red tape is always red tape; the red tapists, like Talleyrand's fair enemy, have but one defect, they are insupportable. The French *people* in general we cannot speak too highly of. It has been said that the grand old French politeness is disappearing; we deny it. Go where we would, we found not only civility and politeness, but kindness and good feeling. Depend upon it, where English people do not meet good treatment in France, it is because by their insular pride and gaucherie they render good treatment impossible. How often do we hear our countrymen use the idiotic expression, "I do not like foreigners." Your true cosmopolitan does not care whether the man he meets is a foreigner or not; if he behaves well he should receive corresponding treatment. Apologising, as in duty bound, for this digression, we resume our study of the objects exhibited. As we stated in our last letter but one, we shall endeavour to take the cases as they are numbered in the Exhibition Catalogue.

In addition to cod-liver oil, Messrs. Allen and Hanbury, of Plough-court, exhibit Liebig's extract of meat, manufactured in Australia at the establishment of Mr. Robert Tooth, of Sydney, who was the first person in Australia to establish works for the manufacture of this article. Up to the present time at Mr. Tooth's works they only employ beef as the raw material. These works alone produce no less at the present time, than between 8,000 and 10,000 lbs. per month, and the resources of the manufactory are such that they are capable of yielding a much larger quantity than this.

When we reflect that 10,000 lbs. of extract represent no less than 160,000 lbs. of the lean flesh, we are in a position at once to see the amount of work that must be gone through in a short time, in order that so perishable a raw material may not be allowed to spoil.

This extract of meat is so rapidly finding its way into our kitchens, and is becoming so much used as "stock" for soups and as the basis of gravies, that it bids fair to become one of the most valuable of our imports, and it is interesting to see that the idea of concentrating a large amount of nutritious matter in a small compass is being worked out in other directions. Powdered meat is now being imported, and if of really high class quality will probably be used in many cases where the employment of the extract would be inadmissible.

We would earnestly, however, caution the manufacturers of these essences or extracts, and powders, against allowing ever so small a quantity of bad quality to find its way into the market. Such substances are precisely those which people at first are slow to appreciate and adopt, and one or two bad samples, by disgusting the purchasers, will retard the sale within a considerable area.

We are now able to announce that no less than 88 gold, 325 silver, and 400 bronze medals have been gained by Bristol firms, and that 270 of those sources of all despair to exhibitors, viz., honourable mentions, have also been inflicted.

Among the exhibitors of chemical and pharmaceutical products the following have obtained gold medals:—

C. Allhusen and Sons, Tyne Chemical Works, Gateshead, and Newcastle-upon-Tyne, bicarbonate, sulphate, and crystals of soda, alkali, caustic soda, chloride of lime, etc.

W. Gossage and Sons, Widnes, near Warrington, for soaps, and silicates used in making soaps.

The Jarrow Chemical Company, South Shields, for specimens of chemical products.

James Muspratt and Sons, 41, Oldhall-street, Liverpool, for chemical products connected with the manufacture of alkali.

Howard and Sons, Stratford, salts of quinine and other chemicals.

Price's Patent Candle Company (Limited), Belmont Works, Battersea, for candles, night-lights, oils, soaps, and glycerin.

James Young, Chemical Works, Bathgate, Scotland, for solid paraffin, candles, oils, etc.

Johnson, Matthey, and Co., Hatton-garden, for articles in platinum and other precious metals. In this instance there has been an award of what, in University language, may be called a "double first," as they have also received a gold medal in group 6, as well as a silver medal.

It is a most invidious task to criticise awards of medals in these cases. We all know how enraged every exhibitor becomes who does not get a medal. We must remember, however, that, in the first place, many of our best houses, disgusted with the labour, expense, and loss of time entailed by exhibiting, had refused to send anything to Paris. In the next place, there is no doubt that a very large proportion of the chemicals and drugs exhibited were not above mediocrity.

We regret that so much of the small space which is weekly at our disposal has of necessity been spent in discussions not immediately connected with the chemistry of the Exhibition. Now, however, that the awards are given, we hope that nothing further will arise to force us to occupy our letters with matters unconnected with science.

The following firms have obtained Silver Medals:—Bailey, Bewicke, British Seaweed, Calvert, H. B. Condy, Cow, Hill, Denton and Jutsum, Demuth, Field, Gaskell, D. and W. Gibbs, Hopkin and Williams, Hurlet Alum Company, Johnson and Matthey, Knight, Macfarlane, Mander Brothers, Mawson, Ogleby, Parkes, Rose, Smith, Tudor, Walker Alkali Company, Warne, Wilkinson. Bronze Medals:—Adams, May, and Baker (Class 30), Britannia Rubber, Burgoyne, Bush, Calley, Clark, W. Cook and Co., Danley, Davy Yates and Routledge, Day and Martin, Dodge, Garrod, Goodwin, Green, Haas and Co., Hodgson and Simpson, Holland, Hosegood, Huskisson, J. Jarrow, Langton and Bicknells, Lamb and Sterry, Lange and Moselle, Lowe (Manchester), M'Dougal, M'Kay, Mason, Nimmo, J. N. Parker and Co., Pulford, W. Ransome, Rogers, Rumsey, Squire, Stephens, Talbot and Alder, W. Taylor and Co., Turner and Son, Wandle, Waring.

Class 45.—Specimens of the Chemical Processes used in Bleaching, Dyeing, Printing, and Dressing.—Silver Medal:

—Hands, Ripley. Bronze Medal:—Barlow, Dickens, Howe. Honourable Mention:—Whincup.

Messrs. C. Allhusen and Sons, of the Tyne Chemical Works, Gateshead, and of Newcastle-upon-Tyne, obtained a medal in Class II. Section A. for their display in the English Exhibition of 1862, and that they fully sustain their old position is evident from their present exhibition illustrating the manufacture of soda. They show bicarbonate, sulphate, and crystals of soda, alkali, caustic soda, of 60 and 70 per cent., chloride of lime, etc. As we stated in our last, they have obtained a gold medal for the superior quality of their productions.

It is both interesting and instructive to study the mechanical and chemical requirements of one of these colossal chemical manufactories. We all know the enormous space required for vitriol chambers, and the absorption apparatus for making chloride of lime. In addition to this, which alone forms an immense plant, there are actually saw-mills, common and fire-brick manufactories, coke-ovens, and gas-works, besides, of course, an extensive cooperage. Such is the nature of Messrs. Allhusen and Sons' "plant," and they are, we believe, by no means the largest manufacturers of their class. After this we shall not be surprised to hear that their weekly consumption of raw material is as follows:—

Coal	2,250	tons.
Pyrites	350	"
Nitrate of soda	10	"
Chalk	900	"
Salt	450	"
Manganese	100	"
Limestone	125	"

The weekly production of sulphate of soda is 500 tons, equivalent to 375 tons of soda ash or unrefined alkali; which is in subsequent processes converted into quantities varying with the demand, and not exceeding separately 450 tons of crystals of soda, 150 tons refined alkali, 100 tons bicarbonate soda, 30 tons caustic soda, and 110 tons chloride of lime.

So much has been written and said about the soda process, that it is entirely unnecessary to enter upon the subject here, except to say that in spite of the innumerable attempts that have been made to supersede Leblanc's method, it is still adhered to by all manufacturers of soda. It is true that Mr. W. Gossage, of Warrington (whose name has been so long and intimately connected with the alkali manufacture), has devised a new plan which appears to have the germs of success in it; but whether it is absolutely a commercial success, we are not at the present moment aware. This much is certain, that the new process cannot be in the hands of any one more aware of the difficulties lying in the way, or better prepared successfully to encounter them.

Vauquelin a very long time ago endeavoured to take advantage of a reaction which even appears to have been known to the alchemists, namely, that when chloride of sodium is heated to a high temperature in presence of silica and water vapour, hydrochloric acid is evolved and a silicate of sodium formed. The production of soda from this silicate is a problem that has attracted a vast number of chemists, but, up to the present time, unsuccessfully. Tilghmann in 1847, and Fritzsche in 1858, have both attacked the question, but, as far as we know, with no material advantage.

Mr. Gossage subjects the silica to the action of chloride of sodium *in the state of vapour*, and in an atmosphere of steam. This is effected by sending the steam and the vapour of the salt into a large tower lined with fire brick and filled with flints. The silicate of soda flows down, thus exposing a fresh surface of the flint to the action of the vapour. The silicate of soda has to be decomposed by either carbonic acid or lime. The details of this part of the process are not in our possession at present, and from the known difficulties which lie in the way, must be very ingenious and instructive; we hope, however, to be able to lay a complete account before our readers at no distant period.

Mr. Gossage has most deservedly had his labours recognised, he being one of the few English exhibitors of chemical products who have been awarded a gold medal.

Before proceeding any further in our notices of the English exhibitors, we must once more express our deep regret that none of the manufacturers of aniline dyes were represented. When we remember the part that England has taken in the discovery and investigation of coal tar colours, it is almost humiliating to see the brilliant cases of foreign manufacturers, whose only part in the matter has been to infringe our patents, and by deluging the market with cheap, and in most cases inferior colours, bringing the trade to the verge of ruin. This remark, be it understood, is not intended to apply to the French chemists, who, it must be admitted, have treated us with far more justice than our German brethren. We looked in vain for a specimen of the superb Britannia violet of the MM. Perkin, which for two years has been the most successful rival of Hofmann's violets. For a long time its extreme solubility in water (almost as great as ordinary "Hofmann" in spirit) made it stand alone, but we believe that M. Porrier's violet from methyl-aniline is also remarkable in that respect as well as for its brilliancy. A method has been adopted, we understand, for rendering Hofmann's violet soluble in water.

The cases of MM. Perrier and Cheppet Fils, and of the Fuchsine Company are, especially the latter, exceedingly interesting and well arranged, and the more we admired them the more vexed we were to see no rivals to them in the English department, especially when we knew how easy it would have been for Perkin and Nicholson and Co. to have beaten them.

The question has often been asked us, "Did the Fuchsine Company, ever bring out any colour of their own invention?" Perhaps some of your readers will be able to answer this.

The next case in order which we shall notice, is that of Messrs. Samuel Berger and Co., who display specimens of rice starch; but their case, which looks very dull, has only two dishes of starch and seven show cards in it, and therefore simply seems as a foil to the case of J. and J. Coleman, and Isaac Beckitt and Sons. Both those firms gained medals in the English Exhibition of 1862. The starches of all these makers appear of the finest possible quality. Messrs. Coleman and Co., and Messrs. Beckitt and Sons both show coloured starches of various shades. By means of these tinted starches muslins can be "got up" of any desired tint, and ladies can therefore have a muslin dress of a different colour every day, it being merely necessary to wash it and then stiffen with starch of the desired hue. We have been informed by those who have used them, that the results are all that can be desired.

Perhaps some of your readers may be unaware that starch can be dyed with the utmost facility, and without destroying the granules, by merely filtering a solution containing the desired colouring matter through a layer of the starch. When dried at a very moderate temperature, the so-called "starch lakes" are thus produced, and in brilliancy and soft beauty of tone they are perhaps unsurpassed. Unfortunately colours thus prepared have but little "body," and, what is perhaps worse, they are decidedly fugitive. Nevertheless for many purposes where a beautiful "mat" surface is required of velvety texture, they are very useful. Coloured confectionery may be cited as an instance where the starch lakes may be legitimately employed.

Messrs. Isaac Beckitt and Sons have had the wisdom to display, side by side with the coloured starches, muslins of corresponding tint prepared by their use. We would recommend these gentlemen to renew the muslins, and also the coloured starches, at moderate intervals, as they will not stand many months of glaring daylight, without undergoing deterioration.

We are not with certainty aware of the nature of the dyes used by these firms for the purpose of colouring their starches, but all the coal tar colours are admirably adapted for the purpose, as the colour is almost entirely absorbed when cold aqueous solutions are filtered through a moderately thick layer of the starch.

Group V.—Class 44: Chemical and Pharmaceutical Products.—Numbered 7 in the English catalogue (8 in the *Catalogue Général*), we find "Borwick, George, 34, Chiswell Street, London. Baking powder, chemicals, spices, etc.," and we are also referred to p. 132 of the appendix for further details. What can induce any one to exhibit baking powder we are really at a loss to imagine. It certainly is not a subject of any scientific interest, it is not a novelty, it is not interesting in appearance; its mere physical character offers no guarantee of its purity, and, in fact, its negative qualities preponderate so largely, that positively we are astonished that its exhibitor did not (like a great firm of blacking manufacturers) obtain a medal. And yet this baking powder, so insignificant in appearance, so uninteresting in a scientific point of view, so unfit, therefore for exhibition, is measured by a commercial standard, of far more importance than many objects that rivet our attention by their beauty or scientific interest. We have been credibly informed that a fortune has been made by its sale.

Then what is this baking powder out of which fortunes have been, or are to be made? Of the composition of the powder of Borwick we know nothing, but Cooley's powder is as follows:—Tartaric acid, ½ lb.; barcarbonate of soda and potato farina, or British arrow-root, of each ¾ lb. (each in powder); separately dry them *perfectly* by a very gentle heat, then mix them in a dry room, press the mixture through a sieve, and at once put into packets, observing to press it hard, and to cover it with the foil or close-made paper, to preserve it as much as possible from the air and moisture. Delfort's formula principally differs in the addition of *alum!* and carbonate of ammonium. With the addition of a little turmeric the compound becomes the "Egg powder" so often seen in the windows of grocers and oil-men. These mixtures are used in domestic economy as substitutes for yeast in bread and butter in pastry, and are in their way, and in their proper places, useful, although humble adjuncts to the *materia* (may we not say *medica*), of the non-professional cook. There is no doubt that by enabling pastry to be made equally light, and with one-third less butter, the better class of baking powders have prevented many a bilious and dyspeptic attack.

Mr. Borwick, in addition to his baking-powders, exhibits what he terms "ozonised cod-liver oil." We are sorry to find cod-liver oil "repeating" itself in our notices of the chemical and pharmaceutical products in the French Exhibition, but we will not prevent the unsavoury nature of the subject to turn us from our duty. We consider "ozonised cod-liver oil" to be the greatest of the many delusions connected with this useful food—for food it is purely and simply, and the sooner medical men understand its true character the better for their patients.

Mr. Borwick's prospectus states that the impregnation of cod-liver oil with ozone is for the purpose of "conveying artificially to the lungs of the delicate and consumptive, without the effort of inhalation, and in larger proportions than found in the atmosphere, this extraordinary life-giving agent." The prospectus concludes with the following somewhat rash paragraph: "In fact, it is now proved beyond doubt, that ozone is to the weak, delicate, and consumptive, what quinine is to those who are affected with fever—*the nearest approach to a specific yet discovered.*" The italics are those of Mr. Borwick.

Now, it appears to us that this prospectus has certain points of resemblance to the three incompatible pleas anent the cracked pot, viz.: "1st, that it was cracked when we borrowed it; 2nd, that it was whole when we returned it; 3rd, that we never had it at all." For, in the first place, a distinguished chemist who purchased some and examined it, came to the conclusion—1st, that it did not convey ozone to the lungs of the delicate and consumptive; 2nd, that if it did, it was not "to the weak, delicate, and consumptive, what quinine is to those who are affected with fever; and, 3rdly, that it did not contain ozone at all." It is really inconceivable how any one with even a smattering of chemical knowledge could imagine even if the oil *did* contain ozone that it would carry it to the lungs. The trifling fact that the oil has to be digested before it can enter the blood seems to escape the believers in this so-called remedy. That certain oils acquire powerfully oxidising properties on exposure to light and air we admit, but it must be remembered that in all cases yet known the active oxygen attacks the *oil itself* as soon as the temperature is raised, and many even attack other oxidisable substances present at the same time. The most remarkable instance of the oxidisation of essential oil, is, undoubtedly, that of isoprene*; but when ozonised isoprene is distilled (although it boils at about 40°C.) the ozone present attacks the isoprene with violence, and converts it into an oxodised substance. We think, moreover, with the observer of that reaction, that it is doubtful if the oxygen in what we have hitherto termed ozonised oils is really in the state of ozone. The phenomena attending the passage of ozone through tubes of caoutchouc seem to indicate the impossibility of ozone existing (as such) in the presence of oxidisable organic matters.

Even if we admit that consumptive patients are better in an atmosphere which indicates the presence of ozone (or what is assumed to be ozone) what does that really prove? It seems to us rather to show that consumptive people are better when the air is free from impurities incompatible with the presence of ozone, than that the ozone itself is beneficial to them. The reaction indicated by ozone test papers is not the measure of the total quantity originally in the air, but of the residue left after the destruction of the impurities. But enough of this; we trust that the time is fast approaching when medical men will know more, and talk less, about oxygen and ozone. If one did not hear it so often, it would seem impossible, that in these days of education, doctors are still found who tell their patients to go to the sea-side, "where there is *more oxygen* than in the close and confined streets of towns."

The British Sea-weed Company (Limited) Whitecrook Chemical Works, Dalmuir, Glasgow, have an interesting collection illustrating Mr. Stanford's process of treating sea-weeds.

When sea-weed is incinerated in the usual way a great loss of iodine is experienced, amounting, it is said, to no less than half. Mr. Stanford, by distilling the weeds in iron retorts, entirely prevents this loss, and obtains, in addition to a valuable series of products of destructive distillation, a charcoal which, after lixiviation, is well adapted for the purposes for which animal charcoal is generally used. Indeed we are informed that the carefully prepared charcoal, from certain varieties of weed, actually exceed in bleaching power the best animal charcoal to be found in commerce. The extreme porosity of sea-weed charcoal is greatly in its favour for bleaching and deodorising, in fact, so readily does it allow even thick syrups to pass, that the filters seldom or never become clogged, if properly arranged. They exhibit three different kinds of this charcoal; No 1 from "Tangle" is intended for sewage filtration and as a deodoriser; No. 2, from Bardarrie, is at present sold for bleaching; and No. 3, from Black Wrack, is proposed for sugar-refining.

The Company also exhibit iodine, bromine, and potash salts, the latter extracted from the charcoal remaining in the retorts after the distillation is finished. There is also sufficient nitrogen in the sea-weed to yield enough ammonia to figure as an element in the profits of the undertaking. Whether the oils and tar shown are of sufficient value to be of importance in a pecuniary point of view we are not aware. We should like to know whether a *thorough* scientific examination of the acid, basic, and neutral products of the destructire distillation of sea-weed has yet been made, as it would be very interesting to compare them with those from wood and coal, and especially peat.

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* Phil. Trans. 1860.

It is highly satisfactory to find that the process which was considered by the jurors of the Exhibition of 1862 as sufficiently hopeful and ingenious to deserve a medal, is now carried to a successful issue under the superintendence of its inventor.

REPORTS OF SOCIETIES.

CHEMICAL SOCIETY.

Thursday, June 20.

Dr. Warren De la Rue, F.R.S., *President, in the Chair.*

In continuation of our report of this meeting, we have yet several communications remaining to be noticed.

Professor J. A. Wanklyn read a paper on "*Water Analysis; Determination of the Nitrogenous Matter,*" of which Messrs. E. T. Chapman, Miles H. Smith, and himself were joint authors. Referring to the present unsatisfactory state of our knowledge respecting the decompositions and mode of detection of organic matters (and particularly those of animal origin) occurring in potable waters, the authors quote experiments which tend to prove that, by the evaporation test, the quality of an identical (bad) sample of water may appear widely different according to the rate of evaporation and the manner of applying the necessary heat in the process of analysis; and, further, when a sample of water containing urea or other sewage products, is diluted with a known proportion of distilled water the indication of impurity is not diminished in the ratio anticipated. Extracts from the Report on Metropolitan Waters by Drs. Hofmann and Blyth, 1865, and from the Discourse on Potable Waters delivered last year by Dr. W. A. Miller were read as authoritative admissions of this and similar practical difficulties. The authors further state that no reliance can be placed on Pugh's method of estimating nitric acid as ordinarily performed, and that ammonia cannot be determined in waters containing nitrogenous organic matters by any process involving the boiling with an alkali.

Direct experiments were made upon known quantities of urea, albumen, and gelatin dissolved in water, and the singular fact was disclosed that, whilst the first of these bodies is completely changed into ammonia by boiling with *carbonate of soda*, the two latter substances resist decomposition until *caustic soda* (or potash) is introduced, when *one-third* of the nitrogen contained in them is evolved in the form of ammonia. The remaining *two-thirds* of this constituent are finally liberated in the same form upon adding some crystals of the permanganate of potash and continuing the distillation. The authors employ Nessler's test for indicating the proportion of ammonia originally contained as such in the water, as well as that subsequently formed, and they rely upon the before-mentioned ratio as confirmatory of the existence of the nitrogen in this most objectionable albuminoid form. The details of operating are minutely described in the paper, and several examples are given by way of showing the application of the process. Thames water collected at London-bridge on June 18, tide two hours' flood, contained—

	Grains per gallon.
Ammonia (ready formed)	·0980
Urea	·0889
Albuminous matter (equivalent to white of egg)	·8820

Other results are quoted for East London water, New River, Lambeth and Vauxhall, and water taken from the pumps in Berkeley-square and Bishopsgate-street.

The President inquired the reason of its being found necessary to determine the ammonia separately in the distilled products obtained in the second and third stages of the operation. Why not mix these together and proceed to estimate at once the whole of the nitrogen existing in the organic form?

Professor Wanklyn, in reply, said this course might be adopted, but he considered that a valuable indication towards fixing the nature of the nitrogen was obtained by their separate examination, and this did not in practice occupy a longer time.

Dr. Thudichum offered some remarks on the probable applicability of this process to the examination of the "rice-water" evacuations in cases of cholera.

Mr. Dugald Campbell said that his experiences were not in accordance with the statement just now made by Professor Wanklyn. He found that a solution of gelatin invariably gave off part of the nitrogen in the form of ammonia even upon distillation with carbonate of soda, and the results in the case of urea were not decisive, since it required an addition of caustic alkali before all the ammonia resulting from its decomposition could be expelled. Permanganate of potash had been tried by the speaker in the examination of abnormal excreta obtained in a diabetic case, but neither this agent alone, or aided by the addition of the caustic alkali, sufficed to liberate the whole of the nitrogen even when the heat applied was so great as to cause the destruction of the glass vessel.

Mr. E. T. Chapman remarked that when the amount of albumen or gelatin operated upon was found considerable, it was necessary to fill up the retort once or twice with pure water, otherwise the whole of the ammonia could not be obtained by distillation. This precaution might even be required in the examination of any impure water.

Dr. Cook made a short statement in general confirmation of Mr. Campbell's remarks.

The Secretary then read a paper entitled "*Analysis of a Biliary Concretion, and on a New Method of preparing Biliverdine,*" by Dr. T. L. Phipson. The concretion was found in the liver of a pig, and was of considerable size, being about 3 by 2 inches, and of a yellow, waxy appearance. In the natural state it contained 37 per cent. of water, but after pulverisation and exposure to air it lost all but the 8 per cent. shown in the analysis. Duplicate determinations were made of most of the constituents. The gall-stone had the following composition:—

Water	8·00
Cholesterine	1·35
Mucus	11·50
Hyocholate of soda, with some hyocholic acid and hyocholine	2·75
Cholepyrrhine	61·36
Carbonate of lime	1·55
Phosphate of lime	3·25
Soda	1·11
Chloride of sodium	7·13
Caprylic acid, matters not determined, and loss	2·00
	100·00

The principal constituent is the yellow colouring matter, cholepyrrhine (or bilipheine); this, on digestion with alcohol acidulated with hydrochloric acid, passes into solution as biliverdine, which has a bluish-green colour, and may be separated on addition of water. The author's experiments have led him to believe that there is a close analogy between the yellow and green colouring matters of leaves and the bilipheine and biliverdine derived from animal sources. The last named body is said to differ from chlorophyll only by the elements of two equivalents of carbonic acid.

Dr. Thudichum said that the occurrence of gall-stones in the pig was exceedingly rare. He regarded the analogy or identity of chlorophyll and biliverdine as altogether impossible, these substances showing great differences in their optical properties as well as in their composition.

A paper, by Dr. Stenhouse, "*On the Action of Chloride of Iodine on Picric Acid,*" was next read. Referring to a previous communication to the society in which the ultimate products of this action were stated to be chloropicrin and chloranil, the author has now investigated the intermediate hich could not formerly (from the manner of con- e operation) be isolated. Three parts each of picric acid, together with one part of iodine, were into a digesting flask through which a current of s passed, whilst the contents were maintained at mperature for several hours, and until red nitrous menced to appear. The chloride of iodine was ed off and the residue in the retort heated with ater, from which, on cooling, crystals of dinitro- nic acid separated out. Analyses both of this acid e silver salt were made. Its formula ($C_6H_3Cl\,[NO_2]_2$ all its properties coincide with those observed in duct obtained by Griess when acting upon chlorinated phenol with nitric acid.

Dr. Stenhouse treated *Styphnic Acid* in a similar manner, but this gave only chloropicrin and carbonic acid, thus adding further support to a previous statement of the author "that styphnic acid is not, as Erdmann erroneously supposed, merely an oxidated picric acid, but it must have a different nucleus."

The Secretary then gave a short account of a paper, by Mr. Henry Bassett, "*On Julin's Chloride of Carbon.*" Dr. Hugo Müller obtained, in 1864, a white chlorinated product by the action of pentachloride of antimony upon benzol, and gave to it the formula C_6Cl_6, suggesting its identity with the chloride of carbon of Julin, to which Berthelot ascribed the doubtful formula $C_{10}Cl_{16}$. Mr. Bassett has now reproduced Dr. Müller's body by passing chloroform vapour through a red hot porcelain tube. It crystallises in long colourless needles, which are fusible at 231° C., and the vapour density and analytical results were found to correspond to the formula C_6Cl_6.

The President then adjourned the meeting until November 7, as already announced.

ROYAL INSTITUTION OF GREAT BRITAIN.

A Course of Four Lectures on Spectrum Analysis, with its Applications to Astronomy. By William Allen Miller, M.D., F.R.S., *&c.*

Lecture III.

Solar Spectrum.—Methods of Observation.—Constituents of the Solar Atmosphere.—Spectra of the Moon, and of the Planets, Comets, and Meteors.—Inferences.

We approach to-day the most difficult, and what may probably be considered the most interesting part of the subject which I have undertaken to bring before you, the application of the principles of spectrum analysis to the examination of the condition of the heavenly bodies. Our attention will be specially directed to the sun and some of the bodies of the solar system.

In order that I may do this with effect, let me briefly recapitulate the principal facts which I have to make use of—facts which I have endeavoured to bring before you experimentally in the last two lectures.

We shall have to-day to examine the third of the classes of spectra represented in the diagram—continuous bright spectra crossed by dark lines. Now it will be remembered that every gaseous body at a sufficiently elevated temperature, has a specific spectrum. That spectrum may have its brilliancy increased as the temperature rises, and it may have new lines brought out, but it does not lose lines which it exhibits at a lower temperature. We saw that compound bodies, when sufficiently heated, were separated into their components, and that such compound bodies under those circumstances gave rise to the special spectra of their components. In many cases some of these components are of a nature which give feeble spectra, consequently the spectra of these bodies may entirely disappear from the image projected upon the screen, although the spectra of the other constituent is exceedingly plain. Spectra of the transparent elementary gases in particular are amongst those which disappear under those conditions, such as oxygen, nitrogen, and the permanent gases generally. In one or two instances, elevation of temperature brings out in these gases, spectra which differ from those produced at lower temperatures. A new spectrum is indicated at the high temperature, which was not previously discerned. It is supposed that in these cases the change in the spectrum is accompanied by a change either in the chemical or the molecular constitution of the body by which that change is manifested.

Let me now remind you of an experiment which I showed in the first lecture, where we transmitted the light of the charcoal points through the vapour of sodium; but it will depend upon the relative temperature of the two spectra what the effect shall be. If the vapour of sodium is at a considerably lower temperature than the body behind it, which is giving the continuous spectrum, sodium vapour in this case absorbs rays corresponding in the frequency of their vibration to its own. The temperature of the sodium being only slightly raised, the light which it emits will be a little greater than that which the sodium alone would have produced, but it will be considerably less than that which would be produced by the portion of the continuous spectrum, behind, which it has absorbed; and the result is that when the image of this sodium light is thrown upon the screen, instead of having a bright line, we obtain a black line, or what appears to us a black line, that black line being really a line of low illuminating power contrasted with the spectrum of high illuminating power, and therefore producing upon our eyes the impression of a black line. The intensity of that black line will vary with the difference between the temperature of the body behind and that of the sodium by which the absorption is effected.

If the sodium be raised in temperature until it acquires the same degree as that of the body behind it, the light which falls upon the sodium will be absorbed as before; but now as the intensity of the sodium light itself is equal to that of the incident light, we shall have no sensible effect produced. Therefore the spectrum which falls upon the screen will be continuous, if the sodium be at the same temperature and equally luminous with the portion of spectrum which falls upon and is absorbed by it. But if, on the other hand, the sodium be still hotter, and be still more intensely luminous than the body behind it, it will now in its turn predominate, and instead of a black line we shall have a bright line, or a line of increased brilliancy.

Now I wish you specially to bear in mind these three conditions, which may be produced by the incandescent sodium vapour. 1. We may have a black line when the temperature of the sodium is low; or, 2, we may have no sensible effect, in which case the temperature and the light of the sodium are equal to those of the incident light; or, 3, we may have a bright line, in which case the temperature of the sodium and its light are considerably greater than those of the incident light. What is true in these respects of the vapour of sodium is also true of the vapours of all incandescent bodies. We shall see the application of these points very shortly.

Before we go directly to the consideration of the solar spectrum, let us endeavour to acquire some notion, if we can, of this vast centre of force upon which we are dependent every instant of our lives, and upon which the whole frame around us is dependent for the maintenance of its energies.

The sun, then, we must remember, is a vast body at the distance of 95 millions of miles from us, or perhaps a little less, a globe the visible disc of which is about 880,000 miles across. This wonderful globe is continually throwing forth an amount of light and heat in all directions into space. Of that light and heat we at any given moment are never receiving more than about a 2,300 millionth part: all the rest is passing into space, here and there intercepted by other planets and by other suns. But the vast mass of the light and of the heat which is being given forth from the sun is radiated into space. What becomes of it? That is a question which no one has answered, and no one can answer in the present state of our knowledge. Let us further consider that this vast globe is maintained in a state of intense incandescence. We have now to inquire whether we have any means of ascertaining what the cause of that incandescence is; and if we do not reach so far as that, have we any means of ascertaining the composition of the matter which is in this wonderfully active state?

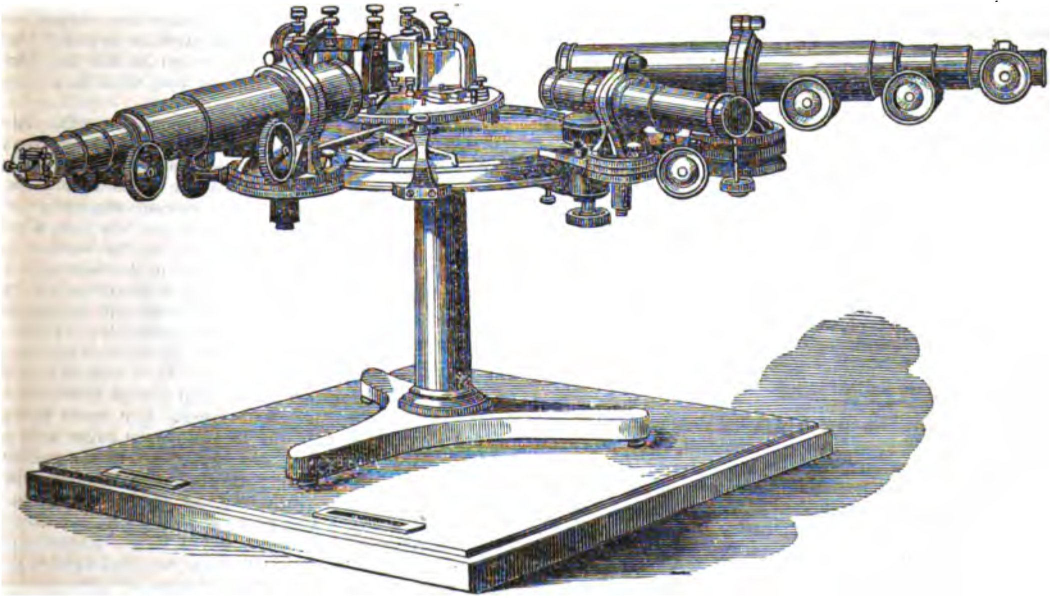
In an inquiry of this kind every aid that is at our command must be pressed into the service. The appearance of the sun, when viewed through a telescope, manifests to us the fact that the solar surface is in a perpetual state of violent agitation. It is not a smooth glowing mass of molten iron—nothing of the sort, for we can see that at different points upon the surface of the sun there are differences in the degrees of activity, and differences in the amount of light which it is giving out. The surface of the sun on the whole may be considered as made up of a series of what have been called bright granules, the forms of which have been variously described by different observers according to the powers of the instruments applied. But these bright granules, be it remembered, represent masses hundreds of miles in diameter. The distance of the sun is such that a circle, a single second of arc in diameter, which, according to Sir John Herschel, is the smallest surface we can see, is 467 miles in diameter. These masses of luminous matter are diffused over the surface of a substance which is much less luminous than itself. Upon the surface of the sun there are dark points which have been called *pores*. In addition to these appearances we have evidence, at intervals, of vast tornadoes or storms which appear to be taking place in this undulating luminous atmosphere. This luminous atmosphere of the sun, or photosphere as it is frequently called according to Sir John Herschel, is best conceived to consist of a quantity of very finely divided, highly luminous, cloudy matter in suspension in a transparent, slightly luminous body of air, differences in luminosity depending upon differences in the distribution of this suspended matter. I am very far from saying that this condition is maintained by an ordinary process of combustion. But I believe there is nothing which gives us a better notion of what the luminous particles of the sun may be, than is afforded by examining the product obtained when a substance is burned in oxygen, like phosphorus, which gives out a quantity of solid flocculent phosphoric acid, glowing with an intense white light by the heat evolved during combustion. The suspended matter in the sun is possibly liquid, but probably solid particles, which are deposited in this intensely heated, but not highly luminous atmosphere, in large cloud-like masses, these masses perpetually sinking to a lower level and as continually being raised again by currents in the solar atmosphere. In order to interpret these appearances we must call to our aid every fact that we can ascertain with regard to the physical condition of the sun. We must apply the laws of physics as we have ascertained them upon the earth. We must invent no new ones to explain these phenomena, if we would proceed in the true path of philosophical inquiry.

Supposing, then, the whole disc of the sun were filled up with matter of uniform density (a very violent supposition, I grant), it must be borne in mind that the mass of the sun, taking that portion of it which we see as its luminous disc, would be represented by a mass of matter a little less than half as heavy again as water. Again, we must remember that the effect of gravity upon the surface of the sun far exceeds its power at the surface of the earth. A pound weight upon the surface of the earth would gravitate upon the sun's surface with the force of a quarter of a hundred weight, so that at the surface of the sun the effect of gravity, like that of many forces in operation upon the earth's surface, is exaggerated to a wonderful extent.

Besides these bright granules, lying between which are the dark pores or spots producing what we may regard as the ordinary appearances, we have upon the surface of the sun, as already stated, evidence of what appears to be violent tornadoes. The luminous mass which constitutes the photosphere of the sun is from time to time apparently torn up, a thousands, aye, hundreds of thousands of square miles of i surface are tossed aside, and a vast cavity is formed in th luminous mass. I am not saying this without warrant de ed from observations upon the sun's spots, first made near a hundred years ago by Professor Wilson of Glasgow, a since confirmed in a remarkable manner by the investigatio which have been made since the state of the sun has been mo minutely examined in modern times. The most recent exa inations of this description are due to our countrymen, De l Rue and Stewart, aided by Mr. Löwy. Their investigatio show us that these vast craters in the luminous surface (if th expression may be allowed) consist of two principal portio the margin consisting of a faintly shaded portion, the *pen bra*, with a dark spot, the *umbra*, in the middle. One of th enigmas for solution is to explain what these solar spots ar Has the photosphere been heaped up to form the adjace brilliant streaks known as *faculæ*? and are these dark spo clouds of colder material which have been depressed into th photosphere, and which gradually disappear in consequen of the incandescent portions gradually again raising the clouds to their usual temperature? Are they—and I think here th evidence goes against this—are they holes in this luminous photosphere showing behind it a dark mass—the interior o the sun itself? I cannot now, however, go more minutely into the question of sun-spots, as it would be foreign to my purpose. My object is to prepare your minds by bringing before you some of the principal points in the physics of the sun, which have been ascertained from different sides by investigating the recent additions to our knowledge by the spectrum. I shall therefore simply now give you on the screen a representation of a photograph of the surface of the sun itself, which, it will be recollected, will be a reversed image of the sun; that is to say, where the sun is brightest we shall have in the photograph the greatest diminution of brightness. At the upper part of the disc you see a brilliant spot. That is a very characteristic sunspot. Remembering that the most brilliant part of the screen is that which is really the darkest on the sun, you will observe in the centre of the opening the umbra of the spot. Then around that is what is called the penumbra, or more slightly shaded portion: on the other sides are groups of spots which are in the process of formation or of healing up, for I do not know at what period this particular spot was taken. These cross lines on the photograph are merely lines of position showing which way the sun is moving. The spots cross the disc of the sun from left to right, and as they gradually diminish it is always found that the umbra is the first portion which disappears.

I wish, in the next place, to show you another fact connected with the sun, which recent observation has enabled us to ascertain. The sun is not simply a great glowing ball, such as it looks to us, but there is something outside the sun of which we in general take no notice, and of which until some twenty or thirty years ago we were in absolute ignorance. The phenomenon to which I am going now to direct your attention is only visible on those rare occasions when the disc of the sun is obscured by the passage of the moon between us and its body. In those cases, and under suitable conditions, we have an opportunity of ascertaining that the sun is surrounded by a vast atmosphere which is not in that intensely glowing and incandescent condition which the surface that we usually see is. I have here a photograph which represents an observation made by Mr. De la Rue in July, 1860. [The photograph was introduced.] This indicates to us the appearance which is seen when the whole disc of the sun, which is visible under ordinary circumstances, is entirely eclipsed by the moon. You will notice that round the dark body of the moon we have a remarkable halo of light, and that this halo is at certain points much more brilliant than at others,—that, in point of fact, there are clouds thrown up into this atmosphere. Some of these clouds have been seen detached from each other. It is estimated that the height at which these clouds occur is in some cases at least 72,000 miles from the surface of the sun, so that around the sun there is a vast atmosphere invisible under ordinary circumstances, into which invisible atmosphere are projected what you see here, and what have been called red flames, clouds, probably, of incandescent matter. In this photograph the solar atmosphere is all of one uniform tint, but as actually seen, its projections, instead of being white, are of a rich red colour, and possess considerable photographic power. What the nature of these flames may be is a point on which further inquiries are necessary. It is probable that next year there will be an opportunity of making observations upon them under conditions more favourable than have ever existed since attention was directed to these points, for in the month of August there will be a total eclipse of the sun, visible in the central portions of India, which will have the unusual duration of nearly five minutes. Thus, if the atmosphere is favourable, opportunity will be given to persons properly prepared for making observations upon these flames by means of the spectroscope, and thus probably of ascertaining what the constituents are.

I will now endeavour to explain how the spectroscope will act in determining the character of these flames, and how it has enabled us to ascertain what some of the components of the sun are. We are indebted for our great stride in this direction to the observations and discoveries of Kirchhoff. Let me call your attention for a few moments to the diagram which we have here, which is intended to represent certain appearances exhibited by the solar spectrum. Suppose the light to be admitted through a vertical slit at a distance, if it be viewed after it has passed through the prism, the spectrum so obtained will be seen to be crossed by an almost countless number of dark lines. I am not sure whether these dark bands were not first observed in this very institution, but at any rate the observation was first made by one of its most distinguished members, Dr. Wollaston. The importance of this observation was certainly not anticipated at the time it was made. He merely looked at the light coming in at the chink of a door, through a prism which he held up to his eye. Some twelve years afterwards Fraünhofer examined the solar spectrum by viewing the slit, which he placed at a distance of 24 feet from him, through a very clear prism, and by means of a telescope. He then saw not merely eight or ten bands, as Dr. Wollaston had done, but he mapped and measured nearly 600 of them, and these lines have been called after him "Fraünhofer's lines." In the diagram you see a number of letters which run along the bottom. These letters were appropriated by Fraünhofer to the indication of the most important and most prominent of the lines which he observed. Fraünhofer found that the solar lines are perfectly fixed in position in different colours, and, being an optician, he applied this observation to the purpose of determining the refractive power of the glass which he used in his lenses and prisms. These lines have always been designated by the letters which Fraünhofer gave them. Many persons have since carefully examined the solar spectrum, and some have described and mapped additional lines. Though I cannot here go into the history of this matter, I may mention particularly the names of Angström and of our own countryman, Sir David Brewster (who has latterly worked in association with Dr. Gladstone): above all we are indebted to Kirchhoff. He used an instrument exactly similar in principle to that which I described in the last lecture, the only difference being that, instead of taking a single prism as is represented in the diagram, he made the light pass through three additional prisms. The light was thrown upon his eye by a telescope. By the kindness of Mr. Gassiot I have an opportunity of showing you what I believe to be the finest instrument of the kind which has ever been constructed. It was made by Mr. Browning, and the accuracy and the workmanship have been attested by all who have used it. In this instrument the light is allowed to fall upon a battery of nine prisms, and after passing out through the last prism it falls upon the face of the telescope, through which it is viewed by a person at the other extremity. It is clear that if we transmitted the sun's rays through such an instrument they would be opened out by the successive action of the prisms, until each line was brought out in such a way that the character of that line could be ascertained with the greatest nicety. Kirchhoff carefully mapped all these lines between A and G. They vary greatly in their strength and degree of definition. This is a curious, and at the same time an important point, and as will be seen it may be made use of in order to enable us to ascertain the origin of the lines in certain cases. As I cannot project the lines of the solar spectrum itself upon the screen, I will substitute for it a photograph of Kirchhoff's map. It is a beautiful map, but there is considerable difficulty in rendering such fine lines visible to a large audience. [The representation of the map was produced on the screen.] You see how very greatly the lines vary in thickness, in blackness, and in definition. Some of them are as sharp as



a line can be drawn, while others are broad and confused and somewhat indefinite in their outlines. Hence it is evident that these lines possess a sort of character by which they can be recognised when they are again rendered visible.

[The attention of the audience was then directed to the character of certain groups of lines, particularly in the vicinity of the lines D and C.]

Before I quit this part of the subject I shall call your attention to a beautiful photograph which indicates the exact position of these lines, as produced by the action of the sun's rays upon a collodion plate. They were taken in New York by Mr. Rutherford, from whom I received the specimen before you. When compared with Kirchhoff's maps, it is wonderful to see how perfect is the correspondence, and how faithfully those maps represent the actual lines.

I once more project the map upon the screen, for the purpose of calling your attention to the green portion. There are three lines in the green due to magnesium. Underneath this map you will see a number of letters. For example, here are the letters Fe. That is a contraction for the Latin *ferrum* (iron), and it shows that every one of these lines so marked corresponds with a bright line in the spectrum of iron. There are a number of other bodies indicated in the same way; thus, Ni signifies nickel; Ca, calcium; Cd, cadmium; Au, gold. Not that every one of these metals has corresponding lines in the solar spectrum, but Kirchhoff has examined the bright lines produced by these different bodies, and has marked the position of the solar spectrum to which these lines correspond.

I will show you now the blue portion of the spectrum which is beyond the parts at which we have just looked. This black line is Fraünhofer's line G. You see what dark groups of lines there are in this part of the spectrum. Each of these lines has its own meaning, if we can only succeed in determining it, though in many cases this has not been done.

I have now to explain to you how Kirchhoff has made these lines interpreters of the composition of the sun. When a series of electric sparks is made to pass between wires composed of any metal, the spark, when examined by the prism, exhibits the special spectrum of the metal. For instance, in this case we have two wires consisting of silver. If I cause this secondary current from an induction coil to pass in sparks through the interval of air between the two silver wires, particles of the metal will be detached in a gaseous condition, and they will give the special spectrum of silver. The heat produced in this way is most intense. Any other metal may be substituted for the silver, and may thus be made to yield its spectrum, the metallic points being placed in such a position that the light of the spark shall be reflected into the spectroscope, and so into the eye of the observer.

I have already stated that it had been ascertained by Fraünhofer and other observers that a particular double black line, called D, in the sun's light, coincided with a bright line which was observed in certain flames, now known to be due to sodium. Kirchhoff, in order to ascertain the exact coincidence of the line D with the sodium lines, placed the light of the sodium so that the sun's light passed through it, and he found that, instead of getting the bright sodium line, he got a still more intensely dark line; he, in fact, discovered that the sodium line was reversed by the action of the more brilliant light of the sun, as already explained. But having found this in the case of sodium, he immediately began to examine other bodies in the same way, and he found that the lines of barium, strontium, and other metals were similarly reversed. He then began systematically to compare the bright spectra of the metals with the dark lines of the solar spectrum. When, for instance, iron is acted upon by the electric spark in the way I have described, it gives rise to a spectrum which contains some 70 bright lines in the space between the extreme red and the extreme violet. These bright lines differ very much in degrees of brightness: some are strong; some are weak; but the interesting point observed by Kirchhoff was not merely that for every bright line in the spectrum of iron there was a corresponding black line in the solar spectrum, but that they also corresponded in intensity. The brightest lines of the iron spectrum were the blackest in the sun's spectrum, and the feeblest in the iron spectrum were precisely the feeblest in that of the sun. This was a case not of the coincidence of two lines merely, which some persons might suppose to be an accident, but it was the coincidence of some 70 lines, line for line and strength for strength. It is impossible to have more striking proof of the identity of the cause by which these two effects were produced. Well, having ascertained in the case of iron that this coincidence occurred, he proceeded to take other metals, and among them magnesium. This metal gives a very limited spectrum, consisting of a remarkable triple group in the green, and that group is found to coincide absolutely in strength and in position with the dark line marked *b* in the spectrum; for when this solar line is examined with care it is found to consist of three black lines, exactly corresponding in character with the three lines of magnesium. Magnesium in vapour, therefore, is one of the constituents of the atmosphere of the sun. So I might go on particularising the character of each metallic spectrum, but to save time I have enumerated in this list all the metals which are known to exist in the sun's atmosphere:—

Sodium.
Calcium.
Barium.
Magnesium.
Iron.
Chromium.
Nickel.
Zinc.
Strontium.
Cadmium.
Cobalt.
Hydrogen.

Copper might have been added, though its presence is rather doubtful. Others are also marked as doubtful. Though many of their lines correspond with dark solar lines, yet there are other lines produced by these metals which have no corresponding lines in the solar spectrum. That may arise from the circumstance that the proportion in which they occur in the sun's atmosphere is small. As a rule, the metals which are enumerated in the list furnish bright lines corresponding, line for line, with dark lines in the spectrum of the sun.

Now, this leads me to another important point. It will naturally be suggested to the mind, "it is very true that these are lines produced in the light which leaves the sun, before it gets to us; but how do we know that they are produced in the sun itself? All the substances of which we have seen lines in the solar spectrum are bodies which we know upon the earth. Is it not possible that all these bodies may exist in such quantities in the earth's atmosphere that they may be the means of shutting out these rays of light which are found to be deficient in the sun's rays when they reach us?" That is, certainly, an important question; but it can be answered perfectly. If we had only the light of the sun to judge from, it might be a difficult point to decide; but we have in the stars a multitude of other bodies from which we derive light quite independent of that of the sun. If we found that every star gave us the same spectrum as the sun, then we might well question whether these lines were really due to matters existing in the sun, or whether they were not due to bodies in the earth's atmosphere. But, as we shall see in the next lecture, every star gives a spectrum of *its own*. Now the light which comes from the stars ought to be acted upon exactly in the same way as the light of the sun, if the cause of the lines were in the atmosphere of the earth. We find it is not so. The cause, therefore, does not reside in the atmosphere of the earth—at least in the majority of cases: although, as I shall show you presently, there are certain lines truly due to the action of the earth's atmosphere.

Spectrum analysis cannot tell us whether the visible surface of the sun is solid or liquid, or whether it is made up of cloud; because either a cloud, or a liquid, or a solid body would give us a continuous spectrum. If we took the light emitted by phosphoric acid, produced in burning phosphorus in oxygen, it would give us a continuous spectrum just as the surface of the sun does. But we know that there is a great atmosphere outside the visible disc of the sun which is revealed to us temporarily during a total eclipse. That great atmosphere contains bodies of all sorts in a state of vapour, volatilised by the enormous heat which is produced in the sun itself. Of course the farther this extends from the sun the colder it must be, and therefore there must be a point in the sun's atmosphere in which we are precisely in the position required to give this reversal of the bright lines due to each metallic vapour; that is to say, we must have an intensely heated nucleus behind a colder atmosphere; but even this colder atmosphere may be so intensely heated as to keep all these bodies, which we cannot even volatilise in our furnaces, in a state of vapour: if so, they must give us the dark lines observed in the solar spectrum, by absorbing, from the light of the incandescent nucleus of the sun, vibrations of the particular frequency corresponding with those produced by the metallic vapours themselves. Thus, in the light which comes to us, we have proof from the absent vibrations. We have, I say, a proof of the existence of these atmospheric bodies in the sun which arrest the corresponding vibrations produced behind them. That is the proof which Kirchhoff has given us of the presence of these bodies in the sun.

I stated just now that there are lines produced by the earth's atmosphere. This is a curious observation. It was first made by Sir David Brewster, and he arrived at the fact in this way. Here is a diagram, which may help to make it plain. Suppose this part to represent the globe of the earth, and here is an exaggerated representation of the atmosphere. You can see easily that if the sun were nearly vertical, the light would traverse a much smaller portion of the atmosphere than it would when its beams are nearly horizontal, near sunrise or sunset. The sun's light would then have to traverse a portion of the atmosphere nearer the earth, and greater in density than that through which the light would pass when the sun was high up; so that if the spectra are different when the sun is in these two different positions, we might then say that it is probable that this effect is due to something in the atmosphere of the earth itself. Sometimes opportunities of proving this occur accidentally. I have had myself such an opportunity. I was one day looking at the spectrum when a thunder-shower came on; suddenly there started into view a group of new lines, evidently produced by some sudden change in the air at the moment. The storm shortly after subsided, and the changes which had occurred in the spectrum vanished. M. Janssen, a distinguished French experimentalist, and Prof. Cooke, an American man of science, have both made observations which show that variations in the amount of moisture in the air are connected with changes of this kind. The experiments which M. Janssen made were of this kind. On the border of the lake of Geneva, he caused a pile of wood to be lighted on the top of a mountain. Having stationed himself on the other side of the lake, about thirteen miles off, he viewed this fire through a spectroscope, across the body of moist air resting upon the lake, taking care to have it at the same time observed near at hand, where a continuous spectrum only was seen. In this way he detected the presence of certain lines in the less refrangible end, occasioned by moisture contained in the atmosphere. He afterwards performed another ingenious experiment, which I regret that I cannot show in this theatre. He took an iron tube, of about 37 metres, that is, about 40 yards, in length, closed by plates of glass at the extremities. At one end he placed a gas flame, and then looked through the tube at the spectrum of the flame, by means of a spectroscope, and thus obtained a continuous spectrum. He then injected into the tube some steam, at a pressure of 7 or 8 atmospheres, so that he got a dense body of aqueous vapor. Then, on looking through the tube at the flame by means of the spectroscope, he saw in the red part of the spectrum, lines, something like those which are seen in the diagram,—strong bands of lines, evidently produced by the absorptive action of the aqueous vapour in the tube. The temperature and the pressure of the vapour were high, but they had nothing to do with the production of the lines; they were merely used as a means of getting a large mass of vapour into a small space.

In the application of this observation we have a number of curious and very interesting facts. You will remember that the solar light has this remarkable feature,—and, indeed, all lights are the same in that respect,—that whether it is seen directly, or whether it is reflected from a clean white surface, it still betrays its origin; that is to say, it always shows the same lines. If we were to look at the spectrum obtained from the sun's light direct, and then look at the spectrum of the same light after reflection from the surface of a cloud or from a mirror, we should find that the position and number of the lines were just the same in both instances. In the reflected light we should not have so intense a spectrum, but the lines would be in just the same places as in the direct light, and we should know for certain that the light in either case came from the surface of the sun.

Now what is true in this case appears to be true in all cases of reflection. It is true, for instance, in the case of reflection from the moon. I will throw upon the screen a representation of the moon, as it appears from one of Mr. De la Rue's recent photographs of the moon. What I want to call your attention to is this, that we have in the spectroscope the means of travelling over the surface of the moon, and examining the quality of light reflected from its different parts. Now, what use can we make of this? Suppose we wish to examine whether the moon has an atmosphere. We find, by the telescope, that we can see right down to the surface of the moon. The parts of its face are never obscured by clouds or by dark vapours connected with the moon itself. But it might yet happen that a delicate atmosphere, containing a very small quantity of vapour, existed round the moon, and the thickness of this stratum would necessarily vary when viewed at different parts of the surface. If we looked at the edge of the moon, the effect would be similar to that produced by viewing the sun near the horizon, the light which was transmitted from the edge would come through a long column of this atmosphere before it reached us. If we looked direct at the centre, of the disc, where the light would have to traverse a smaller depth of atmosphere than at any other point, we ought, if there were any atmosphere containing absorbent vapours, to observe a difference between that light and the light at the moon's edge. Now, on looking at these different portions of the moon, we find that the light comes from all parts, without any change. Hence we must conclude that if there is an atmosphere around the moon, it must be so excessively dilute that it cannot produce any absorptive change in the light which is perceptible in its spectrum.

Again: we can apply these observations to show that the moon does not shine of its own light, though we do not need the proof of that fact from this source; and we may also apply the same proof to all the planetary bodies. But though we already know that the moon and the planets shine by reflected light, in other instances the knowledge we can thus derive may be of importance to us. Take, for example, the case of a comet; does that shine by direct or reflected light? I will show you how this question may be answered. But, before doing so, it will be desirable to consider the result obtained by observations upon Jupiter,—a body which certainly shines by reflected light. And here we acquire some information regarding the atmosphere which surrounds Jupiter, because the light reflected from Jupiter is not identical with the light which falls upon it. Here is a diagram of some observations made at Tulse Hill, by Mr. Huggins and myself, which may make this clear. The spectrum of Jupiter's light shows us, in particular, a dark band in the orange. Besides the ordinary solar lines, which are very well seen in the case of Jupiter's light, there are groups of lines connected with the atmosphere of the planet. The object was to see whether the spectrum produced by Jupiter was different from that of the earth's atmosphere; and this object was attained by comparing the light of the planet with that of the sky, which was at that time reflecting the light of the setting sun, under circumstances in which a reflected spectrum from the surface of the sky itself was not too intense for comparison with the spectrum of Jupiter. These are some photographs of drawings of the appearance of Jupiter, made some years ago by Mr. Huggins, from telescopic observations. Astronomers have long believed that an atmosphere of considerable density exists around this planet. These bands or belts are produced, it is supposed, in consequence of clouds accumulating near its equator. They show us that, in all probability, the atmosphere of Jupiter is largely charged with aqueous vapor, and that the surface of the planet itself is not actually seen. Consequently, the light which we see reflected by the planet does not come from the body of Jupiter itself; it penetrates to a certain depth, and then comes back, after traversing a portion of its atmosphere; and therefore we do not know what is the actual condition of its surface. In Saturn and Mars we also obtain, by means of the spectrum, a certain amount of knowledge of the state of things upon both those planets. In the spectrum of Mars a number of bands in the blue make their appearance. It has been supposed that the red colour of Mars was produced by a peculiar colour of the soil. These spectrum observations seem to show that it is due rather to something in the atmosphere, and not to anything in the soil; because, if it were the latter, we should merely have a blotting out of the spectrum, and there would not be a series of regular bands, which have been observed in the case of Mars.

I have stated that by the method of spectrum observation we may ascertain whether comets are self-luminous, and may even attain to some knowledge of their composition. Our knowledge of the spectra of comets is, indeed, excessively limited. Donati, in 1864, made some observations; but the results he obtained were not very definite. I believe that those of Mr. Huggins, upon the small telescopic comet of 1866, are the best at present existing. It had a bright central nucleus, and around that was a nebulous atmosphere, not prolonged into a tail, as is usual in most comets. By means which I must explain in the next lecture, the spectroscope was brought to bear upon it, when it was found that the coma, or tail, gave a prolonged continuous spectrum, and in the middle of that spectrum there was a spot of bright greenish blue colour, indicating the position of the nucleus. What information does this give? It appears to show that the spectrum of the coma is produced by reflected light, and that the tail of the comet is somewhat in the position of a fog; that that fog reflects from the sun light of all colours, whilst the central portion is giving out light of its own, and light of one colour only. However, I have not time to-day to go into that point. I shall take it up again in connection with those remarkable bodies, the nebulæ, with which I shall deal in the next lecture.

LECTURE IV.

Spectra of the Fixed Stars.—Mode of Observation.—Double Stars.—Variable Stars.—Temporary Bright Star in Corona.—Nebulæ.—Clusters.—General Conclusions.

In the last lecture I gave you some account of the solar spectrum, and stated some of the principal facts which were revealed to us by its examination. You will remember that in the solar spectrum there are a great number of lines produced, as we now know, by absorptive action in an atmosphere which surrounds the more intensely luminous portion of the sun. We have learned to interpret many of these lines, and we have found that in a great number of cases they are produced by the presence of elementary bodies, known to us upon the earth, which, in the gaseous state, exert an absorptive action upon certain parts of the sun's rays. We have learned, also, that there are certain bodies which are not present in the sun—among them gold, silver, lithium, and several others. But there are still a great number of lines of the nature of which we know nothing. Many, no doubt, will be explained as we proceed further with our investigations into the spectra of terrestrial elementary bodies. Notwithstanding the efforts that have been made within the last few years, our knowledge of terrestrial spectra can be at present considered to be only in its infancy. We can not know what elements, indeed, are still existent upon the earth. We can not be supposed to have come to the end of our knowledge upon this point, for within the last five years no fewer than four of these elementary substances have been discovered by the simple application of this method of spectrum analysis.

The investigation of the solar spectrum, difficult as it is, is one considerably favoured by certain circumstances. We are not limited by season, or the angular altitude of the sun, but can pursue the investigation whenever the sun shines. Moreover, we can command any amount of light which the eye can bear. We can, therefore, by the action of an almost unlimited series of prisms, dissect and open out that solar spectrum, and so scrutinise, with minute accuracy, the position and character of every line which it contains.

I have to-day to refer to other sources of light which may be analysed in a similar way, but the study of which presents difficulties of no ordinary character. I propose to explain some of the methods which have been adopted for the examination of stellar spectra, and to state the chief results obtained by that examination, and I shall conclude by giving you some account of the remarkable and unexpected results obtained by a study of some other still fainter objects which are visible in the heavens—the nebulæ.

It may be necessary for me to give you some notion of the kind of difficulties with which we have to contend in these enquiries, in order to explain how it is that the results, important as they are, are still very imperfect. Solar light, as I have said, may be obtained in unlimited quantity; but when we examine the spectra of the stars we have to deal with points of light. We must collect the light, therefore, and for that purpose either a large reflector or refractor is necessary. Cumbrous machinery is required for moving the tube to enable it to follow the motions of the star, which is apparently perpetually shifting its place in the heavens. We have to bring this light to a point by means of our lenses. The more accurate our telescopes the more exactly is this light brought to a mathematical point. Now, if we attempt to analyse by means of a prism a point of light like this, we shall spread it out into a line, but that line will be so exceedingly narrow that we shall not be able to trace across it the lines of which we are in search, and which are to speak to us of its nature. The first thing, therefore, we have to do, after we have obtained our point of light, is to open it out into a line. That may be done, as was practised by Fraünhofer, by means of what is known as a *cylindrical* lens. I shall endeavour to throw upon the screen a little point of light, which you may, if you please, for a moment consider to be a star, and will then elongate its image into a line of light.

[A circular spot of light was projected upon the screen, and then by means of a cylindrical lens expanded into a line of light.]

Though I can imitate a star in the exceeding minuteness of its light, I, unfortunately, cannot imitate it in the quality of its light, and, therefore, on this occasion, I shall not be able to show you the spectra themselves, but I shall have recourse to photographs from careful drawings made upon the observations of the stellar spectra. In the observations from which these drawings were made an achromatic object glass of eight inches aperture was used, and the observations were made at the observatory of Mr. Huggins, at Tulse Hill, where he and I worked together for some years upon stellar and planetary spectra, and where he has since still further added to our knowledge by his examination of the nebulæ.

Having obtained our line of light, the next thing is to project it upon a suitable instrument for making the observations, and this line of light must be kept absolutely steady at the end of a tube ten feet long, upon a slit, the width of which is not more than the 300th part of an inch, or much finer than any ordinary hair. Obviously, this can be obtained only by an exceedingly smooth motion maintained by the clock movement of the telescope.

Here is a star spectroscope in the form which, after many trials, we found to be most convenient for these observations. I am indebted to the kindness of Messrs Simms for the loan of this beautiful instrument, which they are going to send to Professor Cooke, in America. I am also indebted to Mr. Browning for the opportunity of showing you another instrument intended for Mr. De la Rue, which is, in some respects, even better for our purpose, because it enables us to see the parts of which it is composed. This instrument of Messrs. Simms is provided with shutters, so as to exclude dust and other sources of injury to the prisms within. When in use the spectroscope is attached to the eye-end of the telescope, instead of the ordinary magnifying power, and will be carried round with it, accurately following all its motions. The cylindrical lens is placed between the object-glass and the slit, so that the light, instead of falling upon the slit as a point, shall fall upon it as a line, such as I just now showed you, only much more definite than the line which was projected upon the screen. The line of light having fallen upon that slit, is then passed through an apparatus precisely similar in principle to the spectroscope which we examined in the last lecture.

The light of the star is brought to a focus by the action of the object-glass of the telescope itself, exactly at the spot occupied by the slit of the instrument. But before reaching that spot, it passes through the cylindrical lens, by means of which it is spread out into a line, instead of a point. After passing through the slit, the light falls upon a collimating lens—that is to say, upon a lens the object of which is to bring all the rays that fall upon it into a parallel direction. The rays rendered parallel next fall upon a couple of prisms—the first prism dispersing the light to a certain extent, and the second dispersing it still farther, and producing a spectrum of the star, the most refrangible end being that which is most turned from the original direction. The spectrum then falls upon the lens of a small telescope, by means of which the image can be viewed at the proper distance. The object of the screw beneath the tube is to enable us, by a regulated movement, to carry round the small telescope, so that each part of the spectrum can be successively examined. There are cross wires in the telescope, so that, on bringing any of the lines in the star spectrum one after the other upon the cross wires by the micrometer screw, the distance between the lines may be measured exactly.

The object which we had in view in these investigations was not merely to ascertain that the lines existed in the stellar spectra, for that had been done by Fraunhofer, Donati, Secchi, and others, but to determine what these lines represented—to ascertain the constituents of the stars if possible; and that could be done, approximately at least, by measuring the position of each of these lines, and then comparing it with a map in which the lines of certain metals had been laid down, these metals having been examined by the same instrument as that which was to be applied to the stars. This, however, although it would give a result which was very valuable to us as suggesting what probability there was that certain metals were present in these stars, did not give us that absolute certainty respecting the nature of the substances which it seemed it was possible to attain by another mode of experimenting. That mode consisted in reflecting into the instrument the light produced by a series of electric sparks sent through wires of different metals in succession. In order to attain this object we have attached to this instrument a means of producing sparks between two points of silver. The instant that this circuit is included between the terminals of the secondary wire of an induction coil, a torrent of electric sparks passes between the two silver points. The light is reflected from a mirror, by which it is thrown, through an opening in the side of the tube, upon a little prism which acts as a reflector, and sends the light through the slit into the spectroscope for examination. In this way various stars can be submitted to experiment, but it is obvious that even here, although we have at command a large instrument, we are limited by the brightness of the stars. We cannot examine with any degree of precision stars which are below a certain magnitude, the quantity of light being too small. I shall endeavour to show you the results of the most accurate observations we have been enabled to make. Amongst these stars there are two in particular which are of special interest. These two are Aldebaran, the bright, reddish star in Taurus, and α Orionis, or [illegible]elgeux, the principal star in Orion. Here is a list in which are mentioned, certain elementary substances, all of which have been found to produce lines coincident with certain lines in this star Aldebaran:—

Sodium,
Iron,
Magnesium,
Hydrogen,
Calcium,
Bismuth,
Antimony,
Tellurium,
Mercury.

There is not merely the coincidence of a single line in each case, for that might be an accidental circumstance, but the principal bright lines in each spectrum produced by these bodies have corresponding black lines in the spectrum of this star. You see that there are here nine of the substances known to us upon the earth. By similar means we ascertained the absence of certain other bodies; these have no coincident lines in the star spectrum. Among these are nitrogen, tin, lead, cadmium, lithium, cobalt, and barium. We have ascertained not simply what are there, but what may not be there. Of the lines which are contained in this star we measured no fewer than seventy, notwithstanding the faintness of the object. Actual measurement, however, is possible only with a certain number of lines. There are an indefinitely greater number of lines existing in the spectrum of the star, many of which might possibly be measured by the devotion of still more time and application.

These observations are excessively fatiguing to the eye, and require special conditions of the atmosphere. A clear night, which would be very favourable to observations made simply with the telescope, might not be suitable for observations with the spectroscope. The slightest flicker or tremor in the atmosphere disturbs the accuracy of the observation. It must be remembered that we are not observing approximative coincidences, but we are desiring to observe absolute coincidences—coincidences between the bright lines of the metals we have on the earth and the black lines in the spectrum of the star.

In α Orionis we measured eighty lines, and amongst these we find that six of the metals of the earth had lines coincident with those in this star, viz.:—sodium, iron, magnesium, calcium, bismuth, and thallium (?), and that a still larger number were not coincident.

I shall project upon the screen a representation of the spectra of these two stars. They will both be visible together. The upper spectrum is that of the star in Orion, the lower one that of Aldebaran.

Beneath each diagram a number of bright lines may be seen. These represent the bright lines produced by causing sparks to pass between points of different metals attached to the coil. For example, here we have a line marked Sn. This is one of the lines indicating tin, but it has no corresponding line in the star, though we were able to measure within 5000th of an inch, or the 1800th part of the length of the entire spectrum, the coincidence between two lines. Here are the three lines of magnesium which have corresponding lines in the spectrum of this star. Here is the double line D in the sodium spectrum. That is a bright double line corresponding perfectly with two black lines in the star. Sodium is one of the most widely diffused substances in the stars; magnesium and iron are also present in both these stars. The lines in this photograph only extend to a certain distance into the blue. Here is the line *b*, and this is the line F, which is upon the margin between the green and blue. Note in passing that there is no line corresponding to F in the star α Orionis. There is a strong line corresponding to it in Aldebaran, but no line in the other. This line F is one of those due to hydrogen. I carry you to another—near the end of the red. Now this line in the spectrum of Aldebaran is a marked line corresponding with Fraunhofer's solar line C. That line also occurs in the spectrum of hydrogen. There are three lines in the spectrum of hydrogen—C, F, and G; but the point of int[illegible] here is that none of these lines are present in the star α [illegible]nis, and we conclude that there is no hydrogen there. Besides these are a large number of other lines, some of which are interpreted, others still await interpretation.

We are not yet able to explain in the case of these stars, any more than in that of the sum, what every line indicates. It may very well be that many of these lines are caused by substances which are known to us, but of the spectra of which we are still ignorant; in other cases it may well be that they are produced by substances which are not known to us, and which have no existence, indeed, upon our planet. Amongst the substances which are present in the atmosphere of these stars, are some which are present in our own sun. In other instances, as in the case of bismuth, metals are found in these stars which furnish lines which are not present in the atmosphere of the sun. Both in Aldebaran and in α Orionis there are lines corresponding with those of bismuth. Tellurium appears to be an important element in the absorbent atmosphere round Aldebaran. Then there are also in the atmosphere of this star substances such as antimony and mercury, which we consider to be poisons. Here is the bismuth spectrum thrown upon the screen. Though not so striking as that of some other metals, its characters are sufficiently strong to enable us to pronounce with certainty upon the presence or absence of lines corresponding with this substance in any star with which its spectrum is compared.

(To be continued.)

On the Absorption of Gases by Metals, by Dr. Odling, *F.R.S., etc.*

In a Friday evening lecture which I had the honour of delivering here before Easter, I drew your attention to a very singular property possessed by the metal platinum, some very beautiful specimens of which are exhibited in the library, through the kindness of Messrs. Johnson and Matthey, to whom I am also indebted for the greater portion of the articles upon the table. Now platinum, especially that which has been solidified after fusion, appears, at any rate, to be a perfectly homogeneous metal. It does not show the slightest evidence of porosity, and is absolutely impermeable to the passage of gas through it. For instance, if we take a wide platinum tube, drawn out from a single piece of fused platinum, and seal one end by soldering on to it a piece of platinum foil, and similarly close the other end with another piece of foil having a small central orifice, through which a narrow attachment tube projects, we shall have a hollow cylinder of platinum, from which, by connection with the barrel of an ordinary air-pump, or with an air-pump of Sprengel's construction, the air can be exhausted as readily and completely as from a glass tube. That is to say, as perfect a vacuum can be produced and sustained within a tube of platinum as within a tube of glass. Not a particle of air passes bodily from the external atmosphere through the substance of the metal into the internal vacuous space by means of atmospheric pressure, and, what is more to the purpose, not a particle of air passes molecularly through the metal by means of the far more refined and accusing process of diffusion; and this is true, not only of atmospheric air, but of every gas with which the experiment has been made, and is doubtless true of all gases whatsoever. But if, instead of making such an experiment with the platinum tube at ordinary temperature, we make it with the tube at a red heat, under this condition the metal still remains perfectly impervious to the passage of atmospheric air, and of all other gases—except one. One gas alone, under those circumstances, is found to penetrate the platinum, even with very considerable facility, and that gas is hydrogen. The fact that hydrogen has this distinctive property of penetrating ignited platinum, first observed by M. Deville, of Paris, evidently shows that there is something peculiar, something altogether special, in the relation subsisting between the gas and the metal, to which the effect is due. The mode in which the experiment has been recently made is of this kind:—The platinum tube is first attached to an air-pump of Dr. Sprengel's construction, which acts on the principle of the trompe. We have mercury constantly dropping from a constricted funnel down a long narrow glass tube; and as each drop of falling mercury is sufficient to fill the tube, each such drop acts as a small piston which pushes down a column of air before it, and by pushing down successive portions of air eventually effects a complete exhaustion of any vessel communicating with the fall tube. Indeed, by an instrument of this kind, as perfect a vacuum can be obtained as in the Torricellian vacuum itself, and by its employment a vacuum is easily obtained in the interior of our platinum tube. Before being exhausted, the platinum tube is placed within a tube of porcelain; while in the space between the two tubes a current of hydrogen is continually passed. The internal platinum tube being rendered vacuous, the external porcelain tube is heated in any form of furnace,—in a gas furnace, for instance, as shown here, or in a charcoal furnace; and so soon as the external porcelain tube acquires a bright red heat,—so soon in fact, as the interior platinum tube becomes red hot,—and not till then, do we find that as each drop of mercury falls through the tube of the pump, it carries before it a certain quantity of air or gas which it delivers into this small inverted test-tube of mercury placed for its reception. So soon, therefore, as the platinum tube becomes red hot, a portion of the hydrogen gas confined in the intermediate annular space between the porcelain and the platinum tubes, enters the platinum tube, is exhausted from its interior by means of the Sprengel pump, and is delivered into the test-tube for examination.

There is another metal, namely palladium, one of the platinum group of metals, and closely related to platinum, through which the transmission of hydrogen is yet more easily exhibited, since a temperature very far short of redness is sufficient to render this metal pervious to the gas.

Here is an ordinary apparatus for generating hydrogen. The gas passes through sulphuric acid to dry it, and then through the glass tube in which the palladium tube is contained. You see it from time to time take fire, though it does not burn continuously in consequence of its being delivered through the sulphuric acid in the form of bubbles. The small tube of palladium, contained within the glass tube, is made exactly like our platinum tube, except that it is much shorter. At the present time it is vacuous, having been exhausted by means of the Sprengel pump before the lecture. If you will direct your attention to the small inverted test tube of mercury, you will see that at the present time there is no gas being delivered into it, but we will now heat the palladium tube gently through the glass tube, and long before it gets to a red heat you will find that some of the hydrogen gas which is passing outside the vacuous palladium tube will penetrate through the thickness of the metal into the interior of the tube, be sucked from its interior by means of the Sprengel pump, and delivered into the test tube. I have no doubt that after a few moments, you will see that a quantity of gas will be delivered into the test tube in this way, which gas we shall see to be hydrogen, sucked from the interior of the tube of palladium, through the substance of which it has been transmitted. At the present moment, if you listen, you will hear each drop of mercury falling just as it falls in a barometer; but in a few minutes—and I must beg your patience for this experiment, because it will take some little time—we shall lose this clicking sound, through the entrance of gas into the full tube; of which gas, after a little while, we shall get an appreciable volume delivered. A few bubbles of gas have already come over. Those who are near can see that a quantity of gas is being gradually collected in the inverted test tube, which will soon amount to several cubic centimètres. On now putting a light to it, you see that it takes fire and burns in the characteristic manner of hydrogen. Within a few minutes, then, hydrogen, to the amount of several cubic centimètres, has passed from the exterior of the moderately heated palladium tube, through the substance of the tube, into its previously vacuous interior, whence it has been sucked out and delivered by the Sprengel pump.

A curious point of interest is that, in the case of heated palladium, as in the case of ignited platinum, hydrogen is the only gas which passes through the metal. If we take a mixture of hydrogen gas with some other gas, the hydrogen only is sucked through the platinum or palladium, the other gases remaining untransmitted. From the complex mixture of gases, for instance, which constitute ordinary coal gas, hydrogen may be separated in a perfectly pure state by its passage through heated platinum or palladium, which are impervious to all the other coal-gas constituents, as I had the honour of showing you in my former lecture.

Now what is the nature of this transmission of hydrogen through solid metal? On the last occasion, I had an opportunity of demonstrating to you that the phenomenon had no relation to those physical actions which are denominated transpiration and diffusion, but that it was an action of an entirely peculiar character, and was probably preceded by an absorption of the hydrogen in the substance of the metal; and this absorption of hydrogen by platinum, and of other gases by other metals, forms the subject of my present lecture. I wish to show you that in these cases the passage or transmission of the hydrogen gas, through the platinum or the palladium, is preceded by an absorption of the hydrogen gas in the substance of the platinum or the palladium.

The experiment is made in this way: in the first instance some platinum wire is introduced into a porcelain tube of this description, which is closed at one end, and at the other end connected with a Sprengel-pump. The tube is then heated, and its interior rendered vacuous. After the establishment of a vacuum, hydrogen gas is passed over the red hot platinum wire contained in the tube, and the platinum gradually and slowly allowed to cool with the hydrogen gas still passing over it, so that if the platinum has any power of absorbing hydrogen gas it shall have every chance of doing so, and shall, so to speak, select its own absorbing temperature, from the temperature of a red heat down to that of the atmosphere. The platinum was then removed and exposed freely to the air, so that any accidental hydrogen adhering to the surface might be removed. It was then introduced into another tube of this kind, which in this particular case is made of glass in order that it may be seen through, but which, in the actual experiment, was made of porcelain. This tube, containing the charged platinum wire, was attached to the Sprengel pump, and again perfectly exhausted,—the air removed from it by the exhauster being found absolutely free from hydrogen. Then heat was applied, and, at the moment when the tube became red hot a delivery of hydrogen gas began to take place, and continued for some time, showing that the platinum had absorbed a certain quantity of hydrogen gas, which it delivered up at a red heat under the influence of the vacuum. In the first experiment, then, from fused platinum exposed in this manner to the action of hydrogen at a red heat and gradually decreasing temperature, there was afterwards extracted as much as 21 per cent. of hydrogen.

Now what was the nature of this behaviour of platinum towards hydrogen? Did it depend upon surface action? If so it would be increased by an increase of the surface of the platinum. The wire was accordingly drawn out to four times its length, which would give rather less than four times the former surface; but in this case only 17 per cent. of hydrogen was extracted after treating the wire as in the previous experiment. Quadrupling the surface, therefore, did not increase the quantity of gas absorbed, and it was found on a repetition of these experiments that the quantity of gas absorbed gradually got less and less. In a third experiment with the same wire, the quantity absorbed amounted to only 13 per cent.

Now comes the question, if this power of the metal to absorb hydrogen is not a question of surface, is it a question of texture? Accordingly, spongy platinum was employed, and in that case it was found that the metal absorbed 148 per cent. of gas, that is, about 1½ times its volume of hydrogen gas, measured at the temperature of the atmosphere. As fused platinum we had the metal in its densest, or rather in its most compact form. As platinum sponge we had it in its most porous form; while as ordinary wrought platinum we have it in a sort of intermediate form—not so porous as the one, and not so compact as the other. What, then, was the result obtained with the wrought platinum? Well, it was found that one volume of wrought platinum absorbed 4·8 times its volume of hydrogen, or 480 per cent. This is the mean of three experiments made with the same portion of platinum. One of them gave 5·5; another gave about 4·9; and the other gave 3·8 volumes of gas. The next experiment was made with a different portion of platinum; and in this case, although the metal contained 3¾ times its volume of hydrogen gas (379 per cent.), measured cold, it was found that none of this gas could be evolved into a vacuum except at a red heat. At first the platinum was submitted to a temperature of 240°, but not a particle of gas was given off. The metal was then heated by a small Bunsen burner to a temperature just short of redness for an hour, and still no gas was given off; not, indeed, until the temperature arrived at the point of redness, could any gas whatever be extracted.

We are all of us familiar with the effects of atmospheric pressure, and know that a variety of experiments have been devised by which the results of this pressure may be manifested. One of these is the common experiment of bursting a bladder stretched over a short glass cylinder exhausted by the air-pump. [This experiment was performed, a sharp report accompanying the breaking of the membrane.] Now, let us compare the force exerted by atmospheric pressure in this way, with the force exerted by platinum in condensing hydrogen gas. Let us consider the experiments of which the mean result was 4·8. As I have said, one of these experiments gave 5·5; and for the sake of taking round numbers, we will say that 5 volumes of gas were absorbed by one volume of platinum. Taking a cubic centimètre of platinum, then, it absorbs five times its bulk of gas. To condense these 5 centimètres of gas into the space of one cubic centimètre, would require the pressure of five atmospheres; or five times the force just exerted by the air in bursting that bladder. But at the temperature at which the experiment was made, these five centimètres were really 15; and accordingly, we have to consider the compression of 15 cubic centimètres of hydrogen into one cubic centimètre, for which purpose we should require 15 times the atmospheric pressure which was just employed. But in reality, this does not represent anything like what has been done. We have not merely compressed these 15 volumes of gas into 1 cubic centimètre of space, but we have compressed them into so much of 1 cubic centimètre of space as appears to be fully occupied by platinum, but is not really so occupied. If we assume, for instance, that in this cubic centimètre, which appears to be all platinum, there is, say, one thousandth part of it which is not platinum, to compress our 15 cubic centimètres of hydrogen into this space would require a pressure of 15,000 atmospheres. So that when this piece of platinum absorbs 15 times its bulk of hydrogen gas at a red heat, it exerts some 15,000 times the compressing force that was exerted by the atmosphere in bursting that bladder.

My time is passing away so rapidly that I fear I shall have to omit a good many points of interest; but I may direct your attention to the fact that, although the evolution of hydrogen by platinum takes place only at a red heat,—and indeed some charged platinum that had been preserved for two months was found still to retain the whole of its hydrogen—yet the absorption of the gas by the metal takes place at a much lower temperature. Thus at 230°, platinum absorbed 1½ times its volume, or 145 per cent., and even at a temperature below 100°, the boiling point of water, it absorbed 76 per cent. of gas.

When, however, we come to speak of palladium, the facts are far more striking even than with platinum,—palladium appearing to be a metal altogether special in its relations to hydrogen, which it absorbs abundantly even at a comparatively low temperature. A piece of wrought palladium foil heated to 245° was found to absorb 526 times its volume of hydrogen, the gas being measured at the ordinary temperature; but it was found that even the temperature of 245° exceeded the most suitable point, and that at the temperature of 100° the metal absorbed 643 times its volume of the gas. Now, if it is a difficult thing to conceive how 15 cubic centimètres of hydrogen should be compressed into the interspace existing in a cubic centimètre of platinum, still more difficult is it to conceive how 6 mètres of centimètres of hydrogen should in the same way be compressed, not into one cubic centimètre of space, but into so much of what seems to be a cubic centimètre of palladium as is not really occupied by the palladium. At a temperature falling short of 20°, that is without the limits of ordinary atmospheric temperature, the palladium still absorbed 376 volumes of gas.

In the case of spongy palladium, of which I have here a specimen, there was not such a difference between it and the wrought palladium, as there was between the spongy platinum and the wrought platinum. It was found that the spongy palladium absorbed 680 times its volume of gas, while the wrought palladium absorbed 643 times.

Here is some wrought palladium foil which has been charged in the manner I have described, and now contains some six or seven hundred times its volume of hydrogen locked up or occluded in it. This hydrogen does not come off appreciably, or only in very small proportions, until the metal is heated, but on the application of heat, we shall be able to collect the gas with facility. The piece of foil has been charged with hydrogen gas at a temperature of 200°, then exposed freely to the air, and afterwards introduced into the tube. The tube is then exhausted, and now we are beginning to heat it. On the application of heat, the hydrogen gas will be given off and delivered in the test tube as in the former experiment, the only difference being that in the former experiment, we sucked the hydrogen through the tube, and in this case we are extracting the hydrogen which has been absorbed by the metal. It will take some little time for us to collect any considerable quantity, but you see already that the hydrogen gas which was absorbed by the palladium, is now being given off by it. Mr. Roberts, to whose zeal Mr. Graham and myself are both much indebted, has now got the hydrogen in the test tube, and on my applying a light, you perceive the combustion of the hydrogen gas, which we have just extracted from the charged palladium.

The hydrogen gas condensed in this way in the substance of the palladium is capable of exerting certain chemical actions which hydrogen gas in its ordinary state is not. We find that this condensed hydrogen acts as nascent hydrogen, exhibiting all those reducing actions which are characteristic of nascent hydrogen. To give you an illustration of some of these chemical actions of the condensed hydrogen, we have here a solution of permanganate of potassium, into which we will introduce a portion of the palladium charged with hydrogen. The action will not be immediate, but after a little while you will see that the permanganate will become decolorised by the action of the hydrogen which has been absorbed into the palladium. Here we have some solution of the ferricyanide of iron, and on putting into it a portion of the charged palladium, we shall have a gradual development of Prussian blue. The difficulty of the experiment is that the palladium, being heavy, sinks, and does not come much into contact with the liquid; but even now you see that a certain amount of blue tinge has been imparted. You also see that in the other vessel which contains the permanganate a certain amount of bleaching action has taken place. Hydrogen, in this form, will also bleach the iodide of starch. If we introduce into that compound some of this hydrogenetted palladium, hydriodic acid will be formed, and the blue colour of the iodide of starch disappear, though the action will require some little time. In the case of the ferricyanide of iron solution a very decided blueing has now taken place, which will increase to a yet more obvious extent. The permanganate is already almost bleached by the reducing action of the hydrogen absorbed into the palladium, while the colour of the iodide of starch is gradually disappearing. By these results you see that hydrogen is capable, when thus absorbed, of producing those characteristic chemical effects which, under ordinary circumstances, are only observed of so-called "nascent hydrogen."

There are a great number of other interesting points connected with the absorption of gases by these two metals, palladium and platinum; but my time is getting on so rapidly that I must proceed if you please to consider the absorptions manifested by some other metals. In the case of copper it is found that this metal, in the form of wire, will absorb 30 per cent. of hydrogen, whereas in the spongy form it will absorb 60 per cent. Gold is unlike platinum in this particular—that it will absorb a great number of different gases, whereas platinum absorbs hydrogen only. Gold, in the form of assay cornettes, was found to absorb 48 per cent. of hydrogen, 29 per cent. of carbonic oxide, 16 per cent. of carbonic acid, and 20 per cent of air, but of this air, absorbed by the gold, nearly the whole was nitrogen. Whereas ordinary atmospheric air contains 21 per cent. of oxygen, the air absorbed by gold contained only 5·3 per cent. of oxygen. Gold, therefore, seems to be a metal which is singularly indifferent to oxygen. Before charging the cornettes with carbonic acid or carbonic oxide it was necessary to ascertain that they did not contain any gas in the first instance. But it was found that they really did contain a considerable proportion of what may be called natural gas, which had to be removed from them. The gas amounted to 212 per cent. The gold cornettes actually contained twice their volume of natural gas, which consisted chiefly of hydrogen and carbonic oxide, with the exact proportions of which I will not trouble you, and which had been absorbed from the muffle furnace in which the cornettes had been originally heated.

(To be continued.)

ACADEMY OF SCIENCES.

June 24, 1867.

(From our own Correspondent.)

M. Chevreul, president, read a letter from the Minister of Public Instruction announcing to the Academy that the election of M. Yvon de Villarceau in the section of Geography and Navigation was approved by an Imperial decree of the 19th June.

M Elie de Beaumont read a letter from M. Agassiz, dated last November, in which this *savant* gives details on his voyage up the river Amazon and its tributaries. He has found that that part of the American continent is formed of mud or diluvium resting on a cretaceous deposit, similar to the basin of the Seine or banks of the Somme.

M. Rouget addressed a memoir in which he differs from the opinions lately put forward on muscular contraction.

M. Thomas gave a paper on cholera remedies.

M. de Paravay, on ancient Chinese books.

M. Edward Robin, on the length of life.

M. Clausius, now in Paris, presented a copy of his work on the "Theory of Heat."

M. Velpeau sent a note by Trubart on a new mode of the introduction of medicaments. He had only made a small number of experiments, which he communicated in order to take date. For cephalalgia, ophthalmia, etc, he uses remedies introduced by the nostrils.

M. Charles Deville communicated the observations made by M. Janssen on the eruption of Santorin, and submitted the flames to spectrum analysis. The results are, the presence of sodium in abundance, carbon, chlorine, and copper. Experiments made on Stromboli gave the same results.

M. Balard presented the work of M. Friedel on silicates and other chemical products.

M. Jules Pernot, of Avignon, sent a note on the preparation of silicates and other chemical products. He has obtained a first-class material for calico-printing, etc.

M. de Pambour read a paper on hydraulic machines.

The Academy named two commissions, one for prizes for statistics and another for the Bordin prize.

M. Artur gave some explanations of his theory of the molecular actions, capillary attractions, and chemical decompositions.

M. Zalewski read a note on the method of augmenting the power of the Bunsen pile.

M. Caron criticised the artificial processes for preparing substitutes for milk, and concluded by pronouncing them indigestible and injurious to infants.

M. Daubrée read a note of M. Bonafous on the meteor seen on June 11, at 8h. 25m.

July 1, 1867.

New Volcanic Islands.—Works of Lagrange.—Sir D. Brewster on Lighthouses.—Aniline Colours on Cotton.

M. Chevreul, president, opened the meeting, and in the absence of the perpetual secretary read the correspondence, minutes, etc.

M. C. St. Claire Deville handed in a note inserted in a Portuguese journal, the *Perseverangia*, announcing that between Tersira and Graciosa, two islands near Lisbon have been subjected to continual volcanic eruptions; very strong shocks of earthquakes have been felt, and have produced many islets, one after the other, analogous to those of Santorin in Greece. On the 1st June a submarine volcano cast up igneous matters in such quantity, that a tongue of land has been formed with the continent. This ground is as yet unapproachable, on account of the incandescence of the rocks, as well as the sulphurous vapours from the fissures. M. Deville asked that the Academy should take an interest in these new eruptions as it did in those of Santorin.

M. Treuil read a note on the lactiferous vessels of fig-trees and euphorbiacea.

M. Serrett presented to the Academy the first volume of the works of Lagrange, which were published under his direction by order of the Minister of Public Works. This great work consists of eight volumes of 1,800 pages each, with plates, and will be distributed to each member of the academy. They contain papers on mathematics, chemistry, physics, anatomy, hydraulics, etc.

Sir David Brewster sent a very interesting memoir on the history of lighthouses and dioptric apparatus.

M. Morris sent a copy of his book on wine-making, etc.

M. Fournet, of Lyons, sent a great memoir on atmospheric electricity.

M. Chevreul read a note from M. Biddeman, at Berlin, on aniline colours for cotton.

The meeting then resolved itself into a secret committee to elect a member in place of M. Pelouze, deceased. MM. Cahours, Berthelot, and Wurtz are proposed; M. Wurtz will most likely succeed to the vacant chair.

July 9, 1867.

Practical Meteorology.—Letters from Routrou *to* Cardinal Richelieu.—*Alleged Discovery of the Law of Gravitation by* Pascal, *prior to* Newton.—E. Becquerel *on Capillary Chemistry.—Researches on Benzol and its Derivatives.—Cause of Tubercular Disease.*—Daubrée *on the Classification of Meteoric Stones.—Theory of Volcanic Upheavals—Telescopic Views of the Great Nebula in Orion.—Election of* Adolphe Wurtz, F.R.S., *to the Academician's Chair, vacant by the Death of* Pelouze.

M. Chapelas-Coulvier-Gravier presented to the Academy a small volume which he published under the title of "Practical Meteorology."

M. Chasles presented to the Academy two letters from the poet Routrou to Cardinal Richelieu. In the first, Routrou advises the celebrated minister to found, in Paris, a literary society analogous to that of the floral games of Toulouse; in the second he thanks him for having seriously entertained the proposition by founding the French Academy. To these presents M. Chasles added two other letters, one from Routrou to Corneille, and another from Corneille to Routrou. M. Chevreul reminded M. Chasles that he told him one day that he (the latter) was in possession of two autograph letters, from which it resulted that Pascal had discovered and calculated, before Newton, the law of universal gravitation of masses, in the inverse ratio of the square of the distance. Pascal was born in 1623, died in 1662, and Newton only made his great discovery in 1665. If it is true that, in documents written by his hand, Pascal had established the law of gravitation, it is certain that he was in advance of Newton. The documents M. Chasles possesses are: the first, a letter written by Pascal to Robert Boyle, the illustrious physician; the other is a note certainly written by his own hand. M. Chasles promised to bring to the Academy, on Monday next, the two precious documents, and to make them the subject of a more explicit communication.

M. Becquerel, sen., read a third communication on capillary chemistry. He pointed out new facts of chemical decompositions taking place under the influence of capillarity, and he thought he has proved that these truly curious phenomena were produced under the triple influence of affinity, capillarity, and electricity. To demonstrate the intervention of electricity, M. Becquerel has made the following experiment: he immersed his *split* bell-glass, containing nitrate of copper, in a second bell containing a solution of monosulphide, as in the first experiments; then he dips the two extremities of a silver wire, one into the nitrate and the other into the monosulphide. A constant electric current is formed: 1. The deposit of silver is made not in the capillary slit, but on the iron; 2. When the wire is removed the deposit is formed in the slit and on the edges along the side of the split bell-glass. The capillary action is as powerful as an electrical action. M. Becquerel continues to improve his experiments; for the split bell-glass he substitutes prisms of crystal glass pierced with a small hole; the slit or fissure is replaced by plates of glass with edges in contact, or even by sand; and he has thus obtained effects of silvering, gilding, platinising, and very remarkable deposits of gold, silver, nickel and cobalt.

M. Zinin, of the Academy of Sciences of St. Petersburg, read a *resumé* on his researches on benzol and its derivatives, azobenzol, azoxybenzol, etc.

M. Velpeau read a letter in which M. Lebert, of Breslau, states that he thinks he has found the cause of tubercular disease in the shortening of the pulmonary artery.

M. Daubrée enumerated the bases of the classification and methodical arrangement of the collection of meteoric stones and iron of the Museum of Natural History.

M. Edmond Becquerel communicated an observation, of M.

Janssen, from which it results that the oscillatory motions of volcanic upheavings are always perpendicular to the faults that can be compared to an opening, the edges of which open and shut by turns.

Father Secchi, of Rome, handed in two telescopic views of the nebula in Orion, made in 1859 and 1865.

The Academy then formed into a committee for the discussion of the claims of the candidates for the chair in the chemical section, rendered vacant by the death of M. Pelouze. The section presented in the first place M. Wurtz, and secondly in alphabetical order, M. Berthelot, and M. Cahours. M. Wurtz, the discoverer of glycol, and compound ammonias, is most likely to be elected unanimously. The discovery of glycol and compound ammonias gained for him the prize of £400. His "Chemical Philosophy" has been translated into many languages—into English by Mr. Crookes, etc.; his claims are of the first order, even in the presence of the innumerable discoveries of M. Berthelot, and the long and glorious labours of M. Cahours.

July 15, 1867.

Pre-Newtonian ideas of gravitation—Marine Engines—Election of M. Wurtz *as a Member of the Academy—Perfluoride of Manganese—Isolation of Fluorine—Oxychloride of Magnesia to replace Plaster of Paris.*

M. Chasles laid on the table the letters and notes from Pascal alluded to at the last meeting. It is incontestable that in these autographs, the date of which is certainly anterior to 1662, the year of the death of Pascal, the illustrious philosopher speaks of attraction at a distance as well as at the surface of the earth and in the midst of the celestial spaces, and even of molecular attraction, in the same terms as Newton. He makes the same calculation of bodies falling in an orbit, of mass, of distance; he lays down the same laws, etc.

M. Du Puy de Lome made a long and interesting communication on the marine engines of the ship of war Friedland, of the French navy, with three cylinders and of 1,000 nominal horse-power, and an effective power of 4,000, which will give the ship a speed of fourteen knots an hour with sixty revolutions of the screw per minute.

The academy then proceeded to the election of a member in place of M. Pelouze. The number voting was 53; M. Wurtz was elected with 45 votes against 3 given to M. Berthelot, and 2 to M. Cahours. It is, as we have before stated, almost the unanimity of votes; there were two blank billets, that is to say, that two of the illustrious did not consider MM. Wurtz, Berthelot, and Cahours worthy of sitting beside them, and did not give themselves the trouble of choosing between them. It is melancholy to see academicians imbued with feelings of such a foolish disdain, or evincing publicly so great a meanness of spirit.

M. Dumas announced that M. Nicklès, of Nancy, had just made a discovery in mineral chemistry by showing how to prepare a combination of fluorine and manganese, which is to the simple fluoride what the binoxide of sesquioxide is to the simple oxide, the protochloride, and the protoidide to the deuto, sesqui, or polychloride, or iodide. M. Nicklès has found that the new combination, deutofluoride, or fluoride of fluoride of manganese, is less stable than the analogous chloride or iodide. Moreover, and this gives great importance to the discovery, the deutofluoride will certainly be less stable than the simple fluoride of manganese, and it seems difficult to believe that we shall not one day succeed in decomposing it by heat, or otherwise, into fluorine and fluoride of manganese, under conditions which will permit the fluorine to be isolated, and so fill up a great gap in chemistry.

M. Dumas then called the attention of the academy to a new industry of M. Sarel, which nothing could lead us to foresee, viz., that chloride of magnesium can unite and associate with magnesia or oxide of magnesium to form an oxychloride of magnesium perfectly insoluble, and possessing, as does the oxychloride of zinc, in a degree incomparably greater than plaster of Paris, the property of not only taking all variety of forms, but of causing the solidification and taking a high polish of a great number of substances with which it may be mixed, in the proportion of a fifteenth to a twentieth of their weight. Experiments made two years ago leave no doubt on the good quality of stones prepared by this process, and the absolute resistance, of objects so fabricated and moulded, to the deleterious action of water. Industry and art will therefore enter into possession of new elements of construction and transformation. The chloride of magnesium that can be extracted from sea water, or which is found in great quantities solidified in interior seas as that of Stassfurth, does not require to be entirely pure, and costs less than the oxychloride of zinc.

NOTICES OF BOOKS.

MINERALOGICAL PAMPHLETS.

An Index to Mineralogy. By T. Allison Readwin, F.G.S., F.S.S., etc. London: E. and F. N. Spon, Charing-cross.

Royal Agricultural College, Cirencester. A Guide to the Chemical Department of the College Museum. Part I. The Mineral Collection.

Sketch of the Mineralogy of Nova Scotia, as Illustrated by the Collections of Minerals sent to the Paris Exhibition, 1867. By Professor How, D.C.L., University of King's College, Windsor, N.S. Published by authority of the Commissioners for Nova Scotia.

Mr. Readwin, in the preface, states that the list is obviously imperfect, and that he hopes it will elicit correction at the hands of chemists and others. The index is a list of some 2,500 minerals, with synonymes, the constituents expressed by contractions in a small space, and the number of the case in the British Museum containing specimens. We take an example showing the method Mr. Readwin employs:—

"Eulysite (chrysolite) Mg. Sil. B. M. 36 (var. Olivine.)"

By an explanation at the commencement of the book the student finds eulysite to belong to the same species of mineral as chrysolite. The remainder of the information with regard to the mineral is evident at once.

It is a little work which will be appreciated by those interested in mineralogy.

The aim of the writer of the second pamphlet—Professor A. H. Church—is merely to describe the more important minerals to be found in the museum. There is no attempt at being encyclopædic. The minerals are classed under six divisions, and the most important mineral species belonging to each division are pointed out. The information is given in a very clear and concise manner, and were it not published in the form of a guide, we should be inclined to call it brief to a fault.

It is stated by Professor How that the collections of minerals made on the present occasion are sufficient evidence that the mineralogy of Nova Scotia is very interesting, both from a scientific and an economic point of view. In the first place gold is obtained in considerable quantity. Very valuable iron ores are also worked, ores yielding bar iron which ranks with the finest Swedish metal for making steel. Ores of manganese are worked, and the value of that sent from Teny Cape up to the present equals £8,000 or £9,000. Wad, manganite and pyrolusite are exhibited. Native copper and other ores are to some extent worked. A variety of copper ores are exhibited, copper pyrites, cupriferous oxide of iron.

Galena is also represented from several localities. Mispickel is also exhibited from three or four localities, and is sometimes found in large amount; cobalt occasionally occurs with it. Barytes occurs in some places in sufficient quantity to be exported. Gypsum exists in inexhaustible profusion; natroborocalcite, and a mineral containing 59 per cent. of boric acid—cryptomorphite—have been found embedded in it. Both are exhibited. Anhydrite occurs in quantity. Other products are marble, limestones, granite, sandstone, etc. The author, we think, certainly proves his opening statement.

The Alkali Act, 1863. *Third Annual Report by the Inspector, of his Proceedings during the Year* 1866. By Dr. Angus Smith, F.R.S., etc., Government Inspector.

It is gratifying to find that Dr. Angus Smith is able to state in this, his third report, that there has been a further advance in the manner of preventing the escape of muriatic acid gas; and that, although during the last year the escape has been greater in the actual number of tons, it is owing to the increase in the manufacture. The amount of salt decomposed during the first year of inspection was 288,000 tons, during the second 310,000, and during the third it is 371.950. The necessity for inspection is well borne out by many of the facts related in connection with them.

For instance, in one case, where there appeared to be all the ordinary contrivances for condensation, 20·5 per cent. of the muriatic acid was found to be escaping. Another test, taken after alterations had been made, with a view of checking the escape, showed the presence of 13·5 per cent. The owner was prohibited from working with these arrangements. He stopped work, and erected a second condensing tower, but the escape was still 15 per cent. The inspector again prohibited work until a remedy was found. The arrangements ultimately were properly made, and at a subsequent inspection the escape was only 1 per cent. Dr. Angus Smith writes—"The struggle with the condensers may be said to have ceased; the circumstances are understood. Our new struggle is with this escape from the works, and of a certain amount of gas which passes through bricks, tubes, and even stone, as well as of that coming occasionally from the mouth of the furnace."

The author remarks upon the way in which the manufacturers have come to regard inspectors. Finding that in many manufactories there was not a sufficient staff with chemical knowledge, the inspector has given advice with regard to the alterations required, and if the changes have been speedily made, has refrained from prosecution. A letter from a manufacturer illustrates the feeling referred to. In the course of it he writes: "It is very annoying to me that you should ever find anything to find fault with, but as our method of working is uniformly the same, and as we have no means of knowing when the condensation is right except from your tests and inspections, I hope you will not be long before you come again, and, if possible, often." The author of the report says: "I consider this letter an indication that the time has now come for throwing aside much of the responsibility which we have taken from the manufacturers, or they will turn round to throw on us the blame of any infraction of the Act committed in their own works." It would appear that the proprietor found that in the visits of the inspector he obtained such valuable advice as to save him the expense of attaching a chemist to his establishment. No doubt great good has been done by the excellent spirit in which the inspections have been made, but now that manufacturers are making greater profits by the larger amount of acid they are enabled to obtain from the same amount of material, and likewise by the saving in many cases of the amounts formerly paid as damages, we consider that Dr. Angus Smith, and those gentlemen under his direction, have no longer any need to burden themselves with duties which, as inspectors, they were in no way bound to undertake. The author is evidently not inclined to a radical change in this direction, as the following extract in reference to the peremptory demand of good condensation shows:—"When the habit of condensing or of destroying all noxious vapours has been longer confirmed, this demand may grow to be just and reasonable." In another place he remarks that if the number of works under the inspectors were much increased, there would be no time left in which to consider and advise manufacturers. At the end of the report are a few pages devoted to the consideration of what is meant by a nuisance.

Dr. Angus Smith shows, we think very clearly, an advantage of inspection. This, although a minor one, is of great importance to manufacturers, as they possess one interest, and the public another. It is in reference to the subject of a *nuisance*—a word conveying an excessively vague meaning—that the author draws attention. At present, he says, excepting those factories coming under the Alkali Act, the public are not protected from the manufacturer, neither is the latter protected from the public. Take, in the first place, the case of a housholder who suffers from the escape of noxious gases evolved from a nest, such as in London is found at Belle Isle. The processes, which may be bad enough when properly carried on, are probably clumsily managed. He must of course be able to fix the nuisance upon a particular factory, and this is one great difficulty. In such cases the evidence is characterised by great uncertainty; even a chemist, as Dr. Angus Smith says, is frequently obliged to trust chiefly to his senses, as he cannot obtain admission to the factory, and therefore the gases may be so diluted where he examines the air as to make it difficult to prove their presence. The following sentence, occurring in the report, exactly expresses our own opinion upon this point:—

"The want of protection to the public lies, in ordinary nuisance cases, in the want of power to enter the works and to make experiments."

In another place the author writes, "Unless chemists can define a nuisance, and have opportunity of examining in cases of complaint, neither the public nor the manufacturer is protected."

Taking now the case of a manufacturer, a case is stated showing that he is liable to be attacked and injured unjustly:—

"It sometimes happens," as the author puts it, "that a neighbour who is peculiarly sensitive, objects to the smell of even well-conducted works; he may be able to say truly that they are to him very offensive, and he may have no idea that, were all men to have an equally acute sense of smell, manufactures would cease. He may, however, care little for such things, and be determined to seek his own comfort only, and he, therefore, on oath and with truth, says that the smell makes his house unpleasant."

The laws referring to nuisances, we are told, are also vague, owing to the want of precise modes of detecting them.

This is not the case with those which come under the Alkali Act. Here there is complete protection up to a certain point, to both manufacturers and the public. We quote once more to give the opinion of the author, who is necessarily an authority, upon this point. He says:—"It seems to me that the system of inspection protects both sides if the inspector has his direct instructions, as under the Alkali Act, to look for the condensation of a distinct amount. The public is freed from 95 per cent. of muriatic acid by this Act, and the manufacturer is protected from the complaints of the public so far. If a similar fixed point could be adopted in the case of every gas, there would be complete protection to the public and manufacturer on both sides up to that point, occasional mistakes and accidents excepted. It seems to me a most important thing to seek such fixed points, and where they cannot be attained to make the nearest approach to them, so that evidence may be taken by competent persons on the spot, instead of by persons stretching at some distance beyond the works the capacities of their sense of smell."

The Act referring to the manufacture of hydrochloric acid has worked so well that we would urge upon the Government the necessity of the inspection of all similar manufactures. There can be little question that the Alkali Act has resulted in benefit to the manufacturers who were brought under its influence. Imagine a manufacturer of hydrochloric acid (we are willing to believe it an exceptional case) allowing an escape of 20 per cent.!

In an appendix Dr. Angus Smith describes an apparatus, and the method of using it, for the determination of the speed of air in flues. We must refer those of our readers who may be interested to the original, as the paper is long, and not capable of condensation.

CORRESPONDENCE.

A Lecture Experiment.

To the Editor of the Chemical News.

Sir,—The following simple apparatus which I contrived lately for illustrating the manufacture of sulphuric acid, may be interesting to some of your readers. Three tubes are passed through the cork of a wide-mouthed bottle, the largest being connected by an india-rubber junction with a pint funnel, and the small one to the left with a test-tube generating NO by means of copper turnings and nitric acid. The middle tube admits air. A little water is poured into the bottle first, to combine with SO_3 for the production of H_2SO_4. The funnel is covered in with a metal cap to which a small pan is suspended. This pan is a miniature furnace. A bit of sulphur is placed in it and lighted. The fumes of SO_2 immediately flow down in a conspicuous stream into the bottle. Here they encounter NO, and the usual reaction takes place. Any SO_2 which the water may dissolve is expelled by boiling, when the solution answers to all the tests for the presence of sulphuric acid. An extra cover which slides on the metal lid, conceals some air-holes, useful at the beginning of the experiment. Hoping this modification of the apparatus described by Miller for a similar purpose, may be acceptable as an exact and economical imitation of a most interesting manufacturing process,

I am, etc., E. S.

Nottingham, June 6, 1867.

Extinction of Fires.

To the Editor of the Chemical News.

Sir,—Will you allow me to make a few remarks respecting a fire which took place the other day in an oil distillery at Hackney Wick? I happened to be passing at the time, and watched the progress of the flames. We all know that from whatever cause a fire takes place, water is rushed to for putting it out. In the case I refer to, the place was, as usual, drowned with water, which merely had the effect of spreading the flames and increasing their intensity, for the oil burned until there appeared to be nothing left to support the flames. Now my reason for sending you this letter is to point out how easily this fire might have been put out if a simple plan, which I shall mention, had been adopted, and there is no doubt that many similar fires could be extinguished by the same means. At the fire referred to I noticed the flaming oil floating on the surface of the water on the floors. The water running down the walls bore a flaming surface of oil likewise. This shows that the water had little or no power over the burning oil.

There was lying near the building in which the fire broke out, a large quantity of sand. Now if half-a-dozen men, provided with spades, had "dashed" a lot of this sand upon the flames soon after the fire was discovered, I have no hesitation in saying that it would have been put out, and but little damage done. But this was not the case, for long before the engines arrived, the fire had got such a hold upon the building and its contents that the firemen's work was little better than labour in vain, for the place was completely gutted. The sudden throwing of sand or any similar substance upon masses of flame proceeding from burning oil, etc., is generally sufficient to extinguish or choke them out.

Some time ago I put out a fire, which might have destroyed an immense amount of valuable property, by simply dashing fifty or a hundred shovelfuls of slaked lime, which happened to be near at hand, upon the flames, which literally choked them out. The fire in this case was caused by a cask of oil being set on fire accidentally. This is only one of the many fires which I have seen put out by adopting the same means. I consider it would be a good plan if owners of such places as oil works, etc., always had at hand a quantity of sand, dry old lime waste, etc., which could be used in the manner I have stated when necessary.

I am advancing no fancy statement, but giving your readers a plan which has been tried with every success, and I could write much in favour of this plan did I think such a course at all necessary.

I am, etc., T. H. Swindells,
Consulting Chemist.

July 2.

Use of Distilled Water.

To the Editor of the Chemical News.

Sir,—In Mr. Quin's report upon the Paris Exhibition, reference is made to the use of distilled water at the Wallaroo Copper Mines in South Australia, stating that until tanks for collecting rain-water had been constructed, "perhaps for the first time in the history of the world, there was a population of some thousands, with all their horses, cattle, sheep, etc., drinking *aqua distillata*." As many of your readers may not be aware of the fact, it may be interesting here to mention that in the rainless region of the Pacific coast of South America, the entire population of the country, between about the 18th and 28th parallels of south latitude, or some 600 miles from south to north, including the important towns of Caldera, Cobija, Iquique, Pisagua, and several minor ports, have for many years derived their supply of potable water from the sea water of the Pacific, distilled in greater part by coal imported from England and costing above £3 per ton.

Not only is a population of many thousand inhabitants, principally engaged in the mines of this district, as well as a still larger number of beasts of burden, and other animals, supplied from this source, but even the locomotives on the Copiapo and Caldera railway, and some steam engines for other purposes, are actually driven with distilled water. For a distance of some thirty to fifty miles inland from the coast, very few natural springs are met with in this rainless desert, and when met with they are seldom sufficiently free from saline matter to be potable. D. F.

Magnetism and Gravitation.

To the Editor of the Chemical News.

Sir,—The paper by Mr. Newlands appears to me to be based in great part, if not entirely, on erroneous notions regarding magnetism.

If the distance between the pole of a magnet and a magnetic body is very considerable as compared to the size of the latter, the body will not be attracted, inasmuch as two opposite poles are always produced by induction; and as, under the above conditions, both these induced poles may be regarded as at the same distance from the inducing pole, the one will be repelled exactly as strongly as the other is attracted, the result, as regards attraction, will therefore be *nil*. The distance of the poles of the earth being almost infinite compared to the size of a piece of iron on the pan of our balance, the iron will not be attracted by the magnetic power of the earth, and will weigh as much on the equator as on the poles, subject only to the altered force of gravity. I am, etc., A. D.

Vapour Density of Water.

To the Editor of the Chemical News.

Sir,—The apparent discrepancy between the two modes of calculating the expansion of water in becoming steam noticed by Mr. F. O. Ward, in your last number, arises from a slight error in one of the calculations, which should be as follows:—

	Grammes.
2 litres of H at 0·08936[*] = ...	0·17872
1 litre of O at 16 × 0·08936 = ...	1·42976
	1·60848

The 3 litres being condensed into 2, a litre of aqueous vapour at 0°C. and 760 mm. weighs .... $\frac{1{\cdot}60848}{2} = 0{\cdot}80424$

and hence (neglecting the slight expansion of water in cooling from its point of maximum density 0°) 1 litre of water at 0° would become $\frac{1000}{0{\cdot}10424}$ or 1243·4 litres of aqueous vapour. The steam, however, being formed at 100°, will become expanded to $\frac{373}{273} \times 1243{\cdot}4$ or 1,699 litres, so that again neglecting the slight expansion of the water in heating from the point of maximum density to 100°, 1 litre of water at 100° becomes 1,699 litres of steam at 100°.

The vapour density of water (observed, air = 1·0000), and the weight of a litre of air at 0° and 760 mm. as given in Cooke's *Chemical Physics*, p. 693, are respectively 0·6235 and 1·29206 grammes: hence the weight at 0° and 760 mm. of a litre of aqueous vapour will be 1·29206 × 0·6235, or 0·8056 grammes, which does not differ much from the previous theoretical number 0·80424, and corresponds to an expansion at 100° of 1,696 times since $\frac{1000}{0{\cdot}8056} \times \frac{373}{273} = 1{,}696$.

—I am, etc.,

Charles R. A. Wright, B.Sc.

Chemical Laboratory, St. Thomas's Hospital,
July 13th, 1867.

To the Editor of the Chemical News.

Sir,—The few editorial remarks which appeared in your issue of the 12th inst., induced me to look over Mr. Ward's statements on the above-named subject. I think I may safely advance that Mr. Ward has erred in this instance to the extent of the difference indicated between his numbers and the experimental results. Adopting the specific weights which he gives for the volume of the water constituents, the weight of a litre of steam at 0°C., and ordinary pressure, would not be 7·2576 grms., as Mr. Ward states, but 0·8064 grm. It follows, therefore, that a litre of water would produce a volume of steam at 100°C., which, when corrected to 0°, would amount to

$$1241{\cdot}3 \text{ litres } \left(\frac{1000}{0{\cdot}8064} = 1241{\cdot}3\right).$$

According to Mr. Ward's figures this volume should be 137·77 litres $\left(= \frac{1000}{7{\cdot}2576} = 137{\cdot}77\right)$, although he enlarges the result to 1377·8 litres.

Gay Lussac's experimental determination of 1,700 litres as the volume a litre of water acquires when converted into steam by a temperature of 100° if corrected to 0°C. corresponds very nearly with the volume of 1241·3 litres, for $\frac{1700}{1{\cdot}3665} = 1244$.

The corrections to 0° cannot make any important difference in the ratios; and I may state that the relation of volumes deduced by theory, or obtained by experiment, would be coincident at any point intermediate between 0° and 100°. So far, therefore, we may believe that perfect harmony exists between the theoretical and experimental results in relation to the expansion of water into steam at 100°.

Indeed, it is beyond my capacity to comprehend how any difference could arise here between theory and experiment, or that in the entire series of volume-ratios any discord or diversity could be educed under similar or analogous conditions to those referred to.—I am, etc.,

Martin Murphy.

College of Chemistry, Liverpool, 13th July, 1867.

To the Editor of the Chemical News.

Sir,—The assertion that water expands 1696-fold in becoming steam at 212°F., rests on the authority of Gay Lussac, and is a result of direct experiment. He found that 1 c.c. of water gives 1626·4 c.c. of steam measured at 100°C.

Now, 2 litres of H weigh....................	0·1792
1 —— O ——	1·4336
	1·6128

1 litre of steam at 0°C. weighs, then, 0·8064 grm. Hence, 0°8064 c.c. water at 4°C. becomes 1000 c.c. steam at 0°C. and 1367 c.c. at 100°C., or 1 c.c. water becomes 1695·2 c.c. steam at 100°C.

The difference between theoretical and experimental results amounts thus not to 8 per cent. but to 0·08 per cent.

There are three errors in Mr. Ward's calculations, which, however, counteract each other rather curiously.

$16 \times 0{\cdot}0896 = 1{\cdot}4336$, not 14·336

$\frac{1000}{7{\cdot}2576} = 137{\cdot}78$ not 1377·8

I am utterly unable to understand the method adopted for the correction for temperature. I may remark, however, that the coefficient of expansion of gases for 1°F. is

$$\frac{1}{491} \text{ and not } \frac{1}{460}$$

and that the correction should be made from 212°F. to 32°F., and not to 60°F.—I am, etc.,

W. M. Watts, D.Sc.

Glasgow, July 16th, 1867.

To the Editor of the Chemical News.

Sir,—I observe some calculations under the above heading in this week's Chemical News. If Mr. Ward had made his calculations correctly, he would have had no difficulty in reconciling the fact that water expands 1·696-fold when converted into steam at 212° F., with the vapour density of water.

It is not necessary to point out all the errors of calculation. The following calculations will show most of them when placed in juxtaposition, and it is only necessary to state that the weights of hydrogen and oxygen are taken from "Bunsen's Gasometry."

	Grammes.	
2 litres of H at 0·08961 = ...	·17922	at 32° F.
1 litre of O = ...	1·43028	"
2 litres of water vapour = ...	1·60950	"
1 litre of do = ...	·80475	"

$$\frac{1000}{{\cdot}80475} = 1242{\cdot}6$$

$1242{\cdot}6 \times 1{\cdot}3665 = 1698{\cdot}0$ = volume of vapour at 212° F., from unit of water of maximum density (at 39·2° F.). 1,696 has probably been arrived at by cutting off fractions; by multiplying 1,242 by 1·366 we get 1696·5.—I am, etc.,

D. H.

To the Editor of the Chemical News.

Sir,—In Mr. F. O. Ward's calculation of the vapour density

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[*] The weight in grammes of a litre of hydrogen at 0 deg. C. and 760 mm. pressure.—*Roscoe's Chemistry*, p. 19.

of water a mistake occurs, which, if corrected, will remove the discrepancy noticed by him.

$$\begin{array}{lrcl} & & & \text{Grammes.} \\ \text{2 litres H at} & 0{\cdot}0896 & = & 0{\cdot}1792 \\ \text{1 litre O at} & 16 \times 0{\cdot}0896 & = & 1{\cdot}4336 \\ \hline & & & 1{\cdot}6128 \end{array}$$

3 litres becoming 2 we have $\frac{1{\cdot}6128}{2} = 0{\cdot}8064 = 9 \times 0{\cdot}0896$ as the weight of one litre of steam at 0°C., and 760 mm. pressure. A litre of water weighing 1000 grammes, at 4°C. or 999·88 grammes, at 0° will therefore give

$\frac{999{\cdot}88}{0{\cdot}8064} = 1239{\cdot}9$ litres steam at 0°C. and 760 mm. pressure.

Reducing the alleged 1·696 litres at 100° for temperature, we get $\frac{1696}{1{\cdot}307} = 1240{\cdot}6$ litres at 0° and 760 mm. pressure, or almost exactly the theoretical quantity, assuming steam to possess the same coefficient of expansion as atmospheric air, viz., 0·367 between 0° and 100°, and under constant pressure. The *crith*, it should be remembered, is the weight of a litre of gas or vapour at 760 mm. pressure, and at 0°C., and not at 60°F.—I am, etc.,

A. D.

Vapour Density of Water.—"Chemistry of the Future."

To the Editor of the Chemical News.

Sir,—I notice, with not unpleasurable surprise, that you have bestowed the honour of publicity on some remarks of mine, not penned with a view to impression, and wanting, I see, some little revision. They refer to the discrepancy of the figures representing the vapour density of water, as on one hand deduced from its received volumetric constitution, and as computed, on the other hand, by correcting, for temperature, the ordinary statement of the expansion of water in becoming steam at 212°F., viz., 1696-fold. The accidental misplacement of a decimal point, and a casual error in the reduction of boiling point to 60° F. have led to the working out of the discrepancy as greater than it really is. I subjoin the corrected figures, from which it will be seen that the discrepancy is still large; leaving me nothing to alter in the remarks founded thereon.

Computing the vapour density of steam from its volumetric constitution, we have:—

$$\begin{array}{ll} \text{2 litres of H}=1 \text{ at } 0{\cdot}0896 \ldots\ldots\ldots\ldots & 0{\cdot}1792 \\ \text{1 litre of O}=16 \times 0{\cdot}0896 \ldots\ldots\ldots\ldots & 1{\cdot}4336 \\ \hline \text{3 litres weighing} \ldots\ldots\ldots\ldots & 1{\cdot}6128 \end{array}$$

These 3 litres being condensed into 2, we have

$$\frac{1{\cdot}6128}{2} = 0{\cdot}8064$$

as the vapour density required. As a litre of water weighs 1000 grammes, its expansion ratio, in becoming steam at ordinary temperature and pressure, is obviously

$$\frac{1000}{0{\cdot}8064} = 1240$$

Turning now to the usually alleged expansion-ratio, 1696, and reducing it for temperature, at the rate of $\frac{1}{460}$ per degree F., for the difference between boiling point and 60°F.=120°, we have the ratio

$$\begin{array}{ll} \frac{340}{460} \times 1696 = \ldots\ldots\ldots\ldots & 1253{\cdot}5656 \\ \text{which, set against the above computed} \ldots\ldots & 1240{\cdot}0000 \\ \hline \text{Shows a discrepancy of} \ldots\ldots\ldots\ldots & 13{\cdot}5656 \end{array}$$

This is the point to which I called my friends' attention, with reference to the question whether the volumetric relations of bodies are so symmetrical as is our present disposition to suppose.

This touches a subject now strongly attracting the attention of the chemical world—I mean the nature and constitution of matter and the mode to be preferred of conceiving and representing its chemical transformations. With reference to these deeply interesting questions, I will ask your permission to cite here a letter which I received, more than 20 years ago, from Professor Faraday. I was at that time a youth at college, in the first ardor of my scientific studies, and I had devised—as most chemical tyros do, I believe— a fantastical theory of the nature of matter and of the forms and properties of its elementary particles, as also of the building up of these into compound molecules. No doubt I made my acid particles pointed and my sugary and oleaginous molecules round, taking care to provide my atoms with suitable facets whereby to fit one another in the construction of compounds, all which ordinary fluttering of my young chemical wings I must have deemed vastly profound and original, since I posted the paper, with high expectations, for the illustrious professor's judgment.

Two days afterwards I received a letter, on the corner of which I read, with a beating heart, the celebrated philosopher's signature. Being at present far away from my archives, I can but quote this letter from memory, yet I think I can give it to you nearly *verbatim*, so profound was the impression made on my mind by its modest simplicity, contrasted with my boyish presumption, and so lasting was the philosophic lesson it conveyed. It ran nearly thus:—

"I have received your ingenious speculations on the nature of matter, and on the forms and properties of atoms; and, in reply to your question, I have no hesitation in advising you to experiment in support of your views, because whether they be confirmed or confuted, good always comes of experiment.

"As to your views themselves, and my own opinion on the subject, I am fain to confess that I have thought long and closely on the theories of matter, and on the nature of its particles or atoms; and that the more I think, in association with experiment, the less distinct does my idea of an atom or particle of matter become."

I have never known which most to admire in this letter—its modest wisdom, or the condescending kindness which prompted its writer to bestow on an obscure youth—to him a total stranger—so gentle yet impressive a monition.

But why is it cited now and here?

Because its marking words—"*in association with experiment*"—are if I mistake not, particularly apposite to the present posture of chemical affairs.

The chemical world is, indeed, under invitation to embrace a new theoretic system, which incorporates, as an essential part of its fabric, the affirmation that several bodies, never yet decomposed, are compounds of a known with certain unknown elements; and, further, as regards the known elements themselves, that they are the results, some of one, others of two or more "*operations*," strangely likened to so many "strokes on a bell."

It would ill befit a mere student, like myself, of some of the simpler parts of chemistry, to criticise proposals brought forward by a recognised master of the science. Yet my admitted inferiority leaves me all the freer to confess that my dim conceptions of elementary matter derive no light from the suggestion that, in a "chemical operation," the evolution of hydrogen is attended, as it were, with *one* sound, and that of oxygen with *two*.

So, again, I may frankly avow that, to me, the proposed substitution of Greek for Roman letters, in our chemical symbols and formulæ, seems likely to impart to our hitherto attractive science, an aspect of abstruseness uncompensated by any *real* addition thereby made to the profundity of our knowledge.

Our real experimental acquaintance with the nature and constitution of hydrochloric acid, for example, remains unaltered, so far as I can see, whether we write it HCl, as of old, or denote it by the Greek symbol, $\alpha\chi$: and, for my own part, I feel the deep mystery of chlorine undiminished, when I am told, as the result of a series of equations, that

$$\chi = \frac{Cl}{H}$$

Still less, methinks, can the clearness of our physicochemical conceptions and reasonings be promoted by the two-fold interpretation which it is proposed to bestow on the algebraic signs + and —. These, it would seem, we are invited to employ, sometimes in their old accustomed acceptation, sometimes as symbols of differences in "direction;" which differences, an arrow-head turned this way or that, would, I think, much more fitly represent.

By an equally gratuitous confusion of things different, the ordinary collocations signifying in algebra multiplication and division, are to be henceforth employed by chemists, not in those familiar senses only, but also as signs of chemical combination and decomposition. For my part, I must confess that I cannot trace the faintest analogy between the multiplication of one number by another, and the combination of two chemical elements; though here confusion can scarcely be avoided, the algebraists having unadvisedly as I think, made the mere writing of two quantities in immediate succession the sign of their multiplication into each other. No such necessity can be pleaded to justify the use of the symbolic grouping $\frac{a}{b}$, heretofore set apart to express the division of one quantity by another, as the sign of so utterly dissimilar a conception as that of chemical decomposition, which we are, nevertheless, invited to write in much the same manner—putting $\frac{Cl}{H}$, for instance, to signify the decomposition of hydrochloric acid. If symbols of chemical combination and severance be needed, why not devise some simple forms of expression for the purpose; as, for example, marks of parenthesis, ordinary and reversed, the former for combination—(HCl), the latter for decomposition—H)(Cl. Surely this, or some equivalent expedient, would be more philosophical than the forcing into an artificial quasi-connexity, of facts and conceptions so utterly incongruous as to be absolutely incomparable.

I own myself at a loss to discover the additional profundity of reasoning assumed to be derivable from this ambiguous duplication of the meaning of symbols hitherto received, each only in a single sense. This mixing up of algebraic and chemical symbols, so far from assisting ratiocination in either kind, will rather tend, I think, to increase the toil of chemical thought and to multiply the chances of chemical error, by the additional stress gratuitously imposed on the chemist's attention, thus called on, as it will be, to catch correctly so many alternating modes of interpretation, each in its variable turn.

As for the new "fundamental definition" of a chemical phenomenon, it is "an operation on the unit of space, the result of which is a weight" I confess that, for me, this play of words throws not the faintest ray of light on any of the familiar marvels of chemistry, such as solution, effervescence, crystallisation, explosion, and all the numberless transformations of form, colour, combination, properties, etc., which collectively make up our astonishing science. In what sense is the above definition more luminous, more explanatory of the wonders amidst which we move, than if one should say, conversely, that a chemical phenomenon results from the operation of a weight (or weights) within given limits of space (and time). There is no limit to the multiplication of such abstractions, and if that now propounded gains acceptance, I cannot see why, to-morrow, a new Transcendental school may not be started, with a modified "fundamental definition," declaring a "chemical phenomenon" to be an "operation upon the unit of Time, the result of which is a Force."

Such expressions may be more or less neat and antithetic, but they afford us not the slightest insight into those hidden ways of nature which they affect to fathom and reveal. The *essence* of natural phenomena is, indeed, unknowable; we may study their relations, and ascertain the conditions of their existence, so as to obtain means for their modification and control, but when we seek, with subtle phrases, to express their absolute nature and causation, we do but disguise our ignorance and cheat our yearning curiosity with words. Of such kind appears to be the abstract definition of chemical phenomena now put forward to fit our science for the new analytical method proposed; and unless this apprehension should prove to be ill-founded, the new system will do little, I fear, towards promoting the real inductive progress of chemistry.

Far be it, however, from me to deprecate the introduction into chemical science of any high analytical method, if such there be, really compatible with its eminently concrete and experimental character, and really calculated to support the arduous reasoning of chemists through further-reaching concatenations than have heretofore been possible for them. But no such transcendental methods will avail, unless, as Faraday enjoins, they be used in constant "association with experiment," and due subjection to its wholesome check.

This rule would, I think, be as seriously infringed by the premature acceptance of chlorine, and a series of cognate bodies, as compounds of hydrogen with various unknown elements, as it would have been, if, in Sir Humphrey Davy's time, the suspected composite character of the alkalies had been elevated to the dignity of an established fact before its reality had been demonstrated by Davy's ever memorable experiments.

I would not, however, be understood as seeking, by any means, to undervalue or disparage the intellectual power displayed by the eminent propounder of the new doctrine, in his vigorous assault on the so-called "Chemistry of the Past," and equally cogent demonstration of the necessity for a higher and larger "Chemistry of the Future." In this proposition all chemists are, I think, agreed. It has long been felt by philosophical reasoners—Auguste Comte among the number—that the atomic theory fails to satisfy our intellectual requirements, when it is applied to the interpretation of *organic* chemistry, though it has answered its purpose admirably as a means of intelligibly connecting the main facts of *inorganic* chemistry. We, therefore, need a unitary conception, of more comprehensive scope, capable of embracing, under common forms the facts of both divisions of the science, and adapted to make manifest their intrinsic harmony, not only with each other, but also with the recent developments of physical science, such as, for instance, the lately proved convertibility of motion into force, and *vice versa*.

It is not, therefore, in any captious dislike of innovation, still less in a cavilling or disrespectful spirit, that I have ventured to submit what seem to me objections to the system now proposed. My attitude is rather that of a student who, after a perplexing lecture, lays his doubts, with deference, before his professor. I feel anxious lest chemical philosophy, at this juncture, should be led into a false, though brilliant path; lest, in striving after analytical methods, possibly unsuited to a science so essentially concrete and experimental, we should barter solid facts for metaphysical subtleties, and only, after all, make chemistry more abstract, at the cost of making it less real.

Like my chemical betters, however, I await, before finally judging the new system, its full and complete development; and I shall be found among its first and most grateful devotees, should it open up new spheres of really *positive* reasoning and research, and tend to facilitate (in Faraday's pregnant phrase) "THOUGHT ASSOCIATED WITH EXPERIMENT."

F. O. WARD.

VAPOUR DENSITY OF WATER.

To the Editor of the Chemical News.

SIR,—I have read with much interest the five communications called forth by some remarks on the vapour density of water, extracted by you from a letter of mine to a private friend.

It is curious to note the little discrepancies which occur in the computations of so many able and learned men applying their minds to the same subject.

On one point the five letters supply these different figures:—

·80424 ·80475 ·8056 ·8064,

of which the last-named has the support of the majority, and is the figure at which I had previously arrived.

On another point I gather these figures:—

1239·9 1240·6 1241·3 1242·6 1243·4—

my own figure, independently calculated, being 1240.

On the question of the true gas-expansion ratio, touched by one of your correspondents, and lying at the very root of the matter, I happen to have a paper at hand, giving the results arrived at by several eminent authorities, and showing notable discrepancies between them, even as to this fundamental *datum*. These ratios are as folows, for the volume at zero F.:—

Dalton and Gay Lussac	$\frac{1}{448}$
Regnault	$\frac{1}{458}$
Magnus	$\frac{1}{459}$
Rudberg	$\frac{1}{462}$

Correcting these ratios for the volume at freezing-point, they become:—

Dalton and Gay Lussac	$\frac{1}{480}$
Regnault	$\frac{1}{490}$
Magnus	$\frac{1}{491}$
Rudberg	$\frac{1}{494}$

Applying the first and last of these ratios to the question in hand, and taking $\frac{1000}{\cdot 8064} = 1240$ as the standard for comparison (my own computation whereof is sanctioned by the majority of your correspondents), we have, according to Dalton and Gay Lussac—

$$\frac{480}{660} \times 1696 = 1233{\cdot}45$$

and, according to Rudberg:—

$$\frac{494}{674} \times 1696 = 1243{\cdot}06$$

On comparing each of these results with our above-mentioned standard, deduced from the volumetric composition of water, we obtain, as the respective discrepancies, *the values* 6·55 and 3·06; the former being *below* and the latter *above* the standard; so that the discrepancy between the two results themselves is the sum of the amounts above stated, viz., $6{\cdot}55 + 3{\cdot}06 = 9{\cdot}61$.

Neither of these discrepancies, however, is so large as that worked out by me in my last letter—into which, I must own, that error crept, through my using (in the absence of my books of reference) an imperfectly remembered rule of computation.

If lastly, we work out the calculation as before, substituting Regnault's ratio, we obtain

$$\frac{490}{670} \times 1696 = 1240{\cdot}3582$$

which differs from the standard by only

0·3582.

The question therefore turns, so far as the greater or less discrepancy is concerned, upon the relative merits of the respective determinations, by the eminent authorities quoted, of the expansion ratio for gases: as also, upon the more or less thorough soundness of the laws laid down as governing the expansion of gases, one or two of which laws, if I remember rightly, rest partly on assumption.

Meanwhile, though we are free to regard the discrepancy under consideration as larger or smaller, according to the expansion ratio on which we elect to rely, science as yet affords no absolutely certain proof, entitling any one to deny its existence.

The observed and calculated vapour-densities of many other bodies, besides water, show also (unless my memory betrays me) fractional differences more or less important, the very smallest of which may have a real existence, and may exercise more influence than is yet suspected on the play of chemical phenomena. This was the special consideration I addressed to my friend—supporting it by an example drawn from water, and an analogy supplied by music. In the carelessness of intimate pen-talk, I inadvertently made out the discrepancy cited too great; and this, not only as regards the fact itself, but also as regards the fitness of the illustration to my argument. For I was really referring to minute, not considerable differences: and my example is rendered all the more apposite by the correction now supplied.

The want of symmetry established, by Stas, in the ponderal relations of matter, lends colour, I think, to the hypothesis, that a like condition may attend its cognate volumetric proportionalities: for, that these relations should be all absolutely integral, while so many of the others are fractional, seems to me a somewhat hazardous assumption.

In these musings, however, I carefully avoid affirmation; I merely ponder and surmise. Such unripe germs of thought are fitter, no doubt, for the commerce of friends than for the wider public audience, which accident has, in this instance, afforded to mine. If your favour, Sir, have, as I fear, given them more prominence than they deserve, I can only hope that, by stimulating thought, or, still better, by suggesting experiment, they may pay for the time and space thay have occupied.

To your able correspondents, whose courteous indications have afforded me much light, I beg, in conclusion, to offer my best acknowledgements, and I remain, etc.,

F. O. WARD.

North Wales, July 20th, 1867.

CHEMICAL NOTICES FROM FOREIGN SOURCES.

Tungsten, properties and compounds of (O=8).—E. Zettnor. The best method of preparing pure compounds of tungsten, from the mineral, is to fuse the latter with $\frac{1}{3}$ of its weight of sodic carbonate. The fused mass is extracted with boiling water, which, on evaporation, yields crystals of the neutral sodic tungstate, $NaO,WO_3 + 2$ aq. The mother-liquor, acidulated with nitric acid and evaporated, gives crystals of the acid salt, $NaO, 7WO_3 + 16$ aq.

The quantitative determination of tungstic acid may be effected by adding a standard solution of plumbic acetate to an aqueous solution of tungstic acid, acidulated with acetic acid. The results are very accurate.

The atomic weight of tungsten has been deduced from the composition of ferrous and argentic tungstate; the first gave the number 92·038, the latter 91·927. Mean=91 976 or 92.

Metallic tungsten is obtained as a dark grey powder, by melting tungstic acid together with sodium, under a cover of sodic chloride, or by passing a strong electric current through fused sodic tungstate.—(*Pogg. Ann.* cxxx. 16.)

Lead-Chamber Process.—R. Weber. Sulphurous acid, in presence of much water, reduces binoxide of nitrogen to oxide of nitrogen. Sulphuric acid, of a certain strength, prevents this reaction. This explains the loss of nitric acid in the manufacture of sulphuric acid, which always takes place when the sulphuric acid in the lead chamber is below the normal strength.—(*Pogg. Ann.* cxxx. 277.)

Fluorides of Antimony and Arsenic.—Marignac.

Fluoride of antimony forms an amorphous, gum-like mass, when evaporated in vacuo. Its concentrated solution cannot be heated without decomposition. If potassic or sodic hydrate, or ammonia, be added to a very concentrated solution of the fluoride, double fluorides in crystals may be obtained. They are nearly all deliquescent, and their solution is not immediately precipitated by sulphuretted hydrogen, acids, or alkalies. They are stable in the crystalline state, but decompose in solution, forming oxy-fluorides.

The corresponding fluo-arseniates are still more difficult to obtain in crystals. Sulphuretted hydrogen decomposes their solution slowly.—(*Arch. Science*, 1867.)

Dissociation.—H. Debray finds that the phenomenon of dissociation may be observed in solid bodies which are formed by direct combination of a volatile and a non-volatile body.

Pure Iceland spar was heated in a tube, which could be exhausted, or filled with any gas by means of a Geisler's mercury air-pump. At 350°C. the decomposition of the mineral was zero; at 440° scarcely preceptible, at 860° very distinct, but ceased when the pressure in the tube amounted to 85 mm. Heated to 1070° the pressure became stationary at 520 mm. This "tension of dissociation" is constant at a given temperature, increases with the rise of temperature, and is independent of the extent of decomposition the carbonate has undergone. If in these experiments the apparatus, after having been heated, is allowed to cool, the generated carbonic acid is entirely reabsorbed, and a vacuum again produced. The author further finds that caustic lime, at ordinary temperatures, does not obsorb a trace of carbonic acid, if the latter be perfectly dry; the absorption begins at a dark red-heat.—(*Comptes R.* lxiv. 603.)

Hydriodic Acid, action of heat on.—T. Hautefeuille. Hydriodic acid, if gradually heated, begins to decompose at 180°C, the violet colour of the gas increasing slowly up to 440°; but from 440° to 700° the decomposition proceeds rapidly. The amount of decomposed gas varies with the extent of surface. At a given temperature it is greatly increased by spongy platinum, but the latter also causes hydrogen and iodine to combine. If, at a certain temperature, equal volumes of H and I pass over platinum in the finely divided state, the quantity of the gases remaining uncombined is exactly equal to that which would be formed by decomposition, if hydriodic acid had been used.—(*Comptes R.* lxiv. 608.)

Ethers of the Acids of Arsenic (C=12).—T. M. Crafts. The reaction of silicic ethide on boric acid, studied by Friedel and Crafts, in which ethylic borate is formed, has been tried by the latter chemist with the acids of arsenic, but only arsenious acid behaves like boric acid. The action of dry arsenic acid upon silicic ethide takes place under pressure at 220°—230°C., silicic acid is precipitated, and a gas escapes which seems to be ethylene. On distillation, arsenious ethide is obtained. The residue consists of arsenious acid, silicic acid, and some arsenic acid.

Arsenic ethide, $3\ (C_2H_5)\ AsO_4$, is formed by heating ethylic iodide, diluted with twice its volume of ordinary ether, and argentic arseniate, slightly in excess, together in sealed tubes to 120°C. The contents of the tube are extracted with ether, the ether evaporated in a current of carbonic acid, and the remaining liquid distilled under diminished pressure. Under ordinary pressure, arsenic ethide boils at 235°—238°, but decomposes partially. Its sp. gr. at 0°C. is =1·3264. It mixes with water in every proportion, showing then all the reactions of arsenic acid.

Arsenious ethide $3(C_2H_5)AsO_3$, is produced on heating silicic ethide with arsenious acid to 220° under pressure. It boils at 166°—168°. Its vapour density was found ranging from 7·197 to 7·615, according to the temperature at which it was taken; theory requires 7·267. Its sp gr. at 0°C., is =1·224. Water decomposes it.

The ether may also be obtained from argentic arsenite with ethylic iodide.—(*Comptes R.* lxiv. 700.)

Aromatic Hydrocarbons converted into Phenols (C=12).—A. Wurtz. Sulphobenzolic acid and its analogues, melted with potassic hydrate (250°—300°C.), splits up into sulphurous acid and the respective phenol. The phenols thus obtained are very pure. The reaction takes places according to the following equation:

$$SO_2\left\{\begin{matrix}C_6H_5\\OK\end{matrix}\right. + KHO = SO_2\left\{\begin{matrix}K\\OK\end{matrix}\right. + C_6H_5OH.$$

—*Comptes R.* lxiv. 749.

Crystallisable Sugar in Helianthus Tuberosus.—Dubrunfaut. The juice of the tubers of the Jerusalem artichoke, if gathered in September, contain inulin in large quantities; if reaped in March or April, it is found to contain dextrose and a non-crystallisable, inactive sugar instead. These two saccharine matters are, no doubt, produced by the conversion of the inulin, which is formed during the first period of vegetation.—*Comptes R.* lxiv. 764.)

Phenol, derivatives of.—H. Brunk has prepared a series of substitution-compounds of phenol, especially those containing nitryl and bromine together. Their salts crystallise well. He also describes the methylic ethers of the two isomeric nitrophenols, which he finds to be identical with mononitranisol and isonitranisol. The first is an oily liquid, the latter a crystallisable body. Both exchange their nitryl for amide on reduction with tin and chlorhydric acid. The monamidophenyl-methylic ether (anisidin) is a liquid, the isamidophenol-methylic ether (isanisidin), a solid.—(*Zeitsch. Chem. N. F.* iii. 202.)

Phenol-group, Contributions to the history of.—L. Dussart. ($Ꞓ=12, Θ=16$). Naphthalene, on being heated with sulphuric acid (10 parts of the former to 25 of the latter), is completely converted into sulpho-naphthalic acid, which, on continued heating, changes into disulpho-naphthalic acid. If pure naphthalene has been taken, little sulphurous acid is formed. The salts of disulpho-naphthalic acid are decomposed by fusing potassic hydrate under formation of a new body, the analysis of which led to the formula of the diatomic phenol $Ꞓ_{10}H_8Θ_2$. This body is more soluble in water than in naphthol; it dissolves readily in caustic potash, which solution is instantaneously decomposed on exposure to air.—(*Comptes R.* lxiv. 859.)

Benzoin, derivatives of ($Ꞓ=12, Θ=16$).—M. Zinin. If benzoin is heated with fuming chlorhydric acid to 130°C. under pressure, an oily body is formed, which on cooling solidifies to a crystalline mass. Of this mass about 72 per cent. are soluble in ether or alcohol, and this solution, on evaporation, yields crystals of benzil, and a thick yellow oil, insoluble in water, easily soluble in alcohol and ether. The portion insoluble in ether is a new compound which the author calls lepiden; its composition is $Ꞓ_{28}H_{20}Θ$. It is insoluble in water, readily soluble in benzol. It melts at 175°, and evaporates at 220°. Potassic hydrate, either solid or in alcoholic solution, does not act upon lepiden. Nitric acid and chromic acid convert it into crystalline oxy-lepiden $Ꞓ_{28}H_{20}Θ_2$, which is insoluble in water, nearly so in ether, readily soluble in benzol. Its melting point is 220°. Zinc and acetic acid reduce it again to lepiden. A bromine substitution compound of lepiden, $Ꞓ_{28}H_{18}Br_2Θ$, has been obtained by adding bromine to its solution in acetic acid.—(*Acad. Petersb.* xi. 151.)

Platinic and Auric Chlorides, Combinations of.—Weber. (O=8.) The yellow crystals which are obtained on adding fuming nitric acid to a solution of platinic chloride have the composition $PtCl + NO_2Cl + HO$. They dissolve in water with disengagement of NO_2. From a solution of platinic chloride in chlorhydric acid (free from nitric), red brown crystals separate on evaporation under the desiccator, which have the composition of Marignac's double chloride of platinum and sodium, but containing H in place of Na.,—$PtCl_2 + ClH + 6HO$. A similar compound has been obtained with auric chloride $AuCl_3 + HCl + 6HO$.—(*Acad. Berl.*, Feb., 1867.)

Action of Chloride of Sulphur on Metals and Sulphides.—(S=16).—E. Baudrimont has studied the action of sulphurous chloride (S_2Cl) on various metals and sulphides. The reaction in which metallic chloride is formed, and sulphur precipitated, takes place most readily with those metals, the chlorides of which are volatile, but is prevented altogether in the case of magnesium and sodium.—(*Comptes R.* lxiv. 368.)

Benzylic bromide, and bromtoluol.—F. Beilstein. The action of bromine on toluol is analogous to that of chlorine on toluol. Benzylic bromide (not free from bromtoluol) is obtained when vapours of bromine are made to act upon boiling toluol, and bromtoluol (free from benzylic bromide), when bromine in presence of sodium acts on toluol, either cold or hot.—(*Zeitschr. Chem.* N.F., iii. 281.)

Phenylene brown.—Caro and Griess (Θ=12). Experiments of Hofmann, Martius, and Holle, have shown that *β* phenylene-diamine on being acted upon by nitrous acid is decomposed with formation of a brown substance, which under favourable conditions may be obtained as a crystalline body of basic nature. If to a cold, dilute, neutral solution of phenylene-diaminic chlorhydrate is added a neutral solution of a nitrite, a crystalline dark-red mass is precipitated. Washed with water and treated with strong chlorhydric acid, it first dissolves and then the chlorhydrate of the new base separates as a tarry liquor. The latter decomposed in aqueous solution by ammonia, yields the dye in a crystalline state. The phenylene-brown consists chiefly of a new base of the composition $\Theta_{12}H_{13}N_5$, and is derived from *β* phenylene-diamine according to the equation:

$$2\Theta_6H_8N_2 + NH\Theta_2 = \Theta_{12}H_{13}N_5 + 2H_2\Theta.$$

Its constitution is considered to be $\Theta_{12}H_7(NH_2)_3N_2$, that is, triamidoazobenzol.—(*Zeistchr. Chem.* N.F., iii. 278.)

Crystalline Chromic Oxide.—R. Otto has obtained chromic oxide in the crystalline state, by passing a current of hydrogen over potassic bichromate. The salt is reduced to potassic chromate, which serves as the medium in which the oxide crystallises.—(*Ann. Chem. Pharm.*, cxlii. 102.)

Ethyl-pyrophosphoric Acid.—G. Dilling (C=6,O=8). Phosphoric anhydride at ordinary temperature does not act upon zincic ethide; heated together in sealed tubes to 140°C. zincic ethyl-pyrophosphate is formed, besides other products. The basic salt of this acid has the composition,

$$2BaO\left\{\begin{matrix}C_4H_5\\C_4H_5\end{matrix}\right\}P_2O_6,\ O_2$$

Ethyl-pyrophosphoric acid may be considered as a derivative of pyrophosphoric acid in which one or two atoms of oxygen are replaced by ethyl.—(*Zeitschr. Chem.*, N.F. iii. 266.)

Creosote.—E. Probst, in a preliminary communication, states that he has obtained pyrocatechin from beech-wood-creosote by melting the latter with an excess of potassic hydrate. He reserves to himself a more complete investigation of this subject.—(*Zeitschr. Chem.*, N.F. iii. 280.)

Gallic, Pyrogallic, and Oxyphenic Acids, Bromo-derivatives of.—H. Hlasiwetz (Θ=12,Θ=16). These three acids combine readily with bromine, forming the following substitution-compounds:—Bromogallic acid, $\Theta_7H_4Br_2\Theta_5$, readily soluble in hot water, in which solution ferric chloride produces a brilliant violet, ammonia a red coloration. Bromopyrogallic acid, $\Theta_6H_3Br_3\Theta_3$, is somewhat less soluble in water, but gives the same reactions as the former. Bromoxyphenic acid, $\Theta_6H_2Br_4\Theta_2$, is insoluble in water, readily soluble in dilute alcohol; its solution assumes a dark blue colour on addition of ferric chloride.—(*Acad. Wien.*, 55, 1867.)

Titanic Iodide.—P. Hautefeuille. This compound is readily obtained by acting upon titanic chloride with hydriodic acid. When purified by repeated sublimation in a current of hydrogen it forms a brittle red-brown mass, which fuses at 150°C., and crystallises on cooling. It boils above 360°, and its vapour density, taken at 440°, was found =18·054; calculated for 2 volumes, it should be 19·334. It is soluble in water, but the solution soon decomposes with precipitation of titanic acid.—(*Bull. Soc. Chim.*, vii. 201.)

Chloric Acid, determination of.—C. Stelling (O=8). Potassic chlorate is reduced to chloride on being treated with ferrous oxide, according to the equation, $KO,ClO_5 + 12FeO = KCl + 6Fe_2O_3$. This suggested the following method for the determination of chloric acid:—To a solution of the chlorate is added ferrous sulphate and an excess of potassic hydrate; the mixture is boiled and filtered, and in the filtrate the chlorine determined in the usual manner.—(*Zeitschr. Analyt. Chem.*, vi. 32.)

Cobalt and Nickel, Atomic Weights of.—Cl. Winkler has determined the atomic weight of these metals according to the following method:—The metals were prepared in a state of perfect purity; the cobalt by reduction of repeatedly recrystallised purpureo-cobaltic chloride in a current of hydrogen at a high temperature. The nickel by adding to a solution of the commercial carbonate in chlorhydric acid sodic hypochlorite, and treating the liquid in this manner again so long as any cobalt could be detected in it; the solution was then purified from traces of copper and arsenic and precipitated with sodic carbonate. The carbonate was converted into chloride, and sublimed in a current of chlorine, and lastly reduced in a current of hydrogen.

Weighed quantities of the metals were then immersed in a perfectly neutral, concentrated, cold solution of sodio-auric chloride, and the weight of the precipitated gold determined. The mean of five experiments with cobalt gave the number 29·496. The mean of four with nickel, the number 29·527.

The atomic weights of these metals may therefore be taken as identical, *i.e.*, 29·5.—(*Zeit. Analyt. Ch.*, vi. 18.)

Succinic Acid, constitution of.—H. Wichelhaus (Θ=12,Θ=16). While succinic acid, in accordance with its formation from bicyanethylene, has generally been viewed as bicarbethylenic acid,

$$\begin{array}{l}\Theta H_2.\Theta\Theta.\Theta H\\ |\\ \Theta H_2.\Theta\Theta.\Theta H\end{array}$$

it has been represented recently by Claus (*Ann. Ch. Pharm.* cxli., 49) as bicarbethylidenic acid,

$$\begin{array}{l}\Theta H_3\\ |\\ \Theta H(\Theta\Theta.\Theta H)_2\end{array}$$

As a necessary support to the latter view, the acid derived by H. Müller from cyanpropionic acid (which was prepared from chlorpropionic ethide) should have been identical with succinic acid. This, however, is not the case; they differ in melting point, solubility, and in their behaviour towards ferric chloride. On the other hand, an acid has been obtained from *β* chlorpropionic acid (derived from glyceric acid) by the same reaction, that is, treatment with potassic cyanide and decomposition with potassic hydrate, which has the same properties as succinic acid. It would appear, therefore, that *β* chlorpropionic acid is the starting point for the synthesis of succinic acid, and that the latter must still be considered as bicarbethylenic acid.—(*Zeitschr. Chem.*, N.F. iii., 247.)

Tannic Acid.—H. Hlasiwetz (Θ=12, Θ=16). Caffetannic acid, when treated with potassic hydrate, is successively converted into Caffeic acid, sugar, and protocatechuic acid. Caffeic acid

$$\Theta_9H_8\Theta_4\ \tfrac{1}{2}H_2\Theta$$

is obtained by boiling caffetannic acid with a solution of potassic hydrate of 1.25, sp. gr. ¾ of an hour. It is then precipitated with sulphuric acid, recrystallised from water, and decolorised with animal charcoal. It is soluble in alcohol; ferric chloride produces in its aqueous solution a green colour, which turns dark red on addition of soda; it is precipitated by plumbic acetate, and converted into oxalic acid by nitric acid. Fused with potassic hydrate, it breaks up into protocatechuic and acetic acid, and dry distillation converts it into pyrocatechin. Treated with sodium amalgam it takes up 2H forming hydrocaffeic acid. Caffeic acid is triatomic, and is the third in the following series:

Cinnamic acid.....$=\text{Ꞓ}_9H_7\Theta.\Theta H$
Coumaric acid.....$=\text{Ꞓ}_9H_6\Theta 2\Theta H$
Caffeic acid.......$=\text{Ꞓ}_9H_5\Theta 3\Theta H$

which, on being treated with fusing potassic hydrate, are converted, with formation of acetic acid, into the series:

Benzoic acid.......$=\text{Ꞓ}_7H_5\Theta.\Theta H$
Salycylic acid......$=\text{Ꞓ}_7H_4\Theta.2\Theta H$
Protocatechuic acid $=\text{Ꞓ}_7H_3\Theta.3\Theta H$

The solution from which the caffeic acid has crystallized contains sugar; it is neutralised with potassic hydrate, evaporated and extracted with alcohol. After purification the sugar appears as a syrupy, non-crystallisable mass, showing the usual reactions. An analysis gave the formula, $\text{Ꞓ}_6H_{10}\Theta_4$.

Caffetannic acid, by its splitting up into caffeic acid and sugar, is thus proved to be a glucosid.—(*Akad. Wein.* lv. 1867.

Ammoniacal Platinum Compounds.—T. F. Cleve has given an account of his researches on the platinum-bases, comprising a critical examination fo those already known, and a description of many new derivatives of these interesting compounds. For detail, we must refer to the original paper.—(*Bull. Soc. Chim.* vii. 12; or, more complete, *Acta Societ. Scient. d' Uphala*, 1866.)

Tea, constituents of.—H. Hlasiwetz and Malin. ($\text{Ꞓ}=12, \Theta=16$). Rochleder states that tannic acid and boheic acid may be obtained from a decoction of tea by precipitation, first with neutral plumbic acetate, and the filtrate therefrom with basic or ammoniacal plumbic acetate. The authors, however, find that both precipitates alike contain tannic, gallic, and oxalic acid; the latter, besides, a small quantity of quercetin,

$$\text{Ꞓ}_{27}H_{18}\Theta_{12},$$

which imparts to it a yellow colour. If the lead precipitate is decomposed with sulphuretted hydrogen, and the filtrate boiled with dilute sulphuric acid, much more of this body is obtained; from which it appears that the original solution does not contain quercetin in the free state, but probably as quercitrin.—(*Akad. Wien.* lv., 1867.)

Aromatic Aldehydes under the Influence of Dehydrating agents.—V. Longuinine. ($\text{Ꞓ}=12$, $\Theta=16$.) Cuminol ($\text{Ꞓ}_{10}H_{12}\Theta$.) is powerfully acted upon by phosphoric anhydride, being converted by it into a resinous mass. Zincic chloride is without action in the cold, and almost so at the temperature of the water-bath, but if employed in the fused state, a violent reaction takes place, which results in the formation of Cymol ($\text{Ꞓ}_{12}H_{14}$). The author believes that first the hydrocarbon $\text{Ꞓ}_{10}H_{10}$ is formed, which subsequently combines with hydrogen, resulting from the complete decomposition of a portion of the aldehyde.—(*Comptes R.* lxiv. 785.)

Argentic Iodide.—H. Fizeau finds that amorphous argentic iodide (prepared by precipitation), like the crystallised and fused modification (Chemical News, No. 386), shows the remarkable phenomenon of contraction with rising, and expansion with falling temperature.—(*Comptes R.* lxiv. 772.)

Iodine-starch.—H. Pellet. From experiments made as to the cause of the decolorisation of iodine-starch on heating, and reappearance of the colour on cooling, the author arrives at the following conclusions:—1, Decolorisation is caused by the solution of iodine-starch in an excess of hot starch; the solubility being less in the cold, the colour reappears again on cooling. 2. Iodine-starch is decomposed at 100°C. and iodine volatilises. 3. Iodine-starch remains unchanged in alcohol, being equally insoluble in that liquor whether hot or cold. 4. Iodine-starch may be regarded as a salt, which in certain solvents is more readily soluble when hot, than when they are cold.—(*Bull. Soc. Chim.* vii. 147.)

Ozonometry.—A. Cossa is engaged with experiments, the object of which is to discover an exact method for the determination of ozone in atmospheric air. The author has at present established the following facts:—1. A solution of pure potassic hydrate (free from every trace of organic matter) absorbs nitro-oxygen compounds without destroying ozone. 2. The quantity of iodine liberated in a solution of pure potassic iodide is in exact proportion to the amount of ozone that has passed through the liquid, and may be accurately measured by means of Bunsen's volumetric liquid. —(*Zeit. Analyt. Ch.* vi. 24.)

Quinine, Testing of.—Stoddart recommends the following methods for testing quinine for quinidine, etc.:—6 grammes of the suspected quinine are dissolved in a test tube in 5 grammes sulphuric acid, diluted with 3 grammes water; to this is added 7·5 grammes ether, 18 grammes alcohol, and 2 grammes of a solution of sodic hydrate containing about 8 per cent. The mixture is well shaken and left to itself for 12 hours. If quinidine, cinchonine, or cinchonidine are present, they will be found in a layer below the ether—quinidine as an oily liquid, cinchonidine in crystals.

The second method consists of a microscopic examination of the crystalline precipitate produced in a saturated and neutral solution of quininic sulphate by potassic sulphocyanide. (10 gr. in 45 gr. water.)—(*Journ. Pharm. Chim.* iv. 50.)

Ferric Chloride, Volatility of.—R. Fresenius. The author has made a few experiments in order to decide whether any loss is likely to occur in analytical operations through iron being volatilised during evaporation of its solution with an excess of chlorhydric acid. In one experiment he evaporated ferric chloride with chlorhydric acid nearly to dryness; in a second, the evaporation was performed in presence of an alkaline chloride, and the mass afterwards kept on the water-bath for twelve hours; in a third, ferric chloride, with much chlorhydric acid, was kept boiling for 1½ hours. In neither case did any loss of iron take place.—(*Zeitschr. Analyt. Chem.* vi. 92.)

Absorption of Carbonic Acid by Oxides.—J. Kolb finds that the anhydrous and monohydrated oxides of potassium, sodium, magnesium, and barium, like calcic oxide, do not increase in weight when exposed to dry carbonic anhydride, but absorption takes place if the latter is charged with moisture.—(*Comptes R.* lxiv. 861.)

Ozone, Density of.—T. L. Soret. Having previously found that the density of ozone is 1½ times as great as oxygen, the author now applies the method of diffusion as a control of his former observations.

Under similar conditions, in one case, a known mixture of chlorine and oxygen, in another a known mixture of ozone and oxygen, were diffused into pure oxygen. The velocity with which chlorine and ozone passed through the diaphragm was in the ratio:—

$$\frac{\text{Chlorine}\ldots\ldots227}{\text{Ozone}\ldots\ldots\ldots271}=\cdot 8382$$

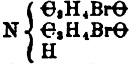
This value approaches closely that of the inversed proportions of the square-roots of their respective densities, taking that of ozone as 1½ that of oxygen, *i.e.* $=\cdot 8243$.—(*Comptes. R.* lxiv. 904.)

Pseudo-hexylurea.—T. T. Chydenius. This compound is prepared by heating pseudo-hexylic iodide (obtained from mannite) with argentic cyanate to 50°—60°C. The reaction is violent, and a liquid strongly affecting the eyes, and possessing a very disagreeable odour, distils over. An excess of ammonia is added, and the solid mass, thus formed, recrystallised from water. Pseudo-hexylurea crystallises in white needles which are readily soluble in hot water, ether, and alcohol, melt at 127°C., and boil, with partial decomposition, at 220°. Its formula is:

$$N_2\left\{\begin{array}{l}\text{Ꞓ}\Theta\\(\text{Ꞓ}_6H_{12}H,)H\\H_2\end{array}\right.$$

When heated with a strong aqueous solution of potassic hydrate in sealed tubes, decomposition sets in at 230°—250°, ammonia being given off, and an oily liquid formed, which probably is isopropylamine.—(*Comptes. R.* lxiv. 975.)

Nitriles, Action on Bromine of.—C. Engler. Propionitrile and bromine combine when heated together, forming bromhydrate of propionitrile, $N\text{Ꞓ}_3H_4Br_2HBr_2$. The body is very deliquescent, fuses at 64°C., and begins to sublime, with partial decomposition, at 72°. It will be seen that bromine in this case acts differently from chlorine, the latter producing, as is well known, dichlorpropionitrile. $N\text{Ꞓ}_3H_3Cl_2$. Water decomposes the bromhydrate to bromammonium, bromhydric acid, and a new body which is dimonobrompropionamid.



$$N\begin{cases}\text{Ꞓ}_3H_4Br\Theta\\ \text{Ꞓ}_3H_4Br\Theta\\ H\end{cases}$$

This latter compound is obtained in white crystals which are soluble in alcohol and ether, almost insoluble in cold water, fuse at 148°C., and decompose at 152°. Aqueous potassic hydrate decomposes the amide with formation of a new acid, the argentic salt of which has the composition $\text{Ꞓ}_3H_5Ag\Theta_3$. Dimonobrombutyramide, $N(\text{Ꞓ}_4H_6Br\Theta)_2H$, is prepared in the same way as the propyle-compound, which it resembles closely.—(*Ann. Chem. Pharm.* cxlii. 65.)

Trichlorhydrin, Action of Ammonia on.—C. Engler. Dimonochlorallylamine

$$N\begin{cases}\text{Ꞓ}_3H_4Cl\\ \text{Ꞓ}_3H_4Cl\\ H\end{cases}$$

is formed by digesting trichlorhydrin with alcoholic ammonia under pressure at 130°—140°C. It is a heavy oil, sparingly soluble in water, soluble in alcohol and ether; it decomposes partially on distillation, the portion analysed went over between 185° and 195°. The salts of this base are deliquescent, and difficult to obtain in crystals from an aqueous solution. The platino-chloride is readily soluble in water, moderately so in alcohol, and nearly insoluble in ether. It has the composition $N(\text{Ꞓ}_3H_4Cl)2H_2Cl,PtCl_2$.

An ethyl-substitution-compound of dimonochlorallylamine is obtained by heating the latter with ethylic iodide in sealed tubes to 100°C. Its composition is

$$N\begin{cases}\text{Ꞓ}_3H_4Cl\\ \text{Ꞓ}_3H_4Cl\\ \text{Ꞓ}_2H_5\end{cases}$$

It differs but little from the original compound.—(*Ann. Chem. Pharm.* cxii. 77.)

Analysis of Organic Compounds, New Method of.—A. Mitscherlich. Of this new method of analysis, which is applicable to all organic, and many inorganic compounds, and which includes the direct determination of oxygen, we can only indicate the principle, as it would be impossible in these few lines to give, without pictorial illustrations, an intelligible description of the complicated apparatus and manipulations required.

1. Oxygen and hydrogen are determined together by heating the substance in a current of chlorine, passing the products of combustion over red-hot charcoal, and absorbing the chlorhydric acid, carbonic anhydride, and carbonic oxide formed by a saturated solution of plumbic nitrate, a solution of potassic hydrate, and a solution of cuprous chloride in chlorhydric acid respectively.

2. Chlorine, bromine, iodine, and sulphur are determined simultaneously with carbon and nitrogen, by volatilising the substance in a current of hydrogen, burning the mixed gases and vapours with oxygen, removing the water by means of sulphuric acid, and collecting the products of combustion in weighed vessels—with the exception of nitrogen, which is measured. The products of combustion, besides the water, may consist of carbonic anhydride, chlorhydric acid, bromine, iodine, sulphurous acid, sulphuric acid, and traces of brom- and chlorhydric acid. A residue of carbon may also be left in the combustion tube. The water is absorbed by sulphuric acid, the chlorhydric acid by plumbic nitrate, bromine by mercuric oxide, iodine is weighed as such, sulphurous acid is absorbed by potassic bichromate, carbonic anhydride by potassic hydrate, nitrogen is measured, and the residual carbon weighed.

The specimens of analysis given, prove that very accurate results may be obtained by this new method.—(*Pogg. Ann.* cxxx. 536.)

Qualitative Analysis without using Sulphuretted Hydrogen and Ammonic Sulphide.—E. Zettnow. The author tests an aqueous solution, which may contain all of the more common bases, with the following reagents in succession:—

1. Chlorhydric acid precipitates lead, mercury, and silver.
2. Sulphuric acid precipitates lead, barium, strontium, and calcium.
3. Baric hydrate sets free ammonia, the filtrate is used for the detection of sodium and potassium.
4. Zinc is added to the filtrate from 2, the hydrogen ignited and tested for antimony and arsenic; antimony, arsenic, tin, mercury, copper, cadmium, and bismuth are precipitated.
5. Baric carbonate precipitates from the filtrate from 4, iron, chromium, and aluminium.
6. Ammonic carbonate, after removal of the baryta, precipitates from the filtrate of 5, manganese and calcium; the filtrate is tested for magnesium, cobalt, and nickel.
7. Zinc is tested for in the original solution.—(*Pogg. Ann.* cxxx. 324.)

Reagent for Nitric Acid.—C. D. Braun. A very delicate test for nitric acid is, according to C. D. Braun, anilinic sulphate. Half a c.c. of a solution of the aniline-salt is added to one c.c. of pure, strong sulphuric acid, contained in a watch-glass. A glass rod is then moistened with the liquid to be tested, and moved about in the aniline mixture, when a red coloration indicates the presence of nitric acid.—(*Zeit. Analyt. Ch.* vi. 71.)

MISCELLANEOUS.

The Representation of the University of London.—We have been requested to state that amongst the names of those who have been proposed to represent this University in Parliament that of Sir John Lubbock, Bart., F.R.S., has met with many warm advocates. There is no doubt that Sir John Lubbock possesses unusual qualifications for such a task; for, while his intellectual and scientific eminence would give weight to his words on questions of science, of education, and civil polity, his position in the City of London, and his reputation as a man of business, would obtain for him a hearing that might be denied to a man occupied exclusively in scientific pursuits. For the same reason he is peculiarly fitted to be the spokesman in the House of Commons of the large and increasing body of scientific men—a class whose opinions have hitherto found very inadequate expression in Parliament. We have heard the name of Dr. W. Allen Miller, F.R.S., proposed as that of a gentleman also fitted to represent this University, and should Dr. Miller be induced to come forward as a candidate, there is little doubt that he will obtain the unanimous support of the chemical fraternity; but in the event of this rumour turning out incorrect, chemists could not have a better representative than Sir John Lubbock, supported as he is by such well known men as Dr. A. Crum Brown, G. C. Foster, Dr. Odling, Dr. Pye-Smith, and H. Watts.

Suspension of the Storm Warnings.—We have more than once expressed our regret that the scientific committee, which has been appointed by the President and Council of the Royal Society, has advised the discontinuance of the Storm Warnings of the late Admiral Fitzroy. At the last meeting of the Literary and Philosophical Society, of Manchester, the subject was again brought forward by Mr. Baxendell, who strongly urged that the Board of Trade would again take the management of the Meteorological Department into their own hands, and appoint Mr. Babington, or some other competent and responsible person, to resume and carry on the system of storm warnings. As an illustration of the lamentable consequences of the suspension of storm signals, it may be stated that among the wrecks caused by the unusually heavy gale of the 10th of April, there were four vessels, which had sailed from Liverpool just before the commencement of the storm. Three of these vessels had been thrown on the Lancashire coast, and totally lost, and the lives of three of the crew of one vessel had been sacrificed. The fourth vessel had foundered in the Irish Sea, and all hands were lost. Now the meteorological phenomena immediately preceding the occurrence of this storm were of such a nature that there could have been no difficulty in giving notice of its approach in ample time to prevent the sailing of these vessels. In fact, without the aid of telegrams from distant stations, its approach was confidently announced in Manchester, so early as the evening of Monday, the 8th of April, and the direction in which the wind would blow during the most violent period of the storm was also stated at the same time.

The Paris Exhibition.—Mr. Sterry Hunt, F.R.S., the celebrated Canadian Geologist, has been made an officer of the Legion of Honour. Had its acceptance been permitted by our Government, a number of honours would have been conferred, on Monday last, upon many English men of science.

The Human Voice.—Dion Bourcicault, commenting on the Albert Hall of Science and Art, in the *Pall Mall Gazette*, says:—"The human voice, when speaking with clear articulation and supplied from good lungs, will fill 400,000 cubic feet of air, provided they be enclosed in a proper manner, and the voice placed and directed advantageously. The same voice singing can fill, with equal facility, 600,000 cubic feet. When singing, the vowels are principally used, because it is necessary to dwell upon a note, and we cannot prolong a consonant. In speaking, on the contrary, we depend for articulation on the consonants; but their short percussive sound does not travel. When we shout, or in open-air speaking, which partakes of shouting, we prolong the vowels, drawling the syllable of each word; but what we gain in sound we lose in clearness of articulation; expression is lost in monotony, because its fineness depends on the infinite variety of which the consonant is capable and bestows on the vowel. 2000 voices, singing or speaking together, travel no further than one voice. They may fill a certain area more completely with that intricacy of waves which, when very troublesome, we call a din; but each voice exerts its own influence on the air, according to its power, and dies away within certain limits. A second voice acts independently, and produces its own separate effect, not fortifying the first, but distinct from it. And so with any number of voices—say 10,000—shouting together, if a single trumpeter were placed among them, the note of his trumpet would be heard clearly at a distance where the Babel of voices would have expired in a murmur. Yet, among the din produced by the 10,000 voices, the trumpet would be inaudible. To illustrate this theory more clearly, it is plain that 2000 persons cannot throw stones further than one person. It is true that the air, within certain limits, will be more full of stones; but they will all come to the ground within a limited area.

On the Manufacture of Zinc.—C. Trainer. Comparison of the Belgian and Silesian distilling furnaces gives the following result: The Belgian furnaces with some 60 effective retorts in 7 to 8 horizontal rows require less fuel, the process is more intense, is finished sooner, and yields more. However, they require a full-flaming coal, and the consumption of retorts is greater; and when breakages occur the under retorts often suffer. The attention to these furnaces requires very intelligent and experienced workmen, as they have to keep up an equal temperature of all the retorts, above and below, front and back. On the other hand, the Silesian furnaces require less skilful workmen, and less vessels, as they last longer, while the consumption of fuel is larger, but a less open burning coal may be used. The latest improvements in the Silesian furnaces for the purpose of saving fuel (two rows of muffles above each other, and a downward flame) require a free burning coal as well as more expert workmen. A greater durability is ascribed to the Silesian furnaces than to the Belgian; this advantage, however, is doubtful after having introduced considerably improved arrangements in the front wall of the Belgian furnaces. Such improved furnaces lasted two years, at an average, near Iserlohn. It is advantageous to the durability of the furnaces for them always to be attended by the same workmen. The Silesian process extracts more zinc from the ores on account of their longer stay in the furnace and the less amount of breakage of the distilling vessels, whilst the Belgian furnaces have, for a fixed time, greater productiveness. When the ore forms tough slags the Silesian method is preferable. Trials of zinc-melting in blast-furnaces have not been successful. It would lead to considerable advantage if the distillation of the zinc-ore could be effected in a large flat muffle, which could be sufficiently heated and would not crack. Such an apparatus, on the one side with openings for charging, on the other with vessels for condensation, would require a less amount of fuel.—*Berg- und Hütten-Zeitung*, 1867, No. 24.

Mercerising Cotton.—To Mr. Mercer must be attributed the discovery of the peculiar action of caustic soda and sulphuric acid upon cotton. This singular process, now called "mercerising," has the effect of untwisting the normally twisted flattened tubes of cotton filaments and converting them into cylindrical tubes. When colours are applied to the cotton so treated, they pass more readily through the minute pores of the tubes and are precipitated in denser layers in the interior of the latter, whereby darker and more permanent shades are produced. Calico so treated becomes greatly increased in strength, and though hitherto no large quantities of cloth thus prepared have been printed, owing to the expense of preparation, advantage has been taken of the process to prepare the cotton fabric used in the production of the endless web known to calico printers as the india rubber blanket, which, when made with prepared calico, is rendered much more durable.—*Proc. Lit. Phil. Soc., Manchester.*

Determination of Bismuth in Lead Alloys.—(Patera). The alloys are dissolved in nitric acid, the solution diluted with water, and the bismuth precipitated by a strip of pure lead. The precipitated bismuth, black of colour and in the state of powder, is quickly washed off the lead, the solution of lead is decanted; the bismuth is then washed first with water and then with alcohol, filtered on a weighed filter, dried and weighed.—*Berggeist*, 1867, No. 28.

Blasting with Sodium.—Experiments are now being made, in the Isle of Man and elsewhere, to ascertain the value of sodium, in contact with water and other substances, for blasting purposes.—*British Journal of Photography.*

An Old Anæsthetic Revived.—An enterprising American dentist advertises that he now takes teeth out painlessly by merely causing the patient to inhale the *constituents of the atmosphere—oxygen and hydrogen—chemically combined.*

Partzite.—This mineral, discovered by Dr. Part in 1865, has been investigated by Albert Arents. It occurs in amorphous masses, generally without lustre. Fracture, varies from conchoidal to even; colour, yellowish green to blackish green and black; sp. gr. = 3·8; hardness, 3·4. Before the blowpipe on platinum it is melted, but with difficulty, to a black slag; on charcoal, especially with addition of carbonate of soda and powdered charcoal, a metallic button is easily obtained much resembling pure antimony. Sulphuric, nitric, or hydrochloric acid decomposes the mineral. An analysis gave numbers corresponding to the formula:

$$(\mathrm{CuO, AgO, PbO, FeO})_3\mathrm{SbO}_5 + 3\mathrm{HO}$$

Partzite occurs with argentiferous galena.—*Silliman's Journal*, May, 1867.

Action of Water upon Carbohydrates at an Elevated Temperature.—O. Loew. Cane sugar at 160°C. yields levolosan and glucose; at 180° caramelan; at 200° caramel, assamar, and carmelin; and at about 250° is totally decomposed. The author finds that the presence of water makes a great difference. Sugar is perfectly decomposed when heated with water in a sealed tube to 160°, the water seeming to act as an acid. Sugar heated with alcohol in a sealed tube to the same temperature remains unaltered. Sugar heated with baryta water in tubes is not decomposed at 170°C. Water acts in the same way upon other "carbohydrates." Starch, gum, and milk sugar with water are decomposed by five hours' heating at 70°C. The products are formic acid, carbon, and carbonic acid.—*Silliman's Journal*, May, 1867.

A Catastrophe Averted.—In the course of last winter the river Irwell rose nearly twenty feet above its ordinary level, and flooded the works of the Magnesium Metal Company on the Salford side to a depth of about seven feet in every part. There were then from three to four hundred-weight of sodium in stock, and, soon after the commencement of the flood, the room in which the sodium was stored was two feet deep in water; but, as it rained in torrents, it was then considered best not to run the risk of attempting to move it off the premises. The sodium was stored in long narrow jars, with loosely-fitting covers, made air-tight by allowing the bottoms of the lids to rest in a circular groove filled with oil. As the flood did not abate, and the position began to grow more dangerous, one of the men volunteered to go on the roof of the sodium shed and watch the water rise, and for hours he lay upon the roof in a soaking shower of rain, watching the sodium jars. Inch by inch the water rose, and at last, when it was only half a foot from the top of the jars, he drew his head out of the hole in the roof where it had been sticking so long and summoned the rest of the men. They unslated the roof of the store room, let themselves down into the water, now reaching nearly to their armpits, and removed the sodium lump by lump into other vessels placed among the rafters of the roof. By accident one little ingot of sodium fell into the water, causing the courage of the men to falter; but the lump fortunately only fumed and fizzed, and dissolved away without exploding. After the flood was over the Magnesium Metal Company built a platform near the roof, on which all sodium is now stored. We have not heard what bonus the Company voted to the men who removed the sodium, especially to the one who stuck to the top of the roof like a limpet to a rock in a storm, but doubtless it was something handsome.—*British Journal of Photography.*

Obituary.—Our readers will probably have already seen the announcement of the death, and have lamented the loss, of Dr. Thomas Richardson. Dr. Richardson was Reader in Chemistry at the University of Durham, as well as a Fellow of the Royal Societies of London and Edinburgh, and member of the Royal Irish Academy. He died somewhat suddenly, at Wigan, on the 10th inst., of congestion of the brain. Of late years Dr. Richardson was best known to the chemical world by his work in connection with "Richardson and Watts' Chemical Technology." Several papers of his are to be found in the volumes of this Journal and the *Chemical Gazette*. They relate chiefly to manufacturing chemistry. Many chemists will have pleasent reminiscences of their visit to the Newcastle meeting of the British Association, and of Dr. Richardson's hospitality on that occasion.

The Water we Drink.—The Registrar-General in his weekly report says, that on the intermittent system of supplying, the pipes are often left empty, and in that case sometimes get saturated with impurities. This happened in Portland-road, where analysis detected sewage contamination in the water drawn from the stand pipe on June 1st; the sewage was equivalent to 1 per cent. of the water, which was turbid when drawn, and, after standing a few days, emitted an offensive odour, and threw down flocculent matter.

University of London.—Science Examination.—Results.—FIRST B. Sc.—1st Division: Bottomley, Carey, Gum, Harding, Hopkinson, Robinson, Tilden, Wormell.

2nd Division: Ball, Brice, Bright, Graham, Leonard, Pearsall, Sheldon, Thorp, Whipple.

SECOND B. Sc.—1st Division: Exall, W. H. (King's College); Payne, J. F. (St. George's Hospital); Smith, R. S. (King's College).

2nd Division: Duer, S. (private study); Ridge, J. J. (St. Thomas' Hospital); Robinson, E. (Owen's College); Sprengell, J. C. F. L. (private study); Waller, A., B.A. (St. Thomas' Hospital); Wigner, J. M. (private study).

HONOURS.—Exall, W. H., second class; Chemistry.

PREL. SCIENT. M.B.—1st Division: Aveling, Ball, Barff, Bruce, Burn, Carter, C. H., Elkington, Gibbings, Harris, J. A., Harris, M., Haynes, Hunt, Saunders, Wall.

2nd Division: Bindley, Burgess, Carr, Carter, A. H., Cotterill, Coupland, Cross, De Mèric, Edwards, Fox, T. C., Franklin, Graham, Herman, Ingoldley, Jones, T., Lowe, Lyell, Male, Paget, Perkins, Pippette, Puglie, Ralli, Rayner, Rowland, Rugg, Simon, Sloman, Smith, Southee, Taunton, Waddy, Willaus, Williams.

Experiments on the Poison of the Cobra-di-capella.—The *Melbourne Argus*, for April 26th, contains an interesting article, by Dr. G. B. Halford, on the above subject, from which we extract the following:—"The melancholy accident which so lately happened with the cobra-di-capella induced me to make some experiments and observations upon the action of the reptile's poison. When a person is mortally bitten by the cobra-di-capella, molecules of living 'germinal' matter are thrown into the blood, and speedily grow into cells, and as rapidly multiply; so that, in a few hours, millions upon millions are produced at the expense, as far as I can at present see, of the oxygen absorbed into the blood during inspiration; hence the gradual decrease and ultimate extinction of combustion and chemical change in every other part of the body, followed by coldness, sleepiness, insensibility, slow breathing, and death. The cells which thus render in so short a time the blood unfit to support life are circular, with a diameter on the average of one seventeen-hundredth of an inch. They contain a nearly round nucleus of one two-thousand-eight-hundredth of an inch in breadth, which, when further magnified, is seen to contain other still more minute spherules of living 'germinal' matter. In addition to this, the application of magenta reveals a minute coloured spot at some part of the circumference of the cell. This, besides its size, distinguishes it from the white pus or lymph corpuscle. Thus, then, it would seem that, as the vegetable cell requires for its growth inorganic food and the liberation of oxygen, so the animal cell requires for its growth organic food and the absorption of oxygen. Its food is present in the blood, and it meets the oxygen in the lungs; thus, the whole blood becomes disorganized, and nothing is found after death but dark fluid blood, the fluidity indicating its loss of fibrine, the dark colour its want of oxygen, which it readily absorbs on exposure after death. It results, then, that a person dies slowly asphyxiated by deprivation of oxygen, in whatever other way the poison may also act, and so far as the ordinary examination of the blood goes, the *post-mortem* appearances are similar to those seen after drowning and suffocation. I have many reasons for believing that the *materies morbi* of cholera is a nearly allied animal poison. I hope also to show the presence of the poison of our snakes in the blood of bitten and inoculated animals, and to make some experiments on the possibility of saving life.

Behaviour of Manganese with Chlorate of Pot-

ash before the Blowpipe.—If chlorate of potash be heated by means of a blowpipe, in a tube closed at one end till oxygen is evolved, and then a trace of manganese added, the potash salt will assume a purple colour, owing to the production of permanganate of potash. This reaction of manganese is quite as delicate as the one proposed by Berzelius. —*T. Landaner.*

CONTEMPORARY SCIENTIFIC PRESS.

[Under this heading it is intended to give the titles of all the chemical papers which are published in the principal scientific periodicals of the Continent. Articles which are merely reprints or abstracts of papers already noticed will be omitted. Abstracts of the more important papers here announced will appear in future numbers of the Chemical News.]

Comptes Rendus. April 15 and 22, 1867.

H. Fizeau: "*New Observations on Iodide of Silver.*"—A. Secchi: "*Note on the Spectra of the Stars.*" "*On the Transparency of Red-hot Iron.*"—V. Louguinine: "*Action of certain Substances having a Powerful affinity for Water on some Aromatic Aldehydes.*"—Berthelot: "*Method for Reducing and Saturating Organic Compounds with Hydrogen.*"—Dusart: "*Preparation of Phenols.*"—Tresca: "*On the flow of Solids under Heavy Pressures.*"—T. S. Hunt: "*On the Formation of Gypsums and Magnesium Lime Stones.*"—P. Gauchler: "*Theoretical and Practical Researches on the Flow and Motion of Water.*"—T. Richter: "*On Indium.*"—Berthelot: "*Method of Reducing and Saturating Organic Compounds with Hydrogen.*"—A. Perrot: "*On obtaining High Temperatures by the Combustion of a Mixture of Illuminating Gas and Air.*"

April 29.

Becquerel: "*On the Causes of Rain.*"—T. S. Hunt: "*Some Reactions of Magnesia Salts, and on Rocks Containing Magnesia.*"—Coupvent des Bois: "*On the Determination of the South Magnetic Pole.*"—Naquet: "*On the Principles of Chemistry according to Modern Theories.*"—Chacornac: "*Resumé of a Memoir on the Solar System.*"—L. Cailletet: "*On a Process of Gilding and Silvering by Means of Sodium Amalgam.*"—H. Deville: "*On the same Subject.*"—L. Dusart: "*Contributions to the knowledge of Phenols.*"—J. Kolb: "*On the Absorption of Carbonic Acid by some Oxides.*"—P. P. Deherain: "*Experimental Researches on the Use of Potash Salts in Agriculture.*"—C. Mene: "*On Yellow and White Iron Pyrites.*"

May 6.

E. Carre: "*On some new Refrigerating Apparatus.*"—J. L. Soret: "*Researches on the Destiny of Ozone.*" (Second Part).—F. P. Le Roux: "*On the Cause of the Undulations produced in Metallic Wires by the Electric Discharge.*"—E. Jungfleish: "*On some Points of Relation between the Melting Points, Densities, and Specific Volumes of some Chlorinated Derivatives of Benzene.*"

NOTES AND QUERIES.

The use of Superheated Steam.—Sir,—About ten years since I carried out a series of experiments with superheated steam, in conjunction with an experienced engineer. In the first instance the steam was applied direct from the boiler at a pressure of 50 lbs., and passed through cast-iron cylinders, heated to redness, from thence through cap welded wrought-iron pipes to a cast-iron steam jacketted pan or pot (each pot was 1½ inches in thickness, and very strongly bolted together). As soon as the superheated steam passed through the pipes, the gun metal steam taps instantly broke off short, owing to the expansion. The stays to the pipes were then removed and the taps replaced. After the superheated steam had been applied to the jacketted pot for upwards of two hours, the interior pot burst from the outer pot at the flange, and was projected through the roof of the building to some considerable distance, and the walls on each side of the building were blown down (the pot weighing about 12 cwt). Some difference of opinion existed as to the cause of the accident, as the steam was not locked in; the valve at the bottom of the pot was open, and also the valve for the outgoing steam. Perhaps the steam, being highly elastic, could not escape with sufficient rapidity. After considerable experience I have employed superheated steam with great success and most economical results both for desiccation and the evaporation of saline solutions, etc. Superheating steam does not require an expensive apparatus; it can be readily accomplished at the back of the furnace of a steam boiler, more especially where Jukes' smoke apparatus is used. A very even temperature can be easily obtained, and in operation it is even superior to Perkins' hot water apparatus, although I have not obtained so high a temperature.—W. H.

Cheap Grease.—Sir,—I am endeavouring to make cheap grease. I have succeeded in making the superior greases. But with the cheapest greases there appears to be a difficulty which I cannot master. They are sold here at 10s. 9s. and 8s. per cwt. These prices are so low that it is to me impracticable to use any other ingredients but rosin, alkali and water. But I find that grease made with these only is sticky and drying, and totally unsaleable. Can any of your subscribers give me an economical recipe for making a good, yet cheap, rosin grease, and greases which will afford a good profit when sold at the above prices?—J. S. W.

To Destroy Ants.—Sir,—Should carbolic acid fail, try liquor ammoniæ fortis: ·880. This is by far the most effective for black beetles.—Q.

Charcoal Biscuits.—Sir,—To make charcoal biscuits, take flour, 1 pound; carbonate of ammonia in fine powder, 1½ drachms; white sugar in fine powder, 2 ounces; mix well together—butter, 2 ounces; one egg; mix into a stiff paste with new milk, and beat well with a rolling pin for 20 minutes; roll out thin and cut out the biscuits with an inverted egg-cup; bake in a quick oven for 15 minutes. Levigated cedar wood charcoal is to be had of Warrick, 3, Garlick Hill, London, and ready made charcoal biscuits of Mr. Bragg, 2, Wigmore Street, Cavendish Square, London.—Subscriber.

Cure for Dry Rot in Houses.—Sir,—I think there is an important oversight in the communication on this subject in your issue of 21st inst. At 27th line should it not read "*beneath* the joists?" I have a remarkable instance in my mind when asphalt had been applied merely *between* the joints of a gentleman's mansion—these joists or sleepers were supported in the centre and nailed to wooden pegs, driven about 18 inches into the soil. In about four years afterwards the whole of the lower apartments were one mass of dry rot, the said pegs having formed excellent conductors. All the woodwork was renewed, and I recommended asphalt to be spread *below, and the sleepers simply to be laid upon the smooth hard surface thus formed.* I believe this has always been effectual. I know a case just now where dry rot has reached the bedrooms.—W. B.

Chemical Calculus.—Sir,—Allow me to point out a correction which it would be desirable to make in your otherwise excellent abstract of my note at the Royal Society on the "Chemical Calculus." In the abstract my translation of Brodie's formula for hydrochloric acid is given thus:—

$$H_2{\genfrac{}{}{0pt}{}{Cl}{H}}$$

instead of thus:—

$$H_2\frac{Cl}{H}$$

And in like manner the lines which I employ between the numerator and denominator of other symbols in the form of division is omitted. My intention was to use the symbol of division to denote decomposition, just as the symbol of multiplication is habitually employed in chemical formulæ to denote combination. I notice that by some accident the number 73 is given as the molecular weight of hydrochloric acid, instead of 36·5.—Alex. W. Williamson.

Acids from Palm Oils.—Sir,—I wish to know the quantities of hard, white palmitic acid and liquid oleic acid which are usually procured from one ton of average palm oil by saponification, followed by distillation and cold and hot pressure. 2ndly. What proportions of the fatty acids are procured from tallow by the lime process, as adopted generally on the continent. I find sundry loose statements in various technical publications, but as they differ so widely I am anxious to learn from some practical person what are the real results obtained in actual working.—Delta.

Refrigerating Machine.—Sir,—Is there any manageable machine in use for manufacturing purposes, for cooling mother liquors?—P.

Distilling Oil.—Sir,—Can any of your readers inform me of any work that treats of oil distilling? I can find no information in any work on chemistry. I want information on rosin oil distilling and refining, and the improvements made in this branch of business. What is the best process for refining rosin oil, nearly destitute of smell, and of a blueish colour?—H. J.

Cheap Grease.—Your correspondent, J. S. W., should try a grease made from the last oil or grease obtained during the distillation of coal tar, or he could try a mixture of rosin and the grease referred to. If your correspondent added a small quantity of the commonest soap to the grease, he would find a great improvement. This would make the cheapest grease for colliery and other purposes.—H.

Charcoal Biscuits.—Sir,—To the receipt given last week add two ounces of levigated cedar wood charcoal, to be mixed intimately with the flour.—Correspondent.

Vapour Density of Water.—In reference to this subject a correspondent draws attention to a work published many years ago, by order of the French Government, containing the results of the very elaborate researches, made by V. Regnault, on the density, etc., of steam. Our correspondent does not mention the title of the book, but he says that in it the subject mooted by Mr. Ward is fully and conclusively set forth.

Sulphate of Magnesia.—A subscriber wishes to know the name of a firm in the North of England who make sulphate of magnesia by the process described in our journal of April 12th.

Origin of the word Naphtha.—Sir,—I have been trying to trace the origin of the word naphtha, but have not succeeded. Can any of your readers tell me where it was first used to designate an inflammable liquid coming out of the ground?—F. Peterson.

Papering Damp Walls.—Sir,—If your correspondent of last week, R. A. Dudley, will get some of the water-glass or soluble silicate of soda advertised in your columns, and brush the dilute solution several times over the damp wall, allowing it to soak in and dry between each application, he will effect a cure.—E.

Manufacture of Sulphuric Acid.—Sulphurous acid, in presence of much water, reduces binoxide of nitrogen to oxide of nitrogen. Can any of your readers kindly inform me what strength the chamber should work to prevent this? I am working two chambers connected. I should also be glad to know what quantity of nitrate of soda is required to make 20 cwt. of sulphuric acid sp. gr. 1750.—Child.

Plaster of Paris.—I should feel obliged to any of your correspondents who would inform me whether there is any way of causing plaster of Paris to set perfectly hard, so that it cannot be scratched by the finger-nail. I have tried the plan recommended in *Muspratt's Chemistry*, page 462, but do not find it to succeed.—A Working Potter.

Making Pyrogallic Acid.—Sir,—Will any one inform me whether a method has been published whereby pyrogallic acid can be made in the wet way from gallic acid? The process of sublimation usually adopted is tedious and uncertain. Some years ago a variety of pyrogallic acid in hard prismatic crystals was introduced in photography; this was evidently crystallized from an aqueous solution. Query—Was it sublimed first and crystallised afterwards, or was it not rather prepared in the wet way?—I. Couper.

Iron made Red-hot by Hammering.—Sir,—In answer to "Sceptic" (Chemical News for August, 1867), I beg to state that it is a common thing for a good blacksmith to hammer a horse-shoe-nail red-hot upon a common anvil. I have seen it done by one Jesse Stubs repeatedly, who informed me that "years ago when he was a lad," it was not an uncommon thing for a journeyman blacksmith on applying for work, to have to prove himself a good hammerman by making a nail red-hot in as few a number of strokes as possible. I once produced a blacksmith and anvil at a lecture, before the Royal Literary Institution of Hull, when the man made the nail glow before the audience by hammering it. Old blacksmiths in the country say that before the days of Congreve, Letchford, or Bryant and May, they many a time lighted their forge fire of a cold morning by means of a nail made red-hot by converting motion into heat, or as they term it, "a few sharpish taps" with a hammer. Let "Sceptic" go to a large blacksmith shop and offer a shilling to every man who will hammer a nail red-hot, and unless blacksmiths have degenerated during the late severe winter, he will soon part with his money.—J. W.

Mercerising Cotton.—Sir,—There is no doubt that Mr. Mercer's discovery mentioned in your present number, is a valuable one, and were the objections to it more generally known, some of your talented readers might succeed in overcoming them. The advantages are that the fabric contracts about one-fifteenth linearly in each direction, and the threads appear rounder, firmer, and closer together: the cloth does not reflect so much white light, but has a translucent appearance. Its strength is also improved; cotton thus treated shows a superior affinity for some colours, especially indigo-blue; it takes as deep a shade of blue in one dip as common cloth takes in six, and, generally speaking, colours look better on this than on untreated cloth. The objection to the process was mainly the expense of the soda, but now that this agent has been reduced in price this objection will not be so formidable. It was also said that the appearance of greater fineness and closeness, produced by the contraction of the fibre, could be more surely and economically produced by the loom.—F. Oglevie.

⁂ *With reference to Mr. Ward's figures in our last number, we feel it right to mention that we printed them from a private note, evidently written off-hand, and unrevised: as also that the writer's corrected version reached us by the post following that by which the* Chemical News *would come into his hands, and antedated the other letters on this subject. The fact of Mr. Ward's letter following instead of preceding the other letters occurs in accordance with a rule adopted in most printing-offices, when there are several communications on the same subject, of placing the shortest first.*

ANSWERS TO CORRESPONDENTS.

⁂ All *Editorial Communications* are to be addressed to the Editor, and *Advertisements* and *Business Communications* to the Publisher, at the Office, 1, Wine Office Court, Fleet Street, London, E. C. *Private* letters for the Editor must be so marked.

⁂ In publishing letters from our correspondents we do not thereby adopt the views of the writers. Our intention to give both sides of a question will frequently oblige us to publish opinions with which we do not agree.

We have been requested to correct an error which has been very generally made by the English Press (the Chemical News included). In the list of firms which have gained silver medals at the Paris Exhibition, the well-known firm of *Morson & Son* has been misprinted *Mancson*.

Clericus.—The powder for Larkin's magnesium lamp can be obtained at the magnesium metal company, Manchester; apply to Mr. Mellor, manager.

Querist.—Apply to F. Viewig and Son, Publishers, Brunswick, for the works of Baron V. Liebig.

J. F.—The substance is sesquioxide of iron; but whether derived from the pipe or deposited from the water, we cannot say. Is the pipe corroded?

Edward, O.—You will be able to obtain all the information you require by applying at the Patent Office, Southampton Buildings, Chancery Lane.

A New Subscriber.—Our first volumes can occasionally be procured second hand. Our publisher is sometimes able to secure a set. Leave your name at our office, and state how much you are inclined to give for Vols. I. and II.

Dr. A. L. P.—We regret that your communication is too long for our columns.

F. Robson.—Pyrophosphate of soda in solution will dissolve sulphur, forming a polysulphide.

F. Huddersfield.—Scheele's green is arsenite of copper. Schweinfurt green is mixed arsenic and acetate of copper. If chromium green will be bright enough for what you want we certainly advise you to use it in preference.

J. S.—We have been unable to find the paragraph in question; about what date did it appear?

W. H. A.—There is no difficulty in taking ink stains from paper. Most acids will effect the desired object. The principal thing to be attended to, is to remove every trace of the acid from the paper after it has done its work. This is a difficult thing to do properly, and its neglect is the cause of the rotting so often complained of after the removal of ink-spots.

W. J. S.—The term photograph is the generic name applied to all pictures taken by the agency of light. A daguerreotype is, therefore, as much entitled to be called a photograph, as a paper print from a collodion negative.

A Druggist.—Picrate of Potash has been used by Braconnot as a substitute for quinine, in intermittent fevers; the dose is from two to five grains. It was said to answer very well, but makes the patients as yellow as guineas.

D. R.—Sulphate of baryta is soluble in hot strong sulphuric acid, but water precipitates it.

A Manufacturer.—If you advertise in our pages you will be certain to receive many answers.

S. Johnson.—To dye silk with gold, immerse the stuff in a bath of chloride of gold for ten minutes, then wring and dry. Expose to the sunlight, when the colour changes to a beautiful lilac.

Papering Damp Walls.—Sir,—One of the walls in my house is so damp that the paper will not stick; it mildews and peels off within a few months. I am not a chemist, but I should think that some of your correspondents will be able to suggest a remedy.—R. A., Dudley.

Errata.—Page 113, line 16 from the top, *for* carbonate of soda, *read* carbonate of ammonia; page 117, line 2 from the bottom, *for* 1863, *read* 1836.

B. H. A.—Nitrite of amyl may be prepared by passing nitrous vapours into amylic alcohol contained in a heated retort, rectifying the distillate, and collecting apart the portion which goes over at 90 deg. C.

James P.—Your problem in hydrostatics fails in one important requisite: you have forgotten to allow for the force of gravitation.

A. L. S.—It would be a breach of confidence to impart the information, even if we possessed it.

Barium.—Your communication is declined with thanks.

Medicus.—One of the best hæmostatic agents is said to be a mixture of perchloride of iron and common salt, dissolved in water. No free acid must be present.

M. Kestner.—The best examination of the fixed line D of the solar spectrum was given with a diagram, by Professor Cooke, in our number for July 4, 1863. The drawing shows eight sharp lines, and a nebulous hand between the two constituents of D.

Junior Student.—The diaphanometer is an instrument proposed and employed by Saussure for measuring the transparency of the air. Its action was very imperfect, and any variation in the acuteness of the observer's sight would vitiate its results.

A. B.—There is no danger. Push the heat as far as you can.

Zena.—Don't be misled, the book is a very good one.

W. Thomson.—Silver solder is best for uniting steel together. Make it by melting together 10 parts of silver, 1 part of copper, and 1 part of brass. Use borax as a flux.

C. Chase.—You had better consult a solicitor. We would rather not advise in such a case.

Dyer.—Stannate of soda is now generally employed. See if your sample contains tungstate of soda. The stannate contains 9 equivalents of water of crystallization. It is efflorescent in a dry atmosphere.

Foreign Science.—Up to the time of going to press we have not received the Abbe Moigno's letter, nor the proceedings of the *Academie des Sciences* for Monday last.

D. Waldie.—Our correspondent's communication is received with thanks. We shall always be pleased to hear from him.

Communications have been received from W. Briggs; W. Huskisson; Prof. Williamson, F.R.S.; F. Field, F.R.S.; Dr. Odling, F.R.S.; C. Greville Williams, F.R.S.; Dr. R. Angus Smith, F.R.S.; D. Forbes, F.R.S.; C. S. Read; P. Jessop; R. Lowe; C. R. Wright, B.Sc.; John Robinson (with enclosure); G. F. Rodwell, F.C.S.; E. Kierman (with enclosure); Alex. Glendining; Prof. G. G. Stokes, F.R.S.; Dr. Letheby; G. A. Keyworth; H. Bywater (with enclosure); J. Wallace; J. H. Swindell; Prof. Daubeny, F.R.S.; Joseph Davies (with enclosure); Edmund G. Tosh (with enclosure); Lewis Demuth and Co.; J. T. Atkinson; W. Ladd; Hood; F. A. Abel, F.R.S.; Dr. Letheby; W. B. King; Morson and Son; H. B. Condy; Sir B. C. Brodie, Bart. F.R.S.; Dr. F. C. Calvert, F.R.S.; B. Westermann and Co. (New York); G. Hill; H. Woodward; D. Forbes, F.R.S.; G. F. Rodwell; E. Beanes; Dr. Odling, F.R.S.; W. Bright; Albright and Wilson (with enclosure); J. Kayess; C. R. C. Tichborne; Muspratt Bros.; Dr. Oxland; John Filley; Professor Williamson, F.R.S.; J. Ilif; T. Sterry Hunt, F.R.S.; H. Burgoyne; L. Twining (with enclosure); A. S. Herschel; J. Ingram (with enclosure); J. Layton with enclosure); C. Greville Williams, F.R.S.; Henry Woodward; F. Bright; C. R. C. Tichborne; J. N. Vinen; the Metropolitan Association of Medical Officers of Health; S. W. Moore; T. Landaner (with enclosure); George Lunge; C. Foster, B.A.; Ludwig Mond; W. M. Watts, B.Sc.; Dr. F. C. Calvert, F.R.S.; C. R. C. Tichborne; Gossage and Son (with enclosure); Jesse Fisher, do.; Benjamin Wheeler, do.; G. Eccles, do.; Smith and Sons, do.; J. Sampson, do.; J. Thom, do.; W. Blyth, do.; W. M'Leod, do.; H. Eve; C. R. A. Wright, B.Sc.; W. Kunde; F. Hughes; R. C. C. Lippincott; D. Waldie (Calcutta); J. D. Dana; Professor How, D.C.L.; Dr. August Stromeyer; F. O. Ward; Captain R. F. Burton (with enclosure); A. P. Hurlstone; S. Norman; Dr. P. Williams; B. Quaritch; J. Foord (Victoria); J. Thom (with enclosure); John Lundy.

Books Received.—"Researchess on Gun Cotton," by F. A. Abel, F.R.S., being the Bakerian Lecture for 1867; "Crystallogenic and Crystallographic Contributions," by James D. Dana.

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NOTES ON SOME COMPOUNDS OF PALLADIUM.

BY HENRY CROFT,

PROFESSOR OF CHEMISTRY, UNIVERSITY, TORONTO.

Palladio-bichloride of potassium is best prepared by passing chlorine through a concentrated hot solution of the palladio-protochloride. Almost the whole of the metal is precipitated as a double salt of a fine colour. What remains in solution may be advantageously employed in preparing the chloride of palladammonium.

The double salt $PdCl_2KCl$ exhibits a remarkable change of colour on the application of a moderate heat, turning quite black, and resuming its scarlet colour on cooling. If heated too strongly it fuses, loses chlorine, and, on cooling, is of a brown colour, being converted into the palladio-protochloride.

Cyanide of Palladammonium, originally described as ammoniated cyanide, is readily obtained by adding HCy to a solution of NH_3PdCl. It forms a white crystalline powder, soluble in hot water. Analysis showed it to be Fehling's salt.

Sulphide of Palladammonium.—When dilute $\left.\begin{matrix}H\\H\end{matrix}\right\}S$, or very dilute $\left.\begin{matrix}NH_4\\H\end{matrix}\right\}S$ is added to a solution of $\left.NH_3\begin{matrix}Pd\\Cl,\end{matrix}\right\}$ a bright red or orange red precipitate is formed, much like the sulphide of antimony; it changes very rapidly into brown or brownish black sulphide of palladium.

Double Sulphocyanides.—These may be obtained in precisely the same manner as the platinum compounds described by Buckton. The potassium salt forms ruby-red crystals, which can be obtained of considerable size, soluble in water and alcohol. The latter solvent was used for separating it from the chloride. The salt is anhydrous, melts at a high temperature, gives off sulphur and bisulphide of carbon, and is oxidised by nitric acid, forming a white compound free from sulphur, apparently analogous to Claus's product. The solution of the potassium salt precipitates various metallic solutions, forming apparently corresponding insoluble palladio-sulphocyanides.

Similar compounds can be obtained from the palladio-protochloride. The potassium compound forms dark red needles. From the few analyses I was able to make, the composition of all the above salts seems to be the same as that of the platinum compounds.

By acting on the potassium salts with ammonia, a salt is obtained crystallising in fine reddish brown needles; the same can be obtained by acting on chloride of palladammonium with sulphocyanide of potassium in the same way as recommended by Buckton for the platinum salt.

The analysis gave the formula $\left.NH_3\begin{matrix}Pd\\Cy\end{matrix}\right\}S$—sulphocyanide of palladammonium.

The sulphur is oxidised with great difficulty, even by hydrochloric acid and potassic chlorate.

Several compounds of palladammonium with organic acids, especially nitro-acids, are well crystallised, more particularly the trinitro-phenylate or carbazotate.

ON THE MANUFACTURE OF NICKEL.

BY DR. AUGUST STROMEYER.

The following communication concerning the production of nickel, will, no doubt, interest some of your readers. The method employed was kept a secret for a long time, and is partly so even now.

Formerly, nickel was produced from the speiss obtained in the making of smalt, but since the perfection attained in the manufacture of artificial ultramarine, smalt has not been so much in demand; less has therefore been made, and the old stores of speiss have been used up.

Recently some speiss has been produced from copper and silver ores containing nickel. These ores are smelted with pyrites, by which means the copper and silver are concentrated in the sulphide of iron. If the ore contains nickel in combination with arsenic (if not, arsenical pyrites is added), a bright heavy regulus composed of arsenic, nickel, and cobalt, called speiss, will be found beneath the slag.

By an oxidising smelting, the speiss becomes purified, so that only arsenical nickel remains.

The speiss, in a pure state, has the composition Ni_2As (52 per cent. of nickel); ordinarily, it contains iron, cobalt, bismuth, lead, antimony, and sulphur.

By smelting the speiss, or any nickel ore in contact with atmospheric air, and a reagent capable of fluxing the resulting oxide, pure arsenical nickel (Ni_4As) is obtained. The remaining separation of arsenic from the nickel is very difficult.

The following process for the production of nickel is that used at Dittenberg (Nassau). The ore is iron pyrites containing *capillary pyrites* (NiS), and occurs in veins of serpentine. Besides the nickel ore, a copper ore is also worked. The nickel ores are not previously prepared, as the matrix gives a suitable slag.

The average composition of these ores is:—

Dolomite	CaO,CO_2	8·07
	MgO,CO_2	8·13
Sparry iron ore	FeO,CO_2	22·86
Copper pyrites	$Cu_2S+Fe_2S_3$	21·98
Nickeliferous pyrites	$\left.\begin{matrix}\frac{3}{5}Ni\\\frac{2}{5}Fe\end{matrix}\right\}S$	6·88
Bismuth glance	BiS_2	2·05
Iron pyrites	FeS_2	7·72
Red hæmatite	Fe_2O_3	11·61
Quartz	SiO_2	10·33
Moisture		0·27
As,Co,KO,NaO		0·30
		100·20

Other analyses make the nickel to exist as capillary pyrites (NiS)—this mineral is seen now and then in the ores. The ore yields on an average 3 per cent. of nickel and 5 per cent. of copper. In 1857, Heusler introduced a process for the production of a nickel-copper alloy similar to the Swedish one.

The process may be divided into four parts.

1. **Raw Melting.**—Roasting the ore and smelting it to coarse metal.

2. **Concentration Melting.**—Roasting the coarse metal and smelting it to a concentrated regulus.

3. **Refining Melting.**—Separation of the iron from the concentrated regulus.

4. **Roasting and Reducing Process.**—Transformation of the regulus into oxides of copper and nickel by roasting, and reduction of the oxides to copper-nickel.

1. **Raw Melting.**—The ore is crushed to pieces of the

size of a man's fist and roasted in kilns. On the sole of the furnace are bedded about $\frac{1}{16}$ cwt. of charcoal, then ¼ cwt. of brown coal, then 15 cwt. of ore, then again ¼ cwt. of brown coal, and finally 25 cwt. of ore. The charcoal at the bottom is lighted, and, after 12 hours, the roasting will have proceeded so far that three-fourths of the ore may be taken out of the furnace as being well roasted. Brown coal and ore are again put on in layers. 100 cwt. of roasted ore is mixed with 63 cwt. of slag from the former melting, and smelted with coke in a furnace. This is filled with charcoal, which is lit and allowed to burn down a little, which having taken place, one barrow of coke and two of slag are put on and the blast-engine worked. Two barrows of mixture are now put on to one of coke, and by a gradually increasing amount, seven or eight barrows of mixture are added within a few days to one of coke. (One barrow of coke=20lbs.; of mixture=30lbs.) The silicious slags from the copper-ore smelting are the most convenient to employ. One operation usually lasts six weeks.

2. **Concentration Melting.**—The regulus is now stamped and passed through sieves. It is then roasted in a reverberatory furnace. The charge of the furnace is 10 cwt., the volume of which becomes doubled by roasting.

The firing in the beginning of the process is kept low, as the heat increases by the oxidation of the charge. After two hours the heat is raised, till at the end of the process it reaches a white heat. During the last two hours powdered charcoal is added to decompose any sulphates formed.

Sohnabel has made most careful analyses of the regulus removed at different times during the process. From these it is found that the desulphurisation takes place gradually.

The regulus still contains 7 per cent. sulphur. It is again smelted, being now mixed with 1 per cent. of lime. 100 cwt. is prepared with 67 cwt. of silicious copper cinders, poor in oxide. The charge commences with 2 barrows of preparation to 1 barrow of coke, and the proportion increases to 5 or 6 barrows in 24 hours.

3. **Refining Melting.**—This melting operates as an oxidation process, and is used to separate completely the iron. It is carried on in a copper refining furnace with silica. Here the concentrated regulus is smelted with coke, when it melts and falls in drops to the bottom of the furnace. The iron remains in the slag as silicate of protoxide of iron, which is easily fusible.

To melt 170 lbs. of regulus it takes about 1¾ hours. The fuel is now taken out of the furnace, and the slag on the surface of the metal, cooled by a blast and renewed. The process is renewed with fresh flux, and repeated until the slag possesses an enamel-like appearance, a proof of the separation of the iron. The resulting metal is melted and drawn off by a gutter in the smelting hearth.

4. **Roasting and Reducing Process.**—The nickel-copper regulus thus obtained is now converted by roasting into oxides of nickel and copper. For this purpose it is ground to powder and sifted through very fine sieves; 4 cwt. of the powder are spread out on the hearth of a reverberatory furnace, and are roasted for 12 hours, being continually and thoroughly stirred up. The regulus by this roasting becomes desulphurised till it contains not more than ¾ per cent. The powder is then again roasted for 8 hours, at first at a red heat, and finally at a white heat. By this treatment all the sulphur, and also the small amounts of antimony and arsenic still retained, are separated.

The manipulation used in this latter process is kept a secret. The writer presumes that the regulus is mixed with carbonate and nitrate of potash; by heating with this mixture sulphantimoniate and arseniate of potash would be formed,—compounds soluble in water. The oxides of copper and nickel have now only to be reduced to the metallic state. The moistened oxide, in charges of 150 or 200 lbs., is reduced in a charcoal furnace.

The slags contain nickel and copper, partly scorified, partly mechanically suspended. They are reserved for re-smelting.

The result of the process just described is an alloy of copper and nickel; the nickel is therefore in a convenient form for use in the manufacture of German silver.

ON THE INACTIVE CONDITION OF SOLID MATTER.

In the Chemical News for Nov. 30, 1866, we gave an account of some remarkable experiments by M. Gernez, as recorded in the *Comptes Rendus* for 19th November, 1866. It is well known that when a solid body, such as a glass rod, is introduced into soda water, seltzer water, or other supersaturated solutions of gas, it becomes immediately covered with gas bubbles, and even effervescence may set in. The rod, however, becomes inactive after a short time, or it may be made so by previously immersing it in water, or by heating it in the flame of a spirit lamp, or by keeping it out of contact with air. The theory is that it is not the solid that disengages the gas from its solution, but the air in contact with such solid. The inactive solids become active if exposed to the air for a quarter-of-an-hour or an hour. Every solid, however smooth, is covered with roughnesses that form a sort of network of capillary conduits, into which surrounding gases penetrate and condense; and the gas bubbles thus imprisoned act as centres of force in liberating gas from solution. The rougher the body the more brisk the effervescence. The disengagement of gas ceases in time, since each bubble carries with it a portion of the gas which produced its liberation. Heat expels the gas from the surface of the solid by expansion, and immersion in water by solution.

In order to support this view, the author relies upon the following experiment:—"I introduced into a supersaturated aqueous solution of carbonic acid an almost capillary tube, closed at one end, and inverted like a gas-jar, and containing air. I had previously deprived this tube of the property of liberating gas. Immediately after immersion, gas adhered to the column of air which the tube contained, forming quickly a large bubble, which was disengaged; then another was produced, and so on. The gas formed then only at the point where the liquid touched the column of air. From this experiment, which I have varied in several ways, it may be concluded that air liberates carbonic acid from its aqueous solution." It is further stated that the nature of the gas is of no consequence, for "supersaturated solutions lose their gas under the influence of any gas bubbles whatever."

In the August number of the *Philosophical Magazine* Mr. Tomlinson, F.R.S., of King's College, combats the above theory, and denies that air or gas has any action in liberating gases from their solutions. The theory that he proposes to substitute rests on the distinction between a chemically clean solid and one that is clean in the ordinary sense of the word. If the solid be chemically clean there is perfect adhesion between it and the solution, and there is no liberation of gas; if the solid be not chemically clean, then the adhesion is imperfect, and there is a separation of gas. If the water is not attracted by the solid, the gas is; for although the rod may not be clean enough for water to adhere to it, yet gas will adhere to a dirty or a greasy rod. If the rod be made chemically clean it soon ceases to be so by exposure to the air; and this circumstance, according to Mr. Tomlinson, has led the numerous writers on supersaturated solutions of salts into error as to the action of *nuclei*, etc., in inducing crystallisation. If the nucleus be chemically clean, the solution wets it perfectly, and there is no separation of salt from the water; if the nucleus be not chemically clean the adhesion is differently distributed between the water and the salt, or the water and the gas, and there is a separation. According to this view a chemically clean body which appears to be "inactive," is really most active; while the so-called "active" condition is really one of imperfect adhesion.

We select a few of Mr. Tomlinson's experiments in support of this novel view, which, if admitted, will throw a great deal of light on a very obscure object:—

Experiment 1. Two ordinarily clean test-glasses, A and B, were taken; A was first filled with methylated spirits of wine and then rinsed with abundance of water. A bottle of soda water was now poured gently into the two glasses. The inner surface of B was profusely covered with gas bubbles, but not a single bubble was seen on the surface of A.

Experiment 2. A glass rod and a platinum spatula were dipped into A and B, and produced abundant effervescence. They were dipped into spirits of wine, rinsed, and again put into A and B. Not a bubble of gas appeared on either surface, except above the points at which the bodies had been made chemically clean, and there numerous gas bubbles appeared. Indeed it was accurately determined by the formation of these bubbles how far the solids had been dipped into the spirit.

Experiment 3. A rat's-tail file made chemically clean did not liberate any gas; but dried and drawn through the moist hand (experiment 4) it liberated gas abundantly.

In like manner dry iron filings (experiment 5) that produced effervescence, did not do so after being washed in spirit; when thrown wet into soda water the spirit was of course displaced by the liquid, and the filings were wetted by it, and yet there was no liberation of gas.

A wire gauze cage (experiment 7) full of air was lowered into soda water and there was no escape of gas so long as the cage was chemically clean; when the cage was handled with dirty hands (experiment 8) and put into the soda water there was an abundant effervescence.

Experiment 9. A glass rod was heated in oil above 300° F.; then wiped with a duster and put into soda water. It was instantly and completely covered with gas bubbles.

Experiment 11. A large fragment of flint was broken into two pieces and put into soda water. Gas was abundantly liberated except from the two new and chemically clean surfaces, and on them not a bubble was seen.

Experiment 12. A narrow tube eleven inches long was kept in spirits of wine for an hour for five inches of its length. It was closed at top and so lowered into soda water. There was no liberation of gas either from the tube or the column of air in contact with the solution. On removing the finger, the solution ascended the tube, but there was no liberation of gas until the solution touched that part of the tube which had not been immersed in the spirit, and from this gas was liberated both from the outside and inside of the tube. It is remarked that had M. Gernez made the inside of his tube as "inactive" as he made the outside, he would have seen that the column of air had really no action in liberating the gas.

ON THE

UTILISATION OF THE WASTE PRODUCTS OF THE MANUFACTURE OF COAL GAS.

BY DR. LETHEBY.

(Continued from page 127.)

LOOKING, therefore, at the compositions of the principal products of coal tar distillation, it may be said that the *crude naphtha* contains certain alliaceous oils, with benzole, toluole, xylole, cumole, and a little cymole, besides the more volatile basic compounds, as pyridine, picoline, lutidine, collidine, and a little aniline, with from 2 to 3 per cent. of carbolic acid and a little naphthaline.

Light oil contains cumole, cymole, and the other less volatile hydrocarbons, with a large amount of naphthaline, and the denser alkaloids, as collidine, aniline, toluidine, and even a little chinoline; besides which it contains from 10 to 20 per cent. of carbolic and cresylic acids.

Heavy oil consists chiefly of hydrocarbons which have not been well studied, and the bases which have a high boiling point, as chinoline, lepidine, and cryptidine, with small quantities of cumidine and cymidine, and from 7 to 10 per cent. of carbolic and cresylic acids.

Carbolic acid ($C_{12}\ H_6O_2$), or as it is sometimes called *phenic acid*, is largely in demand for making dyes and for disinfecting purposes, and it is most profitably extracted from the light oil before it is distilled for benzole, etc. The naphtha which flows over between 300° and 400° Fahr., and which has a gravity below 900, is best suited for the preparation of carbolic acid; for although there is much acid in the heavier oils, yet they are so nearly of the same gravity as the alkaline solution used in extracting it that there is great difficulty in separating them. The light oil is well shaken with about one-third of its bulk of a solution of caustic soda of from 14° to 16° Twaddle (1·07 to 1·08 sp. gr.) and containing from 5 to 7 per cent. of alkali. After standing for some time the oil separates, and the alkaline liquor may be drawn off by means of a syphon. This is to be neutralised with sulphuric or muriatic acid, and then the carbolic acid floats as a dark brown oil. This is the crude acid of commerce, and when purified by means of sulphuric acid and careful distillation from chloride of calcium, it forms the camphor-like substance which you here see. It has a peculiar creosote-like smell, and when largely diluted with water, even to the extent of 1 part in 10,000, it has a sweet taste. It is a very powerful caustic, turning the skin white and quickly raising a painless blister. The specific gravity of the pure acid is 1·065. It melts at from 95° to 98° Fahr., but the merest trace of water will lower its melting or congealing point, so that this is the test of the quality of the acid. It boils at 369° or 370° Fahr., and its vapour burns with a sooty flame. If it be passed through a red-hot tube it is decomposed, forming naphthaline and other hydrocarbons; and if it be heated for some time with ammonia in a closed tube, at a temperature of from 400° to 500° Fahr., it produces aniline and water thus:—

$$\underbrace{C_{12}H_6O_2}_{\text{Carbolic acid.}}+NH_3=\underbrace{C_{12}H_7N}_{\text{Aniline.}}+2HO$$

It combines with alkalies to form salts, but the combination is very feeble, for the acid is set free by heat and even by the carbonic acid of the atmosphere, so that the common preparation of it, carbolate of lime, slowly evolves carbolic acid when it is exposed to the air.

The acid is a very powerful antiseptic and disinfectant. It is especially destructive of the lower forms of organic life, and hence, perhaps, its value as a disinfectant. Several varieties of the acid are now prepared and sold for general and medical purposes, and the experience of the last few years has proved it to be an important hygienic agent. Its use in the preparation of dyes will be explained directly.

The other acids of coal tar, as *cresylic* ($C_{14}H_8O_2$), *phlorylic* ($C_{16}H_{10}O_2$), *rosolic* ($C_{24}H_{12}O_6$), may be obtained by the use of a stronger alkaline solution as recommended by Laurent. A saturated solution of potash, added to the mixed light oil and heavy naphtha, and then treated with a little powdered caustic potash, will produce a magma from which the unattacked liquid oil may be separated. By dissolving it in a small quantity of water and allowing it to stand, it separates into two layers—an upper oily layer which is of no use, and a lower layer which contains the tar acids. When this is neutralized with muriatic acid, the crude acids float as an oily layer, and may be separated from each other by fractional distillation.

IV.—SPENT OXIDE OF IRON.

This is the next substance in order of the purification of coal gas. In its fresh state the hydrated peroxide of iron freely absorbs the sulphuretted hydrogen of foul gas, forming the black sulphide of iron. On exposure to the air the iron again absorbs oxygen, and becomes revived—the sulphur which it had before taken in as sulphuretted hydrogen being set free among the particles of the oxide. In this manner, by a succession of foulings and revivifications, the oxide becomes so charged with sulphur as to be unfit for use. It then contains from 35 to 57 per cent. of sulphur, the average being about 42 per cent.; and although it is useless at the gas-works, it is of some value in the production of oil of vitriol. Special furnaces, however, are necessary for its combustion, for as it contains about 20 per cent. of sawdust it is not capable of being used in ordinary sulphur furnaces. At Messrs. Lawes and Messrs. Hills, where I have seen the spent oxide largely used for making sulphuric acid, the furnaces are constructed with very long flues, for the purpose of completely burning the organic vapour before it enters the vitriol chamber. Each furnace is about 12 feet long and 18 inches square, with a floor of fire-brick, upon which the oxide burns. It takes about 2½ cwts. of oxide at a charge, and it burns continuously for twelve hours. The air is admitted by a sliding door in front, and the gaseous products are conveyed from the furnaces, which are placed side by side, and in three tiers over each other, to a common flue at the back, and this is extended backwards and forwards, below and above, so as to prolong the combustion to the greatest extent before the vapours enter the vitriol chamber, for if the combustion is not complete there is a considerable waste of nitre; as it is, indeed, the quantity of nitre used for the oxidation of the sulphurous acid is always about half as much more as is required with native sulphur or pyrites. I think the process might be very considerably improved by continuous instead of intermittent burning, and there is no reason why the use of sawdust may not be abandoned altogether, and spent oxide employed in its place.

V.—SPENT OR REFUSE LIME.

This is generally a very profitless material—in fact, the blue billy from the wet lime purifiers is incapable of any sort of application but that of luting. Dry lime, however, is not so unmanageable a product, for if it it is treated properly it need not occasion offence; and when it is well weathered it is of some value to the farmer. Professor Voelcker has inquired very fully into this matter, and he states that it is useful to certain soil on the following account:—

1. It improves the texture of stiff clay soils by lightening them, and of light sandy soils by giving them solidity.
2. It neutralises the acidity of some soils, and breaks up the organic matter of soils which are too rich in humus, making them more fit for the sustenance of plants.
3. It acts on the granitic constituents of a soil, and sets free the alkalies, thereby making the mineral elements of it available as food for the plant.
4. It supplies food to the plant in the form of sulphate of lime, which is especially useful to the leguminosæ.

And he concludes that well weathered gas lime, judiciously applied to a proper soil, is most useful to many plants, as clover, sainfoin, lucerne, peas, beans, vetches, and turnips; and that it is a good fertilizer, for permanent pasture, especially if the land is deficient of lime. On natural grasses the best farmyard manure often produces but little improvement until a dressing of lime, marl, or gas lime has been applied to it: the latter, more particularly, destroys the coarser grasses, and favours the growth of a sweeter and more nutritious herbage. It also destroys moss, heath, feather-grass, and other plants which are characteristic of peaty land. It is, therefore, especially suited for the improvement of such land; and so it is for the land which is deficient of lime, and which causes turnips to become warty, and be affected with the disease called "fingers and toes." For this it has been found a complete remedy. It may be applied in quantities of from one to two tons an acre, and even more where lands are very heavy, or are very peaty; and the best time to apply it is in the autumn, when vegetation is dormant, so that it can not only weather before the spring returns, but also act on the land during the whole of the winter.

One special precaution is that the lime should never be used in its fresh state, when it contains sulphide and sulphite of calcium in such proportions as to be injurious to plants. The more it is oxidised the better, and, therefore, when it is drawn from the purifiers it should be covered over with old material, so as to prevent smell, and kept until it has lost its activity. The fresh lime contains from 15 to 25 per cent. of quicklime, with a large proportion of sulphide, carbonate, and sulphocyanide of calcium; and even after six or eight months it may still contain a notable proportion of quicklime, with from 20 to 30 per cent of sulphate of lime, a like proportion of sulphite of calcium, and still more of carbonate, in which condition it is not injurious to plants.

In many places farmers are glad to have the material, and will give as much as 2s. a load for it, although the common price is about 1s. a load.

VI.—Acid and other Absorbents of Ammonia.

At the end of all the purifiers there may be placed the material which has been patented by Messrs. Sugden and Maryatt. It is made by moistening sawdust with sulphuric acid slightly diluted with water, and heating it in a retort. The woody matter is in this way charred by the acid, and contains from 30 to 45 per cent. of free sulphuric acid. When it is exhausted by being charged with ammonia, it contains 40 to 60 per cent of salt, which is easily washed out of it, leaving the charred sawdust ready for another charge of acid. The material, with the sulphate of ammonia in it, is fit for conversion into manure, and is worth £5 or £6 per ton. Another absorbent of a like nature is that used by Mr. Croll. It is made from the spent chloride of manganese from the bleaching-works by adding it to chalk and sawdust, and when saturated with ammonia, it contains from 39 to 40 per cent. of muriate of ammonia, which is easily obtained from it either by washing or subliming.

These are several waste products of the manufacture of gas, and it will be seen that, in the aggregate, their value is not inconsiderable provided they are utilized to the fullest extent.

Coal-Tar Colours.

I will now make a few remarks on the processes which are followed for the production of coal-tar colours. Most of them are derived from the naphtha which is sold as 40 per cent. benzole, which is a mixture of benzole and toluole with a little xylole. The first step of the process is to convert the constituents of this naphtha into the corresponding nitro-compounds, by carefully mixing it with fuming nitric acid or with a mixture of two parts of common nitric acid and one sulphuric. The reaction is very violent if the temperature is not controlled; but, with proper management, the three hydrocarbons lose each an equivalent of hydrogen to a like proportion of oxygen in the nitric acid, and gain the residual peroxide of nitrogen. Thus:—

$$\underbrace{C_{12}H_6}_{\text{Benzole.}} + HNO_6 = \underbrace{C_{12}H_5NO_4}_{\text{Nitrobenzole.]}} + 2HO$$

$$\underbrace{C_{14}H_8}_{\text{[Toluole.}} + HNO_6 = \underbrace{C_{12}H_7NO_4}_{\text{Nitrotoluole.}} + 2HO$$

$$\underbrace{C_{16}H_{10}}_{\text{Xylole.}} + HNO_6 = \underbrace{C_{16}H_9NO_4}_{\text{Nitroxylole.}} + 2HO$$

These three nitro-compounds constitute the dark amber-coloured, oily liquid which floats upon the acid; and when it is separated from the acid and washed with water, and then with a weak solution of carbonate of soda, it constitutes the crude nitrobenzole which is used for the manufacture of aniline colours.

It has a strong odour of bitter almonds, is heavier than water, and is very soluble in alcohol and ether. If this crude nitrobenzole be submitted to the action of a reducing agent, each of the nitro-compounds will lose its four equivalents of oxygen, and gain two of hydrogen, and be thereby converted into a corresponding alkaloid, thus:—

$$\underbrace{C_{12}H_5NO_4}_{\text{Nitrobenzole.}} + 6H = \underbrace{C_{12}H_7N}_{\text{Aniline.}} + 4HO$$

$$\underbrace{C_{14}H_7NO_4}_{\text{Nitrotoluole.}} + 6H = \underbrace{C_{14}H_9N}_{\text{Toluidine.}} + 4HO$$

$$\underbrace{C_{16}H_9NO_4}_{\text{Nitroxylole.}} + 6H = \underbrace{C_{16}H_{11}N}_{\text{Xylidine.}} + 4HO$$

This process of reduction may be effected by sulphide of ammonium (Zinin's method), or by the nascent hydrogen evolved when zinc is treated with dilute sulphuric acid (Hofmann's method), or by acting on the nitro-compounds with iron and acetic acid (Bechamps' process). I show you here an experimental illustration of each of these processes, and you will observe that for lecture experiment the process of Hofmann is the most striking, but in practice the method of Bechamps is the most economical.

One hundred parts of the crude nitrobenzole is mixed with nearly its own weight of strong acetic acid, and to this is added, little by little, about 150 parts of iron turnings. The mixture is generally made in an iron retort, and after being well stirred it becomes hot, and soon forms a pasty mass of oxide of iron with an acetate of aniline and the other bases. The reactions are somewhat intricate, but they may be practically expressed thus—

$$\underbrace{C_{12}H_4NO_4}_{\text{Nitrobenzole.}} + {}_4Fe + {}_2HO = \underbrace{C_{12}H_7N}_{\text{Aniline.}} + {}_2(Fe_2O_3)$$

And the same for the other nitro-compounds, so that theoretically the acetic acid should act indefinitely.

The mixture is then submitted to heat until the retort is nearly red hot, by which means impure aniline, etc., distils over, and when this is treated with a slight excess of lime or soda, and again distilled, it yields the crude aniline of commerce. The best product is obtained when the distillation is going on between the temperatures of 340° and 380°, for as the temperature rises to 626° two new alkaloids are produced, which Hofmann has named *paraniline* ($C_{24}H_{14}N_2$) and *xenylamine* ($C_{24}H_{11}N$).

Other processes have been suggested for the production of aniline and its homologues from the nitro-compounds; thus Kremer has recommended the use of finely powdered zinc; Wöhler, an alkaline solution of arsenious acid; Wagner, the ammoniacal solution of suboxide of copper; and Vohl, an alkaline solution of grape sugar; but none of these methods have taken the place of Bechamps'.

The crude aniline of commerce, which is a mixture of aniline and toluidine, is more or less deeply coloured liquid of an amber tint; it is heavier than water, and it acquires a blue or red colour by various oxidizing agents. A solution of chloride of lime turns it, as we see, of a bluish-purple colour. It was this reaction which suggested the name of *kyanol*—blue oil. Acidulated with a mixture of equal parts of water and strong sulphuric acid, and treated with peroxide of manganese or peroxide of lead, it produces, as you observe, a rich blue. Chromic acid makes it, as you may see, of a green, a blue, or a black colour, according to the degree of oxidation; but the most remarkable experiment of all is the coloration of the aniline when it is acidulated with dilute sulphuric acid and submitted to the action of the galvanic battery. At the platinum pole, where oxygen is evolved, it instantly becomes bronze-green, then blue, then violet, and finally red; showing that the coloration of the alkaloid is due to the nascent oxygen, and that the tint corresponds to the degree of oxidation.

The crude aniline dissolves to some extent in water, but it is more freely soluble in alcohol and ether. It readily combines with acids, and forms crystalline compounds; hence it was called *crystalline* by Unverdorben, its discoverer. These salts become coloured on exposure to the air.

The production of colours from this liquid was the remarkable feature of the Exhibition of 1862. It dates from the year 1856, when Mr. Perkin discovered and patented the process for making a rich violet from aniline by means of bichromate of potash; but it is right to say that several chemists had long before noticed the fact that the salts of aniline were capable of producing rich colours. Runge, in 1835, obtained a violet blue by acting on one of the oily constituents of coal tar with chloride of lime. Five years afterwards Fritzsche observed the blue coloration of aniline with chromic acid, and the like thing was described by Beisenhirtz; but none of these reactions commanded attention until the year 1859, when Messrs. Guinon, Marnas, and Bonnet, of Lyons, introduced a new fast purple under the name of *French purple*, which they obtained from orchil, and which became a favourite and fashionable colour. The *mauve* of Mr. Perkin, which had been for three years before the public, was so much like it, that it rose suddenly into public favour. The year after, in 1859, M. Verguin, of the firm of Rénaud Brothers, of Lyons, obtained a brilliant red from the same base, and it was patented by them under the name of *fuchsine*. These two results commanded so much attention that the scientific and technical world entered very earnestly into the investigation with the view of discovering new processes of manufacture; and at the present time we have the means of making almost every variety of tint from coal-tar products. Most of these dyes are called aniline colours, but in truth they are produced from toluidine as well as aniline, and, as we shall see hereafter, they are obtained by processes of oxidation and substitution. They are generally classified under the heads of violets, reds, blues, greens, blacks, yellows, etc.

Violets.

These have received a variety of fanciful names, as *mauve, violine, rosolane, tyraline, insidine, harmaline, imperial violet, regina purple*, etc., etc.

The first of them was obtained in 1856 by Mr. Perkin, whose patent is dated the 26th of August of that year. His process is to add equivalent proportions of diluted solutions of a salt of aniline (generally the sulphate) and bichromate of potash. A good proportion is 2 parts by weight of aniline, 2 of bichromate of potash, and 1 of sulphuric acid of English commerce. The aniline and sulphuric acid are at first mixed and then dissolved in water. To this solution is added the bichromate of potash, also previously dissolved in water, and after being well stirred they are allowed to remain quiet for 10 or 12 hours, when a dark-coloured sediment appears. This is to be collected upon a filter and well washed with cold water. It is then dried and treated with colourless coal-tar naphtha until all brown tarry and resinous matter is dissolved away. After this it is again dried and boiled in successive portions of alcohol or methylated spirit until the whole of the violet colouring matter is dissolved out. The spirit solutions are then distilled in order that the spirit may be recovered, and the residue is mauve. It amounts to only about 4 or 5 per cent. in weight of the aniline used, but its tinctorial power is very great. In this condition it is not absolutely pure, although it is sufficiently so for common purposes. To purify it, it must be boiled in a large quantity of water, and the solution treated with an alkali. The colouring matter which precipitates is to be collected upon a filter, washed with water until all trace of alkali is removed, and then dissolved in spirit. If the spirituous solution be evaporated to dryness, the pure colouring matter remains as a beautiful bronze-like substance. It is hardly at all soluble in water, ether, or coal-tar naphtha; but it freely dissolves in spirit and in weak acids, especially acetic. Concentrated sulphuric acid dissolves it without decomposing it, and forms a dirty green solution, which becomes of a beautiful blue colour with a little water, and a violet or purple with a good deal. It is, therefore, a very permanent body, although it will not resist the action of chlorine or nitric acid. Reducing agents, as sulphide of ammonium or protosulphate of iron, change it to a brown-coloured solution, which re-acquires its violet tint on exposure to the air. Like most of the aniline dyes it forms a very insoluble coloured precipitate with tannin.

Other processes have been patented for making this colour; thus, Bolley, in 1858, Beale and Kirkham, in 1859, and Depouilly and Lauth, in 1860, patented the use of chloride of lime with a salt of aniline. These solutions, when used in proper proportions, produce an insoluble purple precipitate, which is the mauve of Perkin. It is purified by washing it with water slightly acidulated with sulphuric acid, then dissolving it in concentrated sulphuric acid, reprecipitation with water, washing it with water upon a filter, and lastly dissolving in spirit. In 1859 Mr. Kay patented a process for obtaining it by adding peroxide of manganese to a strong solution of sulphate of aniline, and keeping the mixture for some hours at the temperature of boiling water. The purple solution thus obtained is to be filtered and precipitated by adding ammonia until the acid is neutralised, and the precipitate, when collected upon a fiter, washed with water, and then dissolved in spirit, forms the violet-coloured dye called *harmaline*. In the same year Mr. D. Price produced a patent for manufacturing the colour by means of peroxide of lead, instead of peroxide of manganese, and Mr. Greville Williams obtained a patent for permanganate of potash. The year after (1860), there were several patents for it, as Mr. Stark's, with ferricyanide of potassium, and Messrs. Dale and Caro's, with perchloride of copper and chloride of sodium.

In the year 1861, Mr. Adam Girard observed that a purple colour could be obtained from aniline red by mixing it with its own weight of aniline and exposing it for several hours to a temperature of 350° Fahr., which is a little short of the boiling-point of aniline. The mixtures employed were equal parts of dry muriate of rosaniline and aniline, and the product, which is a reduced condition of aniline red, is washed with water slightly acidulated with muriatic acid until all the unacted-on aniline and aniline red are removed, and the pure purple remains. This is dissolved in spirit or acetic acid, and forms the dye called *Imperial purple*. In the year following (1862), Mr. Nicholson obtained his patent for procuring the same colour by merely heating Magenta or aniline red to a temperature of from 390° to 420° Fahr. The substance first melts, and, after evolving ammonia, is changed into the purple which he named *Regina purple*.

Aniline Reds,

called *fuchsine*, *roseine*, *azaleine*, *rosaniline*, *Magenta*, *Solferino*, and other fanciful names, are conspicuous in the American section of the Paris Exhibition of this year. This colour was obtained by Dr. Hofmann as far back as the year 1843, when he was experimenting on aniline with fuming nitric acid; and 15 years later (in 1858) he again obtained it, when he was studying the reactions of bichloride of carbon on aniline. He found, indeed, that when 3 parts of aniline were heated with one part of bichloride of carbon for some time, a resinous mass was produced which furnished to alcohol a rich crimson colour. This was aniline red; but, as he was studying the reactions for other purposes than the formation of coloured products, he merely noticed the fact, and put it upon record. A year after Messrs. Verguin and Rénaud Brothers, of Lyons, discovered and patented their process for making *fuchsine*, or aniline red, from aniline, by means of bichloride of tin; and thus a practical value was given to the scientific researches of Dr. Hofmann. Fuchsine is obtained by heating together 10 parts of aniline and 6 of anhydrous bichloride of tin in a glazed iron vessel for 15 or 20 minutes. The temperature should be about that of the boiling-point of the mixture (392° Fahr.). At first the mixture becomes yellow, then gradually more and more red, until the liquid mass looks black. When this occurs it is allowed to cool, and the mass is treated with a large quantity of boiling water, which acquires a rich crimson colour. This is the dye, and it may be used at once, or purified by adding to it a quantity of common salt, in which solution the dye is insoluble. The precipitated colouring matter is allowed to subside, and, after being collected upon a filter, it is dissolved in spirit, or acetic acid, and so forms the red dye. The process patented by Mr. David Price, in the year following (1859), was to act upon a solution of sulphate of aniline with peroxide of lead, by boiling them together in the proportion of one equivalent of the former to two of the latter, until the solution acquires a deep red colour. This is filtered; and, after being concentrated by evaporation, it is again filtered, to separate a resinous substance which forms in it. An alkali is then added to neutralize the acid, and the colouring matter is precipitated as a dirty brown powder. When this is collected upon a filter, washed with water, and dissolved in spirit or acetic acid, it forms a beautiful red dye, which is fit for use. Messrs. Simpson, Maule, and Nicholson, used this process very largely until the beginning of the year 1860, when Dr. Medlock committed to them his patent for making aniline red by means of arsenic acid. The process now followed is to mix together a highly concentrated solution of arsenic acid with aniline, using the latter a little in excess; a good proportion is 20 parts by weight of syrupy arsenic acid, containing 76 per cent. of the solid acid, and 12 of commercial aniline. In this manner a pasty mass of arseniate of aniline is formed, and, when this is heated for some time at a temperature of about 300° Fahr., it intumesces, and at last forms a dark-coloured liquid, which, on cooling, sets into a resinous solid, with a bronze-like lustre. The crude colouring matter thus obtained is very soluble in spirit or water, and may be at once used for dyeing purposes, but it is better to purify it by adding a slight excess of slaked lime to the aqueous solution, and so precipitating the colouring matter with the insoluble arsenical salts of lime. The mixed precipitates are collected upon a filter, and the colouring matter dissolved out with acetic or tartaric acid. Another and better method of purification is to dissolve the crude mass in dilute muriatic acid; then to filter, and to precipitate by adding a slight excess of alkali (carbonate of soda). The colour thus set free is to be collected upon a filter, washed with water, and then dissolved in spirit and acetic acid.

Another variety of aniline red, the nitrate of rosaniline, or *azaleine*, has been extensively manufactured in England by the process of Mr. Perkin, and in France by that of M. Gerber Keller. Mr. Perkin heats a mixture of aniline, or its homologues, with dry pernitrate of mercury for some time, at a temperature of 347° Fahr. The mixture first becomes brown, and then gradually acquires a dark crimson colour, during which time the mercury is reduced, and settles to the bottom of the fused mixture. On pouring it off, and allowing it to cool, it forms a solid mass of impure nitrate of rosaniline, which may be purified by dissolving in water, and precipitating with common salt. M. Gerber Keller's process is nearly similar, except that he uses a lower temperature. He takes 10 parts of aniline, and 7 or 8 parts of dry pernitrate of mercury, and heats the mixture for several hours in a bath of boiling water. Messrs. Dale and Caro obtain the colour by heating a mixture of equal parts of aniline and powdered nitrate of lead, and then adding little by little a fourth part of anhydrous phosphoric acid. Other processes have also been patented, as that of Lauth and Depoully (1860), with nitric acid; that of Smith (1860), with perchloride of antimony, antimonic acid, peroxide of bismuth, stannic, ferric, mercuric, and cupric oxides; and Gerber Keller has claimed almost every common metallic salt that is known. As might be expected, a number of these processes are practically useless, and have been claimed for no other purpose than that of anticipating the profits of future discoveries.

Aniline Blues,

called *azaline*, *Bleu de Paris*, *Bleu de Lyons*, *Bleu de Mulhouse*, etc. Soon after the discovery of aniline red, it was observed that certain reducing agents had the property when heated with it of changing its colour to a purple or blue. Mr. Charles Lauth, for example, in 1860, described the blue colour which was obtained from *azaleine* (nitrate of rosaniline) by means of protochloride of tin, aldehyde, the natural essences, etc.; and M. Kopp demonstrated that the same colour was produced from aniline red by means of wood spirit. But as none of these colours were permanent, they were disregarded. In 1861 MM. Girard and De Laire procured their imperial purple in the manner already mentioned, by heating equal weights of aniline and dry muriate of rosaniline, at a temperature of about 350° Fahr., for several hours. If the purple is wanted, the mass is merely treated with dilute muriatic acid until it loses its excess of aniline and aniline red, but if a pure blue is required, the acid treatment is continued until all the red tint is removed, and a pure blue remains. This is finally dissolved in acetic acid, or methylated spirit, and the blue dye, called *Bleu de Lyons*, is obtained. The same blue, but called *Bleu de Paris*, was procured by MM. Persoz, De Luynes, and Salvetat, by heating a mixture of aniline and dry bichloride of mercury in a sealed tube for 30 hours, at a temperature of 356° Fahr. The mass when cold is dissolved in boiling water, and the colour precipitated by means of common salt. This operation is repeated until the blue is quite free from the green pigment which accompanies it.

A blue, called *Bleu de Mulhouse*, may be obtained by the process patented by MM. Gros-Renaud and Schœffer in 1861, and which consists in boiling a solution of *azaleine* (nitrate of rosaniline) with gum lac and carbonate of soda for some time; and another blue named *azuline*, has been produced by M. Marnas by a like treatment of a substance called *peonine*, with eight times its weight of aniline; and the residuum is purified with a succession of solvents, as water acidulated with muriatic or sulphuric acid, then hot naphtha, then caustic alkali, and finally with water acidulated with muriatic acid. The azuline, or blue colour, which remains, is soluble in spirit and forms a rich blue dye. Blues are also produced by the action of numerous oxidizing agents on aniline or its salts, as by a solution of hypochlorous acid (Hofmann), by a solution of chlorate of potash and muriatic acid (Fritzsche), by peroxide of hydrogen (Lauth), by perchloride of iron or red prussiate of potash (Kopp), by peroxide of manganese or pernitrate of iron and hydrochloric acid (Scheurer-Kestner), by bichromate of potash and acid (Willm) and I have obtained it by oxidising the sulphate of aniline by means of the oxygen disengaged at the positive pole of a battery. In all these cases the blue is very difficult of solution, for it resists the action of every solvent but strong sulphuric acid. Taking advantage of this, Mr. Nicholson, in 1862, patented a process for purifying the blue colouring matter, by dissolving it in concentrated sulphuric acid, and then heating it for half an hour at a temperature of 302° Fahr. By diluting it with water it is precipitated in a modified condition, for it is now soluble in pure water. Dr. Hofmann ascertained that it was a substitution compound of rosaniline, in which three equivalents of hydrogen had been substituted by three equivalents of a hydrocarbon called phenyl ($C_{12}H_5$); he therefore named it tryphenylic-rosaniline, and this suggested the possibility of substituting other hydrocarbons, as methyl (C_2H_3), ethyl (C_4H_5), amyl ($C_{10}H_{11}$), etc., in which he was successful by acting upon rosaniline with the iodides of these radicals, and thus producing ethylic, methylic, and amylic substitution compounds of a rich blue and purple colour, called Hofmann's blues. Very recently the change has been effected by a more direct process without the aid of the iodide, but by heating a mixture of aniline, muriatic acid, and methylic alcohol under pressure, and then treating with iodine and chlorate of potash, or other oxidizing agent.

Aniline Greens.

Most of the blue substances just described become green by the action of acids, and again acquire a blue colour when they are washed or treated with alkalies. It has also been noticed that in certain states of oxidation, aniline acquires a green tint; but all attempts to utilise this colour failed, until, in 1860, Messrs. Calvert, Clift, and Lowe patented the process for producing it upon the fabric. Their process was to prepare the fabric with chlorate of potash, and then to print upon it with acid muriate of aniline. In a few hours a beautiful bright green colour, called *emeraldine*, gradually appeared, and it was fixed by merely washing it with water. If a blue tint were required, the fabric was passed through a solution of bichromate of potash, when the oxidation of the aniline was carried still further, and a dark indigo blue, called *azurine*, was produced.

A green colour may also be obtained by heating a mixture of two parts fuchsine with three parts strong sulphuric acid and one part water. When the solution of the fuchsine is complete, it is allowed to cool, and four parts of aldehyde are added. The mixture is again heated until it is a bright blue colour without a trace of violet. It is then treated with a boiling solution of hyposulphite of soda, and filtered. The residue upon the filter is to be boiled in water, and filtered while hot. After standing 24 hours it deposits a green precipitate.

Aniline Black.

Several processes have been proposed for making a black dye from aniline, as by acting on aniline with an oxide of chlorine, and then with a salt of copper; but the colour is not of sufficient importance to command attention.

Aniline Yellow,

called *chrysaniline*, or *phosphine*. This colour was first obtained by Mr. Nicholson, in 1861. He procured it from the residuum of rosaniline by the action of steam, whereby a dirty yellow solution was obtained. On adding nitric acid to the solution, the yellow dye was thrown down as a nitrate of little solubility, and by decomposing it with an alkali the base is set free, which either alone or in a form of a soluble salt communicates a rich yellow colour to silk and wool.

These are the principal colours obtained from aniline, and it may be of interest to examine the leading properties of these remarkable compounds. At first you will have remarked that the bases of nearly all the aniline colours are very insoluble in water, ether, and coal naphtha. They are more soluble in water acidulated with the mineral acids, and are still more soluble in acetic acid. Alcohol, however, is the great solvent for them. You will likewise observe that they are generally precipitated from their saline solutions by alkalies and by common salt, and in this manner they are generally purified. Tannin also produces an insoluble compound with them, and thus they are often fixed upon vegetable fabrics. They are endowed with great power of resistance, for they will bear the action of strong sulphuric acid without undergoing decomposition, but they cannot resist the action of powerful oxidizing agents, as chlorine, chloride of lime, or nitric acid. Reducing agents, as sulphide of ammonium and protosulphate of iron, destroy their colour; but the action is not permanent, for on exposure to the air oxygen is absorbed, and the colour reappears.

The bases themselves are not generally coloured, but they acquire their characteristic tints when they combine with acids. I have here the colourless, or nearly colourless, solutions of rosaniline, mauvine, and aniline blue, and you will remark that directly I expose them to the vapours of an acid (acetic) their characteristic tints appear.

The tinctorial power of these dyes is remarkably great. If, for example, I put a little Magenta, mauve, or aniline blue upon paper, and then shake off the powder as completely as possible, there yet remains sufficient to give deep tints when I blow a fine spray of alcohol and acetic acid upon the paper.

The affinity of animal substances, as silk, wool, feathers, horn, ivory, leather, etc., is so great that the dye instantly combines with them, and produces a permanent stain. The affinity, indeed, is so great that, as you will here see, a piece of flannel will completely absorb and remove the colouring matter from its solution in water. Vegetable tissues, however, have no such affinity for the colour, and therefore processes must be adopted for fixing the dye upon cotton and linen fabrics. One of these processes is to prepare the fabric with some animal substance, as albumen, serum of blood, the caseine of milk, or the gluten of wheaten flour. Advantage is also taken of the power which tannin has of combining with the colour and rendering it insoluble. The process of Messrs. Puller and Perkin is to soak the cotton tissue in a decoction of shumach, or other tannin material, for an hour or two, and then in a solution of stannate of soda for another hour; after which it is dipped into dilute sulphuric acid, and is then ready for the dye. By these contrivances the aniline colours are made fast upon all kinds of vegetable fabrics.

Starch appears to have the power of fixing the colours, for if shaken with weak solutions of them it will absorb the colour, and by falling to the bottom of the liquid leave the solution colourless.

The rationale of the change which takes place during the formation of the several colours is not altogether clear, although there can be no doubt that the essential part of it is the oxidation of aniline; for, as I have already stated, when a salt of aniline is exposed to the action of nascent oxygen set free from the positive pole of a galvanic battery, the characteristic tints of aniline are successively and quickly produced. At first there is bright yellow, then green, blue, violet, and lastly red, as if these were the successive phases of oxidation. The researches of Dr. Hofmann have demonstrated that all the aniline reds are salts of a well defined base, which he has named *rosaniline*; and the more recent inquiries of MM. de Laire, Girard, and Chapoteaut have shown that there are four such bases entering into the composition of coal-tar colours, as *violaniline*, *mauvaniline*, *rosaniline*, and *chrysotoluidine*, which form an arithmetical series advancing by successive additions of C_2H_2, thus:—

Violaniline	$C_{36}H_{15}N_3$
Mauvaniline	$C_{38}H_{17}N_3$
Rosaniline	$C_{40}H_{19}N_3$
Chrysotoluidine	$C_{42}H_{21}N_4$

Each of these bodies is produced in the same manner, by the oxidation and removal of 6 atoms of hydrogen from 3 atoms of aniline, or 3 atoms of toluidine, or 3 atoms of the mixed bases, thus:—

6 atoms of hydrogen from 3 atoms of aniline produce *violaniline*.

$$3\underbrace{(C_{12}H_7N)}_{\text{Aniline.}} + H_6 = \underbrace{C_{36}H_{15}N_3}_{\text{Violaniline.}}$$

6 atoms of hydrogen from 2 atoms of aniline and 1 of toluidine produce *mauvaniline*.

$$2\underbrace{(C_{12}H_7N)}_{\text{Aniline.}} + \underbrace{C_{14}H_9N}_{\text{Toluidine.}} - H_6 = \underbrace{C_{38}H_{17}N_3}_{\text{Mauvaniline.}}$$

6 atoms of hydrogen from 1 atom of aniline and 2 of toluidine produce *rosaniline*.

$$\underbrace{C_{12}H_7N}_{\text{Aniline.}} + 2\underbrace{(C_{14}H_9N)}_{\text{Toluidine.}} - H_6 = \underbrace{C_{40}H_{19}N_3}_{\text{Rosaniline.}}$$

and 6 atoms of hydrogen from 3 atoms of toluidine produce *chrysotoluidine*.

$$3\underbrace{(C_{14}H_9N)}_{\text{Toluidine.}} + H_6 = \underbrace{C_{42}H_{21}N_3}_{\text{Chrysotoluidine.}}$$

These colour bases are perfectly homologous in all respects, for they not only unite with acids to form salts which crystallise very freely, and which have remarkable tinctorial power, but they also contain within them three atoms of typic hydrogen, which may be replaced by certain radicals, as of the alcohols, etc.—methyl, ethyl, phenyl, etc.—forming derivative compounds of like basic properties, and frequently of high tinctorial quality.

The best known of these bases is *rosaniline*, which in its anhydrous condition is represented by the formula

$$C_{40}H_{19}N_3,$$

but which always contains two atoms of water in the hydrated state in which it is set free from its compounds, thus:—

$$C_{40}H_{19}N_3,2HO$$

It is readily obtained by decomposing its salts—the aniline reds—with an excess of alkali, soda, or ammonia, and in this state it falls as a dirty yellow or brownish-yellow precipitate; but by careful purification it occurs as a colourless base, which quickly becomes rose-red on exposure to any acid, even the carbonic acid of the atmosphere. It is nearly insoluble in water, slightly so in ammonia, and very soluble in alcohol, forming a deep red solution. Ether and coal-tar naphtha have no solvent action upon it. It combines with one, two, or three equivalents of acid to form salts which crystallize very readily, the first of them, the mono-acid salts, being remarkable for their lustrous metallic or bronze-like appearance and their beautiful rose-red solutions; these, indeed, are the true colouring compounds, the most important of which are the following:—

Fuchsine, or muriate of rosaniline	$C_{40}H_{19}N_3,HCl$
Azaleine or *Magenta*, the nitrate	$C_{40}H_{19}N_3,HNO_6$
Roseine, the acetate	$C_{40}H_{19}N_3,HC_4H_3O_4$

It was the last-named salt which composed the splendid bronze-like crystals of the crown which were exhibited in 1862 by Mr. Nicholson. And, besides these, there are sulphate, arseniate, oxalate, chromate, tannate, etc., of rosaniline. Most of them are freely soluble in water and in spirit, but the tannate is so insoluble in water that it is used for fixing the colour upon calico, and for recovering the dye from very weak solutions. To this end the otherwise waste products of aniline red are treated with a fresh infusion of nutgalls, and in a short time the rosaniline is precipitated in the form of a magnificent red lake of tannate of rosaniline, leaving the solution quite colourless. This lake is soluble in spirit and in acetic acid, and may be thus used for dyeing.

The salts of rosaniline with two equivalents of acid have not been studied, and even those with three of acid are not of any technical value.

Under the influence of reducing agents, as sulphide of ammonium, or the nascent hydrogen evolved from zinc when a solution of rosaniline in muriatic acid is left in contact with the metal, it is rapidly decolorized, and is transformed into a new base, which Dr. Hofmann has named *leucaniline*. This is effected by the absorption of two atoms of hydrogen, thus:—

$$\underbrace{C_{40}H_{19}N_3}_{\text{Rosaniline.}} + 2H = \underbrace{C_{40}H_{21}N_3}_{\text{Leucaniline.}}$$

The new base occurs in the form of colourless acicular crystals, which are scarcely at all soluble in water, but freely so in alcohol. The salts of it are also colourless, or dazzling white, although they re-acquire the red tint of rosaniline when their solutions are exposed to the action of oxidizing agents or even to the air.

Dr. Hofmann has ascertained that there is still another base derivable from, or closely related to, rosaniline—viz., *chrysaniline*. It is procured from the residual, or waste product of rosaniline, by the action of steam and nitric acid, as I have already described. It contains two atoms less of hydrogen than rosaniline, and therefore it stands in its relation to this base as rosaniline does to leucaniline, thus:—

Chrysaniline .... $C_{40}H_{17}N_3$
Rosaniline .... $C_{40}H_{19}N_3$
Leucaniline .... $C_{40}H_{21}N_3$

It is very soluble in water, and it forms yellow salts with acids, one of which, the nitrate, is a very soluble compound. The solutions of the base and of its salts communicate a splendid golden yellow colour to animal tissues.

Aniline blues are, for the most part, substitution compounds of aniline red, the three atoms of typic hydrogen being replaced by three of an organic radical. The blue, for example, which is produced by the action of aniline on a salt of rosaniline, is a compound in which the three atoms of hydrogen are replaced by three of phenyl, thus:—

Aniline red, or *Rosaniline*............ $C_{40}H_{19}N_3$
Aniline blue, Triphenylic rosaniline $C_{40}\left\{\begin{matrix} H_{16} \\ (C_{12}H_5)_3 \end{matrix}\right\}N_3$

And its production when aniline is heated with a salt of rosaniline is accompanied with the evolution of ammonia, disregarding the acid of the compound, thus:—

$$C_{40}H_{19}N_3 + 3C_{12}H_7N = C_{40}\left\{\begin{matrix} H_{16} \\ (C_{12}H_5)_3 \end{matrix}\right\}N_3 + 3NH_3$$

Other substitution compounds, in which the three atoms of hydrogen are replaced by three of methyl, ethyl, amyl, etc., have been produced by Dr. Hofmann by the action of the iodides of these radicals on the salts of rosaniline, or even by the more simple and direct process of heating them with the alcohols of the radicals under pressure. All these compounds are basic in their character, and they mostly form, with one equivalent of an acid, the blue colours which are known as Hofmann's blue and violet, and the violet of Paris.

The other bases of aniline and toluidine colours have not been so well studied, but it is very probable that the reactions and the general properties of *violaniline*, *mauvaniline*, and *chrysotoluidine*, are very similar to the preceding, and that they are capable of forming the like reduction and substitution bases.

ON THE ANALYSIS OF CAST IRON.

BY EDMUND G. TOSH, PH.D.

Before proceeding with the regular analysis of cast iron, I have examined some of the more important processes for the estimation of the numerous substances which make up its constitution. The analysis of cast iron is one of the more difficult operations of analytical chemistry, and to ensure accuracy many special precautions and much extra manipulation are necessary—not because the ingredients are of themselves difficult of determination, but because the quantity of iron in every case preponderates so largely over that of the other elements.

In comparing the merits of the various processes one specimen of iron was used throughout.

Estimation of Carbon.—The estimation of this element as it occurs in iron is a problem which has engaged the attention of many celebrated chemists. Connected with the literature of the subject we find the names of Berzelius, Karsten, Wöhler, Gay-Lussac, Regnault, Caron, and numerous others, but notwithstanding the labour which has been expended in this direction, we have not arrived at a method which does not necessitate a large amount of time and work for its satisfactory accomplishment.

a. Regnault's method,[*] with the modifications of Bromeis,[†] is thus carried out. About two inches of a combustion tube of hard Bohemian glass, closed at one end, are filled with a mixture of equal parts of chromate of lead and chlorate of potash. 3 grms. of the iron under examination, in a state of very fine division, are intimately incorporated with 50 grms. of a mixture of 40 parts of chromate of lead, and 6 parts of previously fused chlorate of potash, and introduced into the combustion tube, and lastly a layer of chromate of lead. To the tube a chloride of calcium and a Leibig's potash apparatus are attached; the former to retain traces of moisture, the latter to absorb the carbonic acid formed. The combustion tube is cautiously heated, first near the open end as in the conduction of organic analysis. When the mixture of the iron with the lead salt is brought to a dull red heat, the metal burns with incandescence, and the carbon is oxidised to carbonic acid, which is absorbed by the potash solution. At the close of the operation the mixture at the extreme end of the tube is heated, oxygen is evolved, all carbonic acid is driven forward, and the last traces of carbon consumed. From the increase of weight of the potash apparatus, due to carbonic acid, the amount of carbon may be calculated. In this way Regnault obtained very concordant results, which were afterwards confirmed by the experiments of Bromeis. I made two estimations by this process, and the results agree well with one another.

1. 3·240 grms. of iron gave 0·462 grm. CO_2 equal to 3·886 per cent. of carbon.
2. 3·8245 grms. of iron gave 0·5605 grm. CO_2 equal to 3·996 per cent. of carbon.

I have reason to think, however, that the percentage of carbon indicated, is somewhat too low in both cases, on account of loss of carbon during pulverisation of the iron. This loss, as pointed out by Morfit and Booth,[‡] is often appreciable, and in the case of highly graphitic iron very considerable. With this one exception the process is in every respect commendable, and where, as with spiegeleisen or white iron, this loss of carbon cannot take place, it strongly recommends itself. When instead of a mixture of chromate of lead and chlorate of potash, chromate of lead is used alone, no very reliable results can be obtained, and invariably the amount of carbon shown by this method is too small.

b. Fresenius's Method.[§]—A weighed portion of the metal, in borings or chippings, is dissolved in dilute sulphuric acid by the aid of heat. The gases evolved

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[*] *Ann. d. Chem. u. Pharm.* xxx. p. 352.

[†] *Ann. d. Chem. u. Pharm.* xliii. p. 241.

[‡] *Chemical Gazette*, vol. xi.

[§] *Zeit. Anal. Chem.* iv. 69.

during solution, consisting mostly of hydrogen, are passed over red-hot oxide of copper. The gaseous hydrocarbons are burned, and the carbonic acid formed, after drying by chloride of calcium, is absorbed by potash solution in a Leibig's apparatus, and thus weighed. Fresenius states that in cases where the percentage of combined carbon is very low, this process may be used for its direct estimation. Where an estimation of the total carbon is required, the matter insoluble in the dilute sulphuric acid, remaining behind in the flask, is collected and burned in a stream of oxygen, and from the weight of the resulting carbonic acid, the amount of carbon may be deduced. This quantity, added to that obtained by burning the gases over oxyde of copper, gives the total quantity of carbon contained in the iron. In drying the insoluble residue previous to combustion in oxygen, an elevated temperature must be carefully avoided, as I have occasionally noticed that at a temperature of about 100° C., this insoluble matter gives off a strong-smelling, disagreeable vapour, in all probability a volatile hydrocarbon, formed by the contact of hydrogen and carbon in the nascent state. At a temperature of about 80° C. the odour evolved is very slight, and the loss inconsiderable, but the safest way is to dry over sulphuric acid. The presence of hydrocarbons in the graphitic residue, at once shows that this process could not be safely applied in the present case for the estimation of combined carbon directly. The following experiments illustrate the same point.

1. 1·42425 grms. iron dissolved in dilute sulphuric acid, and gases evolved passed over oxide of copper, gave 0·2525 grm. CO_2 = 0·4834 per cent. carbon.
2. 1·62425 grm. iron treated in the same manner gave 0·01625 grm. CO_2 = 0·2728 per cent. carbon.

The insoluble residues in both cases were collected and burned in oxygen.

From 1 gave 0.1964 grm. CO_2 = 3·7612 + 0·4834 = 4·2446 per cent. carbon.
From 2 gave 0·2319 grm. CO_2 = 3·894 + 0·2728 = 4·1168 per cent. carbon.

The deficiency in the amount of carbon in the gases in No. 2 is made up by the larger quantity in the insoluble matter.

This method requires a large amount of time for its execution, the apparatus is somewhat complicated, and the great number of operations which the carbon must go through render the exercise of extreme care more than usually necessary.

I would here remark, that in all my experiments I found the perfect combustion of graphite, even in oxygen, required a very high temperature. In my first trials, I sought to burn graphite at a dull red heat in a tube of hard Bohemian glass, but it remained almost unaffected in a stream of oxygen. I found it most convenient to place the graphite first in a platinum boat, insert this into a well-glazed porcelain tube, and expose to a full red heat in a small charcoal furnace. In a gentle stream of oxygen the carbon is perfectly burned in a few minutes, and the resulting carbonic acid is absorbed in the usual way by potash solution.

c. Wöhler's Chlorine Process.—This most excellent process is carried out in the following way. A weighed quantity of iron contained in a porcelain boat, is placed in a hard glass tube, and is exposed at a dull red heat, to the action of chlorine (first dried by passing over pumice stone saturated with sulphuric acid) till no more perchloride of iron is formed. The whole of the carbon remains in the boat, which, when cool, is transferred into a porcelain tube, and the carbon burned in oxygen as before described. An estimation by this method may be performed in two hours. Care must be taken to have the chlorine perfectly free from moisture, otherwise a portion of carbon may be lost by the formation of hydrocarbons. The results given by this process are very concordant, as the three experiments given beneath show.

1. 1·001 grms. of metal in borings gave 0·1595 grm. of carbonic acid = 4·348 per cent. of carbon.
2. 1·06775 grms. of metal gave 0·171 grm. CO_2 = 4·357 per cent. carbon.
3. 1·002 grms. of metal gave 0·159 grm. CO_2 = 4·327 per cent. carbon.

According to Max Buchner,* this process affords results as accurate as those obtained by Berzelius' chloride of copper method; and Professor Kerl of Clausthal states that in his laboratory it is employed almost exclusively.

d. Weyl's Galvanic Method.†—This very ingenious and beautiful method for the estimation of carbon is founded upon the fact that a piece of iron, attached to the positive pole of a galvanic battery, and suspended in hydrochloric acid, is dissolved, while the hydrogen is given off at the negative pole, dipping into the solution. Carbon and hydrogen do not thus come in contact in the nascent condition, and the formation of hydrocarbons, and consequent loss, is prevented. A recommendation of this method is that the iron does not require to be in powder.

A piece of iron 2 to 4 grammes in weight, attached to the positive pole of a Bunsen's cell, is suspended in dilute hydrochloric acid, just below the surface of the liquid. From the negative pole hydrogen passes off, while the iron dissolves quite quietly, and the strong solution of protochloride of iron formed may be seen falling in a regular steam through the lighter liquid. The iron is dissolved in about 24 hours, and the carbon is left behind in the same shape as the piece of metal from which it was derived.

In Weyl's earlier experiments it was found that some of the liberated carbon at the positive pole was carried over to the negative pole by the mechanical working of the stream. To prevent this, a diaphragm of bladder or parchment paper is interposed between the two, which entirely obviates the possibility of loss in this way.‡

Weyl recommends that the piece of iron should be suspended by means of platina pointed forceps, in such a manner that the acid does not reach the iron at its point of contact with the forceps, otherwise after the partial solution of the iron, the separated carbon intervening between the forcep points and the remaining piece of metal, would interrupt the galvanic current and the experiment would be lost. In my researches I used a small platinum sieve, kindly lent me by Professor Wöhler, in which I laid the pieces of iron, wholly immersed in the acid, and the action proceeded till the end as well as it did at the commencement. This interruption of the current is not, I think, to be feared, as both amorphous carbon and graphite are good conductors of electricity.

Schnitzler § when examining this process always found that a small quantity of hydrogen was given off

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* *Berg-u. Huttn-Zeitung. Jahrg.* 24 No. 10. p. 84.
† *Pogg. Ann.* Bd. iii. p. 507.
‡ *Pogg. Ann.* Bd. 126. p. 617.
§ *Zeit. Anal. Chem.* b. iv. p. 78.

from the piece of iron during solution, and attributed this to the action of acid on small particles of metal which were unconnected with the main piece, distributed through the surrounding carbon, and beyond the influence of the galvanic stream. If we look upon carbon as a conductor of electricity this theory does not hold good. I noticed this evolution of gas in all my experiments, however dilute the acid. Immediately on dipping the piece of iron into the acid, gas bubbles formed on its surface, showing that the action had commenced, which, for my own part, I am inclined to attribute to a secondary and independent galvanic action between the iron and free carbon, either previously existing throughout the metal, or liberated the moment solution begins.

This evolved hydrogen possesses the characteristic odour due to the presence of hydrocarbons, always noticeable when cast iron is dissolved in acids under ordinary circumstances. As shown by Schnitzler, a loss of carbon consequently ensues, and my estimations lead to the same conclusion.

When no more hydrogen is given off at the negative electrode, showing that all the iron is dissolved, the carbon is collected in a small funnel stopped with asbestos, dried cautiously, transferred to a platinum boat, and burned in a stream of oxygen, and the resulting carbonic acid is absorbed in the ordinary way by potash solution. Subjoined are the results of two experiments:—

1. A piece of iron weighing 3·59125 grms. gave 0·5575 grm. CO_2=4·235 per cent. of carbon.
2. 2·04075 grms. of iron gave 0·30575 grm. CO_2=4·086 per cent. of carbon.

Weyl* has also proposed a second method for the solution of iron without the evolution of hydrogen, which consists in suspending a piece of the metal in dilute sulphuric acid, containing bichromate of potash dissolved. The carbon is unaffected, and when most of the iron is removed, the residue may be collected and burned in oxygen. I have made no estimation by this means.

Of these various methods I selected that of Wöhler for the determination of carbon in the specimens of iron I have examined, because while recording results of equal reliability with any, its performance requires much less time.

Arranged below in a tabular form are the amounts of carbon per cent., indicated by the various methods, in the same sample of iron.

	Carbon. Per cent.
1. Regnault's combustion process.........	3·886
2. “ “	3·996
3. Fresenius' method....................	4·244
4. “ “	4·166
5. Wöhler's chlorine process..............	4·348
6. “ “ “	4·367
7. “ “ “	4·327
8. Weyl's galvanic method...............	4·235
9. “ “ “	4·086

NOTE ON THE CRYSTALLISATION AND THE SOLUBILITY OF PLUMBIC CHLORIDE.

BY J. CARTER BELL, F.C.S.

The various manuals and dictionaries of chemistry when speaking of plumbic chloride, fail to say anything on the phenomenon of its crystallisation; it is generally written that "it crystallises in brilliant needles.". This state of crystallisation only occurs under certain conditions, for if pure plumbic chloride be taken and dissolved in boiling distilled water, and then allowed to cool, the chloride does not crystallise out "in brilliant needles," but in a sort of cuneiform or arrow-shaped crystals, which are not white but of a delicate cream colour, and in the many experiments I have performed, I have failed in producing white brilliant needles by this method. But if the solution contains free hydrochloric acid, then we obtain white needle-shaped crystals; and according to the amount of HCl present in the mother liquor, so will the crystals vary in size, colour, and shape. With a small percentage of HCl, the crystals are white, and from 10 to 20 millimètres in length; if the HCl increases, very small white needles are obtained, and if it is strong HCl of 1·116 specific gravity, the crystals are no longer needle-shaped, but are of the form of the base of the right rhombic prism.

The Solubility of Plumbic Chloride.—A pure saturated solution of plumbic chloride at 16·5° Centigrade, contains ·9414 per cent. of the chloride, being the mean of three experiments; the addition of HCl diminishes the solubility considerably, and up to a certain point, which I have not yet determined, it may be stated that as the HCl increases the solubility decreases.

	Per-centage of $PbCl_2$
Water containing 1 per cent. of HCl (sp. gr. 1·116) at 16·5°C. only holds in solution	·3470
“ “ 2 “ “ “ “	·2013
“ “ 3 “ “ “ “	·1656
“ “ 4 “ “ “ “	·1459
“ “ 5 “ “ “ “	·1310
“ “ 6 “ “ “ “	·1078
“ “ 7 “ “ “ “	·1007
“ “ 8 “ “ “ “	·0991
“ “ 9 “ “ “ “	·0968
“ “ 10 “ “ “ “	·0931

A saturated solution of plumbic chloride in hydrochloric acid of specific gravity 1·116 at 16·5°C., contains 2·566 per cent. of $PbCl_2$, but on the addition of water nearly the whole of the chloride is precipitated. These experiments seem to point that there must be a minimum hydrochloric solution of plumbic chloride, and also a maximum. Future experiments which I shall make will perhaps decide.

Manchester, July 29, 1867.

ON THE ACTION OF CHLORINE ON CARBONATE OF SILVER. PREPARATION OF CHLORATE OF SILVER.

BY PROFESSOR J. S. STAS.

If oxide or carbonate of silver suspended in water is diffused through an excess of saturated chlorine water, the silver is completely changed into chloride, as in the case of oxide and carbonate of mercury; and the water only contains besides the excess of chlorine pure hypochlorous acid, without a trace of chloric or perchloric acid. The chlorometric standard of the liquid, after the chlorine has acted on the carbonate, is almost identical with that of the chlorine water employed.

By passing a slow current of chlorine, *with constant agitation*, into water containing an excess of carbonate of silver in suspension, the first action is identical;

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* *Pogg. Ann.* Bd. cxxvi. p. 617.

there are still produced chloride of silver and hypochlorous acid, but this hypochlorous acid only remains momentarily free, it slowly transforms another part of the carbonate into metallic hypochlorite. Indeed, if at the end of a short time the current of chlorine is interrupted, the agitation being continued all the time, the liquid loses the characteristic odour of hypochlorous acid, but preserves its energetic decolorising power because the hypochlorite of silver which is formed is very soluble in water.

Hypochlorite of silver, which to my knowledge has never before been described, is sufficiently stable in the presence of an excess of carbonate of silver, to remain undecomposed for several days; it is, on the contrary, very instable in the absence of this metallic oxide or carbonate. It has, indeed, appeared to me that so long as the solution of hypochlorite of silver is kept agitated with the carbonate the liquid preserves its transparency and decolorising power; if, on the contrary, it is left at rest, scarcely has the carbonate of silver settled when the limpid liquid becomes opalescent, and soon deposits large flakes of chloride of silver, which cover with a white coating the carbonate of silver at first deposited. The liquid at the same loses its decolorising power, and only contains chlorate of silver in solution, rendered alkaline by a small excess of carbonate dissolved.

According to what I have here shown, it is evident that the chlorate of silver by the action of chlorine on the carbonate, is the result of a secondary reaction of the hypochlorite of that metal which is previously formed. It would appear, moreover, as if all other chlorates were formed in a similar manner, but this is not the place to enter into this question.

The successive reactions may be represented by the following equations:—

$$12Cl + 3Ag_2O + 3H_2O = 6AgCl + 6HClO$$
$$6HClO + 3Ag_2O = 3H_2O + 6AgClO$$
$$6AgClO = 4AgCl + 2AgClO_3$$

I may say that argentic hypochlorite is very soluble in water; indeed, the clear liquid containing carbonate of silver suspended in it, and through which a slow current of chlorine has been passing for some hours, contains a considerable quantity of hypochlorite, which remains intact so long as it is in contact with the carbonate; but the excess of carbonate employed fixes on itself a large quantity of this hypochlorite, or, at all events, of the elements of this salt. The fixation of hypochlorite on this very slightly soluble argentic compound, results from two facts observed during the four times I have produced chlorate of silver by the action of chlorine on the carbonate and oxide. The first is the impossibility of washing these bodies after chlorine has acted on them for a certain time. Whatever care is taken, and however the washing is effected, the water always contains, besides the carbonate, a salt of silver containing chlorine and oxygen; the second fact is, that the carbonate on which chlorine has acted for a sufficient time to produce hypochlorite in solution, will still, after washing, furnish, under the influence of chlorine, a fresh quantity of hypochlorite, much more considerable than that which could result from the chlorine used in this second reaction. Observation has even proved that the greatest production of soluble hypochlorite takes place when two-thirds of the carbonate have been already submitted to the decomposing action of the chlorine.

Hypochlorite of silver is the sole silver salt that is formed by the action of chlorine on *an excess* of oxide or carbonate of silver suspended in the liquid and kept in a state of continual agitation. Spontaneous decomposition, or decomposition effected by the aid of heat, never produces the least trace of perchlorate when I have operated on a hypochlorite rendered slightly alkaline by an excess of carbonate in solution. During the passage of the chlorine, the hypochlorite which is formed may be destroyed again with formation of chloride of silver; but hypochlorous acid is still formed under these circumstances, and by reacting on a fresh quantity of carbonate of silver, a double quantity of hypochlorite is reproduced.

The conditions necessary to form chlorate of silver, according to the above observations, are therefore these:—A *slow* current of chlorine must react on the carbonate of silver (previously treated with chlorine to remove the alkali which it may contain) suspended in water and kept constantly in agitation until the chlorine has attacked the greater portion of the argentic compound employed; this agitation must be continued after the interruption of the current of chlorine, so as to change the free hypochlorous acid existing in the liquid into hypochlorite; the solution of argentic hypochlorite must be separated from the excess of the argentic compound employed in the first instance, so that the hypochlorite may change spontaneously into chloride and chlorate. The following is a description of the arrangement I adopted, so as to satisfy, as far as possible, the above conditions:—

Three kilogrammes and 935 grammes of nitrate of silver, free from foreign metals, were dissolved in twenty litres of distilled water; the solution was poured in small quantities at a time, into an equal volume of solution of carbonate of potassium prepared from pure cream of tartar. The carbonate of silver, which was voluminous, and of a very pale yellowish white, after having been kept for a long time suspended in an excess of solution of carbonate of potassium, was washed by decantation in the cold until no more potassium could be detected in the residue left by the washing water after evaporation to dryness. To arrive at this point I was obliged each time to violently shake up the carbonate of silver with water in a closed flask, as is done in a silver assay, to clarify the liquid. Taking these precautions, the washings lasted for fifteen days, repeating them several times a day.

The thin paste of carbonate of silver (which from its original yellowish white, had become of a beautiful yellow colour) was introduced into a flask of 45 litres capacity, covered with black cloth. This flask was fixed firmly in a frame attached to a kind of stirrup suspended above the ground by long cords. At each side, and at the foot of this stirrup, a string was fixed, and by alternately exerting a tractive movement by these strings, as strong an oscillatory movement as is desired could be communicated to the flask. To the neck of the flask was affixed a glass stopper pierced with two holes, one of which allowed the passage of a glass tube bent at right angles and leading the chlorine into the liquid, and the other admitted a glass tube, likewise bent, which allowed the escape of the carbonic anhydride set at liberty by the decomposition of the carbonate of silver. The tube by which chlorine was passed into the liquid was connected with a chlorine generating apparatus, by means of a vulcanised caoutchouc tube, long enough to permit free oscillation of the apparatus without exerting traction on the chlorine apparatus. The vulcanised caoutchouc tube had been boiled for an hour with a ten per cent. solution of hydrate of sodium to desulphurise it, and was then washed with pure water.

The apparatus being so arranged I allowed a *very slow* current of chlorine* to pass into the flask for an hour and a quarter, keeping it during the whole of this time in continuous movement. I then stopped the current of chlorine and continued to agitate the liquid so long as it exhaled the least odour of hypochlorous acid. The apparatus was then left at rest and the strongly coloured supernatant liquid was decanted. The carbonate of silver was then washed again by decantation, the first liquid decanted being added to the solution of hypochlorite already separated. The carbonate of silver was washed as long as it was possible to ascertain by aid of the spectroscope any traces of potassium in the residue left after evaporating the washing waters to dryness.

The solution of hypochlorite of silver, after having been preserved in darkness until it ceased to deposit chloride of silver, was evaporated to dryness; it left 31·819 grammes of a white saline residue.

The washed carbonate of silver, in the form of thin paste, was now introduced, with four litres of water, into the flask, and exposed for two hours to a current of chlorine, the agitation being incessant the whole time, and being kept up after the current of chlorine was stopped so long as the mixture smelt of hypochlorous acid. It furnished by decantation five litres of a strongly decolorising solution, which, left in darkness to spontaneous decomposition, yielded on evaporation 58·237 grammes of chlorate of silver.

The carbonate of silver was now washed for a third time. Already in the first washing water it was impossible for me to discover the least trace of potassium, by the aid of spectrum analysis, on examining the residue left by a whole litre of liquid.

I then diffused the carbonate of silver in a volume of water equal to that of the liquid decanted off, and for three hours exposed it to an uninterrupted current of chlorine with continual agitation. After the current of chlorine was stopped the mixture was kept in agitation for half-an-hour; after decantation the clear and colourless liquid did not emit the least odour of hypochlorous acid, but it had strong bleaching properties. On standing it deposited large quantities of chloride of silver, and on evaporation yielded 72 grammes of chlorate of silver.

Finally, the carbonate of silver, mixed with a considerable quantity of chloride of silver, was washed for a fourth time. Diffused through six litres of pure water it was exposed for six hours in the vibrating flask to a continuous current of chlorine. The hypochlorous acid having been changed into hypochlorite of silver by agitating the liquid with the remaining excess of carbonate of silver, I left the liquid to repose till all the hypochlorite was changed into chlorate and chloride. On evaporating the clear liquid with the usual precautions, I obtained in this operation 23 grammes of chlorate of silver.

I ceased acting with chlorine on the carbonate of silver because the salt was so whitened by admixture with chloride of silver that it was impossible by sight alone to distinguish whether any were present or not. Besides, when I stopped the operation the evaporation of all the liquids was not finished sufficiently for me to know how much chlorate was formed. On the supposition that the chlorine had acted to its fullest extent, I ought to have obtained 700 grammes of chlorate, but the total weight of the salt left on evaporating the four decanted liquids only came to 285 grammes. I had only produced two-fifths of the theoretical quantity, and it was therefore impossible to have obtained any perchlorate of silver.

The chlorates obtained in these operations were submitted to the following treatment:—

A portion of the 31·819 grammes obtained in the first operation having been examined in the spectroscope, was found to contain much chlorate of potassium. I did not therefore purify it.

The 58·237 grammes of chlorate from the second operation were dissolved in cold water. The solution was found to be faintly alkaline. Dilute chlorine water was added to it so as to cause the alkaline reaction to disappear, and it was then boiled in order to precipitate the traces of chloride of silver thereby produced. The perfectly neutral liquid was decanted and evaporated. Three separate crops of crystals were obtained from it. Each of these was separately reduced to the state of chloride by means of sulphurous anhydride.

A current of sulphurous anhydride precipitates sulphite of silver from a solution of chlorate, and the liquid contains chloric acid. This sulphite of silver only passes to the state of chloride by the consecutive action of sulphurous anhydride on chloric acid. During the passage of the gas there is, however, no way of telling whether the reduction of the chloric acid is partial or total, whether there is too little or an excess of sulphurous acid, and the amount of the excess. Another difficulty is occasioned by the relative slowness with which sulphurous acid reduces at 0°C. very dilute chloric acid; and, notwithstanding this, a low temperature and a great dilution of the liquid are indispensable conditions of success. I have, therefore, effected the reductions of chlorate of silver by means of a standard solution of sulphurous acid.

I now return to the three portions of chlorate of silver obtained from the 58·237 grammes of the second operation. The first portion containing the least soluble salts, still contained potassium, as shown by spectrum analysis. The second portion I lost in an attempt to reduce it in a current of sulphurous anhydride gas; the third part, weighing 23·932 grammes, was reduced by a standard solution of sulphurous acid at 0°C.

The 72 grammes of chlorate of silver produced in the third operation were dissolved in cold water. The solution was opalescent, and appreciably alkaline. I added carefully, dilute chlorine water till the alkaline reaction disappeared, then heated to precipitate the chloride of silver formed, and evaporated the decanted liquid over a water bath till a pellicle was formed. In order to obtain the chlorate in small crystals the liquid was cooled suddenly. The mother liquor was removed by aspiration. The chlorate was washed three times with ice cold water. After drying over sulphuric acid there remained 40·336 grammes of chlorate of silver, white and unalterable in the light. The washing waters and mother liquors yielded 27·581 grammes of salt equally white and unalterable in the light, but extraordinarily changeable by exposure to the air.

These 40·336 grammes of chlorate were added to the salt obtained from the 123·500 grammes yielded by the fourth operation. In order to free it from the perchlorate which, in spite of every precaution it

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* The binoxide of manganese employed in the preparation of chlorine, after being finely powdered, was treated with dilute. boiling sulphuric acid so as to free it from the nitro-compounds which it always contains; it was then washed in pure water. This treatment is indispensable when chlorine is required absolutely free from chloride of azotyle.

might contain, I submitted it to the following treatment:—

It was dissolved in cold water, and the solution was mixed with dilute chlorine water until the very slight alkaline reaction which it at first possessed, had disappeared. The liquid was then boiled to precipitate the traces of chloride of silver thus produced. The clear liquid was then decanted, evaporated over a water-bath till a pellicle formed, and cooled quickly. The mother liquor was removed by aspiration, and the crystals washed with ice-cold water; all the washings except the first, were evaporared, till a pellicle formed, and the same treatment pursued in their case. After repeating the crystallisations, the washings, and evaporations, I succeeded in obtaining from the 123·5 grammes originally taken about 99 grammes of chlorate of silver, which I consider is as pure as can possibly be obtained. The mother liquors, which I carefully kept separated, yielded pure chlorate to the last trace. The preparation of chlorate of silver in the manner above described, I consider to be the most laborious and delicate operation which can be performed in analytical researches.

TECHNICAL EDUCATION.

The subject of Technical Education has recently attracted attention in this country, owing to the evidence considered to be afforded by the International Exhibition at Paris, of the inferior rate of progress recently made in manufacturing and mechanical industry in England, compared with that made in other European countries. It has been stated that this alleged inferiority is due in a great measure to the want of technical education, and steps have, therefore, been taken to ascertain from many eminent English jurors whether they agree with this opinion.

We have been favoured with an early correspondence which has taken place on this subject, and we think we shall be doing good service to the cause of education by laying an abstract of some of the letters before the readers of the Chemical News. The inquiry originated in a letter from Dr. Lyon Playfair to Lord Taunton, Chairman of the Schools Inquiry Commission. In it he states that, with very few exceptions, a singular accordance of opinion prevails that our country has shown little inventiveness, and made but little progress in the peaceful arts of industry since 1862. When he found some of our chief mechanical and civil engineers lamenting the want of progress in their industries, and pointing to the wonderful advances which other nations are making; when he found our chemical, and even textile, manufacturers uttering similar complaints, he naturally devoted attention to elicit their views as to the causes. So far as could be gathered by conversation, the one cause upon which there was most unanimity of conviction was that France, Prussia, Austria, Belgium, and Switzerland possess good systems of industrial education for the masters and managers of factories and workshops, and that England possesses none. A second cause was also generally, though not so universally admitted, that we had suffered from the want of cordiality between the employers of labour and workmen, engendered by the numerous strikes, and more particularly by that rule of many trades' unions, that men shall work upon an average ability, without giving free scope to the skill and ability which they may individually possess. Dumas asserts that technical education had given a great impulse to the industry of France. In going through the Exhibition, whenever anything excellent in French manufacture struck his attention, his invariable question was, "Was the manager of this establishment a pupil of the *Ecole Centrale des Arts et Manufactures?*" and in the great majority of cases he received a reply in the affirmative. General Morin, so well known as the director of the *Conservatoire des Arts et Metiers*, has lately sat on a commission to examine into the state of technical education in other countries, and to extend it in France, and their recommendations are likely to be promptly and largely acted upon. General Morin was of opinion that the best system for the technical education of *workmen* is to be found in Austria, though the higher instruction of masters and managers is better illustrated in France, Prussia, and Switzerland.

In 1853 Dr. Playfair published a little work on "Industrial Education on the Continent," in which he pointed out that as an inevitable result of the attention given to it abroad, and its neglect in England, other nations must advance in industry at a much greater rate than our own country. It is feared that this result is already attained for many of our staple industries, and the writer concludes his eloquently written letter by urging upon the Government to hold an official inquiry on this subject, and tell the people of England authoritatively what are the means by which the great States are attaining an intellectual pre-eminence among the industrial classes, and how they are making this to bear on the rapid progress of their national industries.

This letter was considered so important that copies of it were sent to a great number of eminent English jurors, with a request that they would communicate their views on the subject.

We extract from the numerous answers the following opinions, which more particularly bear on chemical and physical education:—

Professor Tyndall, F.R.S., expresses a general concurrence in the views of Dr. Playfair. The facilities for scientific education are far greater on the Continent than in England, and where such differences exist, England is sure to fall behind as regards those industries into which the scientific element enters.

He has long entertained the opinion, that in virtue of the better education provided by continental nations, England must one day—and that no distant one—find herself outstripped by those nations, both in the arts of peace and war. As sure as knowledge is power this must be the result.

Dr. Frankland, F.R.S., says that Dr. Playfair's communication substantially expresses his own convictions in regard to the matters therein mentioned. As a juror in class 44 of the present Paris Exhibition, he was not only forcibly struck by the want of evidence of progress in the different branches of chemical manufactures carried on in Great Britain, but still more so at the great advances made by other nations, more especially by Germany, France, and Switzerland, in respect of such manufactures since the year 1862, when, as a juror in the corresponding class, he had also an opportunity of comparing the chemical manufactures of different nations. He refers this want of progress in the manufactures of this country chiefly to the almost utter lack of a good preparatory education for those destined to take part in industrial pursuits. This great defect in the school and college education of England affects the masters and managers of our factories even more deeply than the workmen themselves. The former have but rarely had any opportunities of making themselves acquainted with the fundamental laws and principles of physics and chemistry; they therefore find themselves engaged in pursuits for which their previous education has afforded them no preparation, and hence their inability to originate inventions and improvements. It is true that such men not unfrequently imagine themselves inventors, and the yearly files of patent specifications abound with instances of their so-called inventions. The great loss of time and money attending these futile patents would be rendered impossible by a very moderate, if accurate, knowledge of chemical and physical science.

In the polytechnic schools of Germany and Switzerland the future manufacturer or manager is made familiar with those laws and applications of the great natural forces which must always form the basis of every intelligent and progressive industry. It seems that at length this superiority in previous training is more than counterbalancing the undoubted advantages which this country possesses in raw material.

DR. DAVID S. PRICE considers that as far as relates to chemical products, the exhibition made by Great Britain is a "deficient representation," and will not enable foreigners to form a correct estimate of the nature and extent of chemical manufactures now carried on in this country; what is shown in class 44 is, as a rule, injudiciously exhibited, contrasting painfuly with the taste and spirit evinced by the French in their arrangements in the same class.

The writer's conviction is that it is most important that these international competitions should not be allowed to degenerate into a means for advertising, and that it behoves those who are intrusted with their organisation to see that the several departments of industry are intrusted to men who take an active interest in them, and are thus a guarantee that every endeavour will be made to have them fairly and properly represented, which is not the case on the present occasion, so far at least as refers to classes 40 and 44.

He does not agree with Dr. Playfair that the technical education of working men is the most important method for the maintenance of our industrial supremacy. The information gleaned by acting upon his suggestion would be instructive, and great good would result from its application; but what is really wanted for this country, and is of vital consequence to our future prosperity, is a higher scientific culture of those who are likely, in the natural course of events, to be master manufacturers, so that when discoveries are made they may fructify and not stagnate or decay, as has too often been the case, for want of intelligence on the part of those who command capital and works to perceive their merits; and that they, the manufacturers, may be able to appreciate and adequately remunerate the scientific talent that this country is, and always will be, able to afford them. No reformation bearing upon industrial progress is more required than in the Legislature, and it is a reproach to the country that science is not represented in Parliament.

It would be well if an investigation were made as to what have been the results of the teachings in science of the German universities; what Liebig has done for modern chemistry, and how the system inaugurated by him at the small University of Giessen has spread throughout the world, and what benefits have resulted from it; what we owe to the teachings of other chemists, the physicists, metallurgists, and geologists of those excellent seats of learning. Whilst advocating the necessity for the dissemination of scientific training in England, Dr. Price does not omit to bestow a passing tribute of commendation to the success of those institutions of recent date which were established to supply a want that existed many years since;—the Royal College of Chemistry, of which the late Prince Consort was the President, the School of Mines, and the colleges in the metropolis where scientific departments have been founded. In the first named, many of the men who have taught, and not a few of those who have studied there, have not only enriched chemical science by their researches, but have left a permanent mark upon the leading industries of this country. From the School of Mines have emanated men who in metallurgy and geology have greatly extended the application of those sciences. It is, however, a well-known fact that the public do not rightly appreciate the education that this institution is capable of affording, and that comparatively but few of the sons of manufacturers avail themselves of its advantages.

The writer calls particular attention to a plan proposed by the eminent chemist, Frémy, that young chemists of talent, who are desirous of devoting their time to the advancement of science, and therefore for the benefit of mankind, should be liberally supported by the State. It is suggested that this excellent idea should be brought to the notice of the Chancellor of the University of London, who from his well-known zeal in the cause of education, and from his position, is better able than any one else to obtain the evidence of scientific men as to its value, and, if approved of, to secure its adoption in this country. The same principle might well be extended to the other departments of science which bear upon industrial progress.

The author's firm belief is that extended scientific education is of the highest consequence to us if we wish to retain our present position in the scale of nations; that it will mostly benefit the future master manufacturer, that it must tend to elevate the social position of the intelligent working man, and to create a greater sympathy between master and man than at present prevails, and if it do this, the evils which threaten to impede, if not to paralyse our national progress, may be averted.

JAMES YOUNG, Chemical Works, Bathgate, feels bound to say that his experience accords with that of Dr. Lyon Playfair. So formidable did the rate of progress of other nations appear to many, that several meetings of jurors, exhibitors, and others took place at the Louvre Hotel on the subject. The universal impression at these meetings was that the rate of progress of foreign nations in the larger number of our staple industries was much greater than our own. But it must be stated that a large number of our first-class machine and other manufacturers are not exhibitors in Paris, whereas other nations, he believes, have taken care to bring forward their very best; still, the great progress of other countries is evident. The reason for this increased rate of progress is the excellent system of technical education given to the masters of workshops, sub-managers, foremen, and even workmen.

England for a long time excelled all other countries in the finish of her machines; but now we find that foreign machine makers are rapidly approaching us in finish, and, having skilled and intelligent labour cheaper than ourselves, are progressing in all the elements of manufacturing.

The writer uses his own case as an illustration. Originally he was a working man, but he has succeeded in increasing the range of manufacturing industry. The foundation of his success consisted in his having been fortunately attached to the laboratory of the Andersonian University in Glasgow, when he learned chemistry under Graham, and natural philosophy and other subjects under the respective professors. This knowledge gave him the power of improving the chemical manufactures into which he afterwards passed as a servant, and ultimately led to his being the founder of a new branch of industry, and owner of the largest chemical manufacturing works of the kingdom. It would be most ungrateful of him if he did not recognize the importance of scientific and technical education in improving and advancing manufactures. Many men without such education have made inventions and improvements, but they have struggled against enormous difficulties, which only a powerful genius could overcome, and they have been sensible of the obstacles to their progress. Stephenson, who so greatly improved locomotives, had to be his own instructor, but he sent his son Robert to Edinburgh University, and the son did works at least as great as the father, and with far less difficulty to himself.

The improvement in locomotion has necessarily created great competition in the industries of the world; and unless we add skilled instruction to manual labour, England cannot expect to maintain her position in the industrial race.

ON THE PRACTICAL LOSSES OF SULPHUR Etc., IN THE VITRIOL MANUFACTURE.

BY CHARLES R. A. WRIGHT, B.SC.

The main causes of the great differences often visible between the amount of sulphuric acid theoretically obtainable from a given quantity of sulphur, and that practically obtained, seem to be three in number, viz.:—

(1). Loss of SO_2 by leakage from burners, pipes, chambers, etc.

(2). Incomplete combustion of all the sulphur used.

(3). Non-conversion of all SO_2 formed into SO_4H_2 and consequent loss of SO_2 by its passage as such into the chimney.

(I.) The prevention of the first source of loss is a mere mechanical matter; without great care on the part of the workmen a considerable amount of sulphurous gas may escape from the kilns during the process of recharging, etc.; inattention to the speedy repair of all leaks in brickwork, pipes, tunnels, etc., may also cause a considerable amount of loss.

Notwithstanding the manifest bad policy of such a course, it is not uncommon to find a manufacturer neglecting to stop a chamber for repairs until the escape of gases by leakage has almost completely corroded the wooden framework of the chamber. The writer has seen a chamber, when stopped at last, present, to an observer inside, the appearance of a star-spangled sky, owing to the large number of small holes and leaks; in such an instance as this, the loss of time in stopping earlier for repairs and the expense, would have been many times repaid by the saving of sulphur lost by leakage, not to speak of the prevention of damage to the framework of the chamber and to surrounding objects by corrosion. Not only is sulphur thus lost by leakage and diffusion, but excess of air is often drawn into the chamber through the holes, thus practically diminishing the size of the chamber, and still further reducing the yield. It may be noticed here that want of attention on the part of the plumbers in first building the chamber, in so attaching the strap as to distribute the weight of the lead equally, is a frequent cause of small holes and leaks; the leaden sheet is apt to become torn where the strap is attached if too great a strain be upon it, and even if not torn the lead is strained, becomes thinner, and is more readily perforated by the action of the contained gases. The quality of the lead used, too, is of great importance; it is a false economy to buy cheap sheet lead, as it is almost certain to wear unequally, and last a much shorter time than a really good well manufactured article.

Some manufacturers consider it more profitable, in the long run, to pull down a chamber after working four, five, or six years, to melt up the lead, and rebuild with fresh materials, giving extensive repairs to the foundations, framework, kilns, etc., wherever necessary; calculating that the loss of time and capital in thus rebuilding is no more than what would have been necessary for repairs alone had the chamber been kept at work a few years longer; whilst the prevention of damage by leakage and the saving of sulphur render a handsome return for the outlay; others, again, prefer to work a chamber for eight, nine, ten years almost uninterruptedly; in fact for as long as it can by any artifice be made to hold gases.

The following numbers, being the results of successive years' working with a series of chambers to which but little repairs were allowed during the whole time, illustrate the enhanced loss from the increasing leakage:—

	Nitre used per 100 parts of sulphur burnt.	Cubic mètres of chamber space per kilogramme of sulphur burnt per diem.	Practical yield. Theoretical=100.
1st year ..	9·31	1·150	81·5
2nd " ..	9·84	1·073	75·4
3rd " ..	10·02	1·017	68·4

It is here manifest that while the sulphur burnt has increased a little, there is a very great falling off in the yield, notwithstanding that the proportion of nitre used is progressively greater; the total amounts lost in the three periods are respectively 18·5, 24·6, and 31·6.

(II.) The loss of sulphur from incomplete combustion in the burners is only noticeable to any great extent where pyrites is used. It is almost impossible to burn on the large scale any pyrites so that the burnt residue shall not contain at least 2·5 to 3·0 per cent of sulphur; where slaty pyrites containing about 30—35 per cent. of sulphur (such as Wicklow sulphur ore) is used, the residue will probably retain 3·5 to 4·0 as a minimum,* and as this burnt pyrites amounts in weight to about 80 per cent. of the green pyrites used, about 8 to 10 parts out of 100 of original sulphur will remain unburnt as a minimum.

When richer ores (such as Huelva) containing 48—50 per cent. of sulphur are employed, the residue may contain 3 per cent. of sulphur when fairly burnt, and will amount to about 67 per cent. of the weight of green ore used; so that about four parts in the hundred of original sulphur remain unburnt. Sulphur may also be volatilized as such, condensing in the pipes, or being

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* With a hard slaty ore, 4 per cent. of sulphur is probably below the mark for an average on the large scale; the writer has repeatedly seen heaps of hundreds of tons of such ore, carefully burnt, and yet retaining an average of 5 or 6 per cent. of sulphur.

carried over into the chambers: this probably is entirely preventable by paying due attention to the temperature of the kilns, supply of air, etc.

(III.) The loss of sulphur from incomplete conversion of SO_2 into SO_4H_2 may be due to several minor causes, such as the introduction of too little steam, or too much air; the use of an insufficient quantity of nitre; or what is, perhaps, most frequently the case, the allowance of too little chamber space in proportion to the amount of sulphur burnt.

Each manufacturer has a different standard as to the proportion of nitre which should be allowed to a given amount of sulphur; more nitre is required when pyrites is used than where sulphur is burnt, to overcome the dilution of the gases by the nitrogen of the air used to oxidise the iron, etc., contained in the pyrites; more nitre is again required when more sulphur is consumed to a given amount of chamber space. (*Vide* numbers previously given.) The following numbers indicate the average amounts of nitre used by different manufacturers, who have kindly furnished the writer with information on this point:—

Material burnt.	Nitre per 100 parts of sulphur consumed.
Pyrites containing 45—50 per cent. of sulphur	8·5
" " 30—50 " "	12·0
" " ditto " "	10·0
" averaging 35 " "	12·5
Sulphur	10·0

N.B. In none of these instances were Gay Lussac's nitre towers employed. For comparison, the following numbers are quoted from Richardson and Watts's *Technological Dictionary*, Vol. 1., Part iii., second edition:—

		Nitre per 100 parts of sulphur used.
Page 71	Sulphur	5·0
317	ditto (Payen, Marseilles)	6·0
318	ditto (Jarrow Chemical Co.)	8·9
317	Irish pyrites (assumed to contain 30 per cent. of sulphur, Gossage)	13·3

Again, each manufacturer has a different opinion as to the proportion to be observed between the chamber space and the amount of sulphur burnt. Thus the numbers given as an average in R. and W.'s *T. D.*, p. 80, correspond to 1·672 cubic mètre per kilogramme of sulphur burnt daily; in "Muspratt's Dictionary," *Art. Sulphuric Acid*, the numbers given as in a Lancashire works represent about 1·35 cubic mètre per kilogramme, whilst the numbers given before show that less space than this is often allowed. With reference to this point, it may be observed that, as a general rule, it is more profitable to sacrifice a small portion of the theoretical yield (*i. e.*, to cause the amount lost to be greater) in order to produce a greater quantity of a manufactured article, than it is to lessen the amount produced in order to gain the maximum yield possible. Some manufacturers indeed say it is better to strain everything to the utmost, to produce the maximum *quantity*; of course, however, there is a limit beyond which it is unwise to go, as the increased wear and tear and damage to *quality*, together with the diminished yield, may render this course the least paying.

It is difficult to give more than a rough estimate of the amount of loss which is practically wholly unavoidable. With Huelva pyrites, the results obtained with a large series of chambers furnished with coke towers, but not with Gay Lussac's denitrating apparatus, showed that it is perfectly possible to obtain 82—84 parts of sulphur in the form of acid of specific gravity 1·700 from 100 parts of sulphur used as pyrites; the nitre used being about 10 parts for every 110 of sulphur used; and from 1·100 to 1·200 cubic mètre of chamber space being allowed to every kilogramme of sulphur burnt daily. Of the 16—18 per cent. of this *total loss* from 4 to 5 per cent. were due to the sulphur left unburnt in the pyrites, and consequently about 12—13 per cent. to other causes—viz., leakage, non-conversion of SO_2 into SO_4H_2 etc., etc.

"A Practical Chemist" writes to the Chemical News of July 13, 1866, that he obtains from a ton of Irish ore, at 30 per cent. of sulphur from 18 cwt. 1 qr. 5 lbs., to 18 cwt. 2 qr. 0 lb. of acid of specific gravity 1·75. Acid of this strength at 15°C. contains 81·45 per cent. of SO_4H_2 (Bineau). Hence his total loss is from 18·0 to 18·9 parts out of 100 of original sulphur, of which probably eight or nine parts are due to the unburnt sulphur in the pyrites.

Payen obtained from 1,600 kilogrammes of pure sulphur 4,280 of acid of specfic gravity 1·85 (*i. e.*, of SO_4H_2). [Richardson and Watts's *Technological Dictionary*, I. iii. 317.] Hence his total loss was 12·7 parts in 100.

It would appear, therefore, that whether pure sulphur, rich pyrites containing 48—50 per cent. of sulphur, or poor pyrites at about 30 per cent. are employed, from 10 to 13 parts of sulphur out of 100 employed are lost by causes other than the non-conversion into SO_2 of all the sulphur used. This probably represents about the minimum practical loss on the large scale; as noticed before, by bad repair of chambers, etc., etc., this loss is greatly enhanced.

FOREIGN SCIENCE.

PARIS EXHIBITION OF 1867.

(From our Special Correspondent.)

Your correspondent is so profoundly impressed with the vulgarity of enthusiasm, that he has endeavoured to the very utmost to suppress any traces of that objectionable sentiment in his letters. In fact, it is very difficult to be guilty of the fault alluded to after spending a week in the Exposition. Gazing at works of science or art for a long time, especially if one is to write about them, produces a state of mind in which an almost stupid tranquillity is a prominent feature, and your correspondent verily believes that the "dignified repose" which the world admires in some of its idols, is often so nearly allied to stupid tranquillity, and is so difficult to distinguish from it, that it may be said to be merely an isomeric modification. The poor worn-out Frenchman who complained so bitterly that he had had "beaucoup de Sultan," told me in confidence that the air of dignified repose which so eminently distinguishes Abdul Aziz when inspecting the greatest triumphs of science, was in fact the stupid tranquillity of ignorance. He also informed me that the predecessor of Abdul-Aziz (Abdul-as-was ?) generally enjoyed the same enviable state of mind. Certain it is, that the only thing in the building that caused a smile of intelligence to pass over the Sultan's face, was a punching machine in full work, and when he was told by his interpreter that it was capable of administering 1,000 punches a minute, he replied with decided animation, that he would take one home for the benefit of the heads of those who had persuaded him to leave Turkey. Let us hope that the fatigues to which you will subject that unhappy potentate in England, will make him forget his severe (although just) determination. To return to our work:—

Your correspondent examined with great interest the two cases of Messrs. Calvert and Lowe. They are described in the English catalogue as follows:—

14. Calvert, F. Crace and Co., Gibbons Street, Bradford, near Manchester. Carbolic acid, etc. 60. Lowe, Charles and Co., 14, Fountain Street, Manchester. Carbolic acid and its derivatives, etc.

The chemist who would follow the history of carbolic acid would have to pass in review one of the most interesting phases in the whole of organic chemistry. Discovered in 1834 by Runge, it forms one of the vast number of intensely interesting substances presented by that remarkable man to science as mines of wealth to be explored step by step, and more and more minutely, as methods of investigation were perfected. It is, in fact, one of those products of destructive distillation, whose history, which reads like a romance, has been developed by the labours of nearly all the greatest names associated with organic chemistry—Runge, Laurent, Gerhardt, Wöhler, Kopp, Hofmann, Williamson, Kolbe, Cahours, Erdmann, Fritzsche, Piria, Liebig, Dumas—all these names are found at the head of the memoirs in which are developed step by step the history of carbolic acid.

Carbolic acid, known also as phenol, phenic acid, hydrate of phenyl, phenylic alchohol, and coal tar creosote, has the formula $\Theta_6H_6\Theta$, and may be represented as water in which one atom of hydrogen is replaced by the radical phenyl. It is produced in a state of considerable purity by distilling salicylic acid; the carbolic acid from this source has, however, been asserted, too hastily, to be only isomeric with the coal-tar substance.

The fact is, that until very lately, chemists had no opportunity of working with carbolic acid of perfect purity. This has led to many incorrect ideas regarding it; for example, one of the differences between the carbolic acid from coal tar and that from salicylic acid, has been said to be the greater rapidity with which the crystals from the former source liquefy when exposed to the air, but, when perfectly pure, crystals of the coal-tar acid may be exposed without deliquescing for months together.

It is remarkable, moreover, to find that the substance whose properties we are discussing, is one of those which, as far as purification goes, has been grappled with more successfully in the manufactory than in the laboratory. The most severe test for the purity of carbolic acid, like that of most crystalline organic substances, is its melting point. According to Gerhardt this is between 34° and 35° (*Chimie Organique*, iii. 16.) But crystalline carbolic acid can now be obtained in commerce, of which the melting point is as high as 42°, or even a little higher. On the other hand, the boiling point of this pure acid is given by Mr. Lowe as 182° thermometer, in liquid, or 178°·8 thermometer in the vapour, the atmospheric pressure being 743·19 mm. (29·26 inches), whereas Laurent gives 187°—188°, and Scrugham, who worked on the subject in the laboratory, and under the eye of Williamson, gives 184°. From this we conclude that pure carbolic acid has a much higher melting point, and a somewhat lower boiling point than has been ascribed to it hitherto.

Messrs. C. Lowe and Co. exhibit a large mass of absolutely pure carbolic acid, weighing no less than two cwt., having the melting and boiling points given above. It is proper to mention that Dr. Crace Calvert, who is fully cognisant of all the facts recently discovered relating to carbolic acid and its congeners, gives 187° as the boiling point of carbolic acid* agreeing nearly with the determination of Laurent.

The firm of F. C. Calvert and Co. were the first to manufacture carbolic acid in a comparatively pure condition; it then consisted in a colourless fluid yielding crystals at 15°. This was in 1859. In 1861 they succeeded in obtaining it in colourless detached crystals, fusing at about 25°. Two years later the manufacture was so far improved that the melting point of their best acid was raised to 34°, the number given by Gerhardt. At the end of 1864 they succeeded in removing the sulphur compounds which adhere with such tenacity to the acid products of coal tar, and thus rendered the carbolic acid fit for medicinal purposes. It was not until 1866, however, that the absolutely pure acid was isolated with a melting point between 41° and 42°. Other manufacturers do not appear to be aware of the processes employed by Messrs. Calvert and Lowe, for they do not exhibit in their cases acid of a higher melting point than about 25°.

Messrs. Lowe and Co. state that absolutely pure carbolic acid *does* slowly become coloured by exposure to light, and that when this coloration is not observed, it arises from the protective influence of certain impurities, apparently sulphur compounds.

Messrs. Calvert and Co. also exhibit their disinfecting powder, salve for foot rot in sheep, and sheep dip, also picric acid and aurine, and, finally, rosolic acid.

Messrs. Lowe also exhibit picric acid, prepared by the action of nitric acid on sulpho-phenic acid, a process which is said to have several advantages, particularly as regards the quantity of the resulting product. They also show rosolic acid and a fine lake prepared from it for the use of paper-stainers. In addition to the above they have in their case picramic acid prepared by reducing picric acid with an alkaline sulphide. It is used in commerce for dyeing browns.

Messrs. Lowe's case contains a series of specimens which almost exactly represent the various epochs of the improvements in the manufacture of carbolic acid, alluded to above. They consist of the various commercial qualities, having the following melting points:—

Crude carbolic acid, fluid at ordinary temperatures.

Purified	do.	"	melting	at	15°
"	"	"	"	"	29
"	"	"	"	"	35
Pure	"	"	"	"	42·25

They also exhibit what they and Dr. Calvert insist on calling bi-hydrate of phenyl. This substance is simply carbolic acid, with water of crystallisation. Dr. Calvert's formula (C = 6) is

$$C_{12}H_5O.2HO.$$

But as the water of crystallisation is given off on the application of heat, it is obvious that the true formula is:—

$$(C = 6) \quad 2C_{12}H_6O_2.2HO, \text{ or:—}$$
$$(\Theta = 12) \quad 2\Theta_6H_6\Theta.{}_2H\Theta.$$

It gives your correspondent much pleasure to admit the gratification he derived from the examination of the cases of Messrs. Calvert and Co., and Messrs. C. Lowe and Co. Messrs. Calvert and Co.'s case would, however, have appeared to much greater advantage if the globes containing the specimens had not been covered to such an extent by loose paper advertisements, which to a great extent hide the contents.

Messrs. Calvert and Co. have obtained a silver, and Messrs. Lowe and Co. a bronze medal.

Fatigued with wandering from case to case to make comparisons between English and foreign manufacturers (for, owing to the perhaps inevitable distance between them, your correspondent at times has had to walk miles in this way), it was a relief to come at last to a case where attempts at comparison would have been useless. I allude to the, in many respects, unrivalled display of Messrs. Howard and Sons, of Stratford. Their name has for many years held an enviable position with regard to the purity and beauty of their chemicals, and the contents of their case fully sustain their old reputation. In the English catalogue they are described thus:—"48. Howard's and Sons, Stratford, near London, salts of quinine, and other chemicals."

Among the many remedial agents which organic chemistry has afforded us, quinine occupies the first place, chloroform the second. Without quinine, large tracts, indeed whole countries, would be simply uninhabitable for Euro-

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* Chem. Soc. J., xviii. 66.

peans. To the backwoodsman a supply of quinine is as important as gunpowder. The "quinine famine" in the Mauritius demonstrated to thousands how small a thing even gold itself might become in comparison with the life-saving salt.

If the search for artificial quinine has been as unsuccessful as that for the Philosopher's Stone, it has at least resulted also in some great discoveries. It does not appear to be generally known that the first of the aniline colours was discovered during the search for artificial quinine! But, tired of waiting for that which did not come, finding that chemists could not produce quinine, it struck certain minds that it would be a surer plan to assist Nature a little, and Nature, as she always does when properly called upon, responded liberally. In effect, owing to the wasteful and ignorant manner in which bark was collected in its old habitat, it was, especially in the finer and richer varieties getting scarcer, this circumstance has induced certain enterprising men to cause the cinchonas to be introduced into India, and it has not only been found that the change of habitat does not prevent the development of quinine, but the valuable discovery has been made by McIvor, and confirmed by De Vrij, that covering the bark during its growth with moss increases the percentage of alkaloïds. The cinchona plantations in India are now so flourishing that there need be no apprehension of the supply of quinine ever failing, and if the discovery of artificial quinine should ever now be made, it would have to depend upon its value for its cheapness. We are aware that the discovery of artificial quinine has more than once been announced, but up to the present time such announcements have never been supported by positive evidence.

Messrs. Howards show an unrivalled collection of barks, including several from the new Indian plantations, and a specimen grown in England by Mr. J. E. Howard. It is exceedingly interesting to find that this English bark also contained quinine, for a specimen of the alkaloid extracted from it and its sulphate are exhibted. In addition to the above, this remarkable case contains specimens of the following alkaloids or their sulphates:—

Quinine	$C_{20}H_{24}N_2O_2$
Quinidine	$C_{20}H_{24}N_2O_2$
Cinchonine	$C_{23}H_{24}N_2O$
Cinchonidine	$C_{20}H_{24}N_2O$
Aricine	$C_{23}H_{26}N_2O_4$

It appears to your correspondent that an attempt to convert quinidine, cinchonine, or cinchonidine into quinine would be far more likely to meet with success than any efforts to produce that alkaloid by building it up step by step, and considering the enormous quantities of cinchonine which have been accumulated by quinine manufacturers, and are almost valueless, it is evident that such a discovery, however hopeless it may look at present, is well worth an effort. It must not be forgotten moreover, that researches of this kind, if carried on by competent persons, are certain to be fruitful in discoveries; even if the great end sought for be not attained. The admirable experiments of Pasteur are quite enough to prove the truth of this assertion.

In addition to the above, Messrs. Howard exhibit numerous chemicals of the highest possible quality. Amongst them is benzoic acid, on the perfect purity of which they pride themselves. It is free from the slightest admixture of the hippuro-benzoic acid, made by boiling hippuric acid (from the urine of cattle) with hydrochloric acid. This artificial benzoic acid is largely imported from the continent and sold as the genuine article. The acid thus produced is, however, never quite free from the odour of the urine of herbivorous animals, and, no matter how carefully purified, is without the fragrance which characterises it as obtained by sublimation from gum benzoin. This has induced some unscrupulous persons to mix a portion of the acid from benzoin with that from urine, in order that the fragrance of the one may carry the other off.

The tartaric and citric acids exhibited by this firm are in superb crystals, and have the appearance of being in the highest possible state of purity.

A curiosity in its way is carbonate of ammonium, prepared entirely from volcanic products, and therefore free from the impurities of which traces are almost always to be found in the salt as obtained from the crude sulphate of ammonia of the tar distilleries. As is well known, crude boracic acid from Tuscany often contains borate, sulphate, and chloride of ammonium. One sample analysed by Wittstein, contained no less than eight-and-a-half per cent. of sulphate of ammonium. The carbonate of ammonium shown by the Messrs. Howard, is obtained in their process for preparing borax from the crude boracic acid of commerce, and is, therefore, as they say, of purely volcanic origin. We believe there is no other firm who prepare salts of ammonium from the same source. The morphia and its salts shown in the same case, appear of very fine quality, and the same remark may be made with regard to their mercurial preparations. I am much pleased to see that the admirable display I have described, has obtained the distinction of a gold medal.

It is becoming common now to hear people express the wish that the present Exhibition will be the last for many years to come. But, as is so often the case in other matters, the majority of the persons who talk thus have really no definite reason for what they say. When you ask them for a motive for expressing their objection to exhibitions, you generally get such vague and unsubstantial reasons as "Oh, they are such a bore, you know." "They don't really do any good," or "They injure the retail trade of the cities where they are held." "One is sick of exhibitions," and so on. Now, while your correspondent has never once heard a really sound argument against exhibitions, he has, on repeated occasions, seen and heard enough to show him that the present Paris Exhibition must (if we do not allow our insular vanity to cause us to fall into decadence), exercise a most important influence upon our arts and manufactures.

I contend that no Englishmen of sound judgment could possibly examine in detail, as your correspondent has done, the various departments allotted to the European kingdoms, without having the fact forced upon him that our neighbours are running abreast of us in a race which, if we lose, will disgrace us for ever.

The intense dislike which a vast number of influential people took to all the schemes of the late Prince Consort, led them to oppose everything connected with his attempts to encourage technical education. But, if we are to retain our supremacy—nay, if we are not to be ignominiously beaten in the very departments where we have somewhat too superciliously fancied ourselves invincible, we must not only soundly educate our workmen in all the subjects relating to their special avocations, but we must also *stimulate their ambition.*

Fortunately the minds of some of the most active and far-seeing of our scientific and political men are directed to this deeply important question, and your correspondent trusts that you, Mr. Editor, will keep the matter before the scientific public.

To chemists the matter is as important as to engineers and artists. The chemical manufactories of France and Germany, especially the latter, are always able to obtain the services of men who, having received their chemical education in laboratories directed by high-class men, are able to devise new processes, improve those already existing, and, especially to adopt, simplify, and cheapen methods invented in this country, so as to deluge our markets with low-priced and often good articles, to the serious injury and even ruin of our own manufacturers.

Of all the faults which tend to the perpetuation of the system, or rather want of system, under which we are now obstinately insisting on learning how to be beaten, the greatest is the stupid vanity which leads us to underrate the strength and, above all, the intelligence of our rivals. We know how ruinous this is in war; it is equally so in the conflict between English and foreign manufacturers.

Now that the Universities generally are to be represented, such men as Playfair and Lowe will find a proper sphere, and will, we doubt not, force this matter on the attention of Government. Whether our present men in office will take up the matter is another affair; on the Continent the idea appears to be that our administrators are too busy with the political questions of the day to do anything towards the advancement either of science or art. Unhappily it needs no ghost to tell us that the "country gentlemen" who do us the honour of governing us, feel very little interest in the matter, and the so-called representatives of the trades are too much occupied with supporting strikes, to see that the prosperity of those whose interest they betray, are in reality dependent upon questions of which violent and uneducated men do not even know the existence.

But all this cannot last for ever; foreigners who only judge of us by our newspapers, ask with astonishment, "But these MM. Broadhead, Beales, and Whalley, who are they?" As soon as our workmen are properly educated they too will ask similar questions; and it will then be seen that the workman's friend is not he who shows him how to strike against his employer, but he who shows him how to develope to the utmost those great gifts of intelligence, patience, skill, and strength which for so many years made British engineers and manufacturers the astonishment of the world.

Before concluding this letter, your correspondent must again protest against the use of exhibitions as *mere* advertisements of the exhibitors. I contend that the committees to whom is delegated the power of acceptance or refusal, should resolutely refuse admittance to articles which do not show upon the face of them the condition of the industry which they represent. When Messrs. Wood and Co. send a few bottles of effervescent drinks which for all that appears might be merely water, I say they should be refused admission. They *may* be of the highest class, but as they stand in the case, they show nothing that may not equally well be seen in the window of a restaurant. It is true that one bottle, by leaking a little, tries its best to attract attention, and by pouring out a sorry libation to earth may perhaps get removed to a more congenial sphere where soda-water bottles are at rest.

To return, your correspondent trusts that you, Mr. Editor, will not think that he has dwelt too much upon the subject of the industrial education of the working-classes in this country. The question is of vital importance, and it is only by the publicity attainable through the medium of the scientific journals that the inertia of the masses can be successfully overcome.

It shall not be said that I have sent you a letter upon the Paris Exhibition in which no allusion is made to the contents of the building, so I will now proceed with my notes upon the English section.

13. Calley, Samuel, New Road, Brixham, Devon. Specimens of "Torbay," iron oxide paints, and compositions for ships, metal sheathing, etc.

The problem of producing a good sound paint of various colours suitable for wood, iron, and stucco-work, with a base of iron instead of lead, has certainly been creditably solved by Mr. Calley. In addition to extreme cheapness they are free from the deleterious character of lead paints, and are said to be much more lasting. The inventor states that a square yard of iron can be covered for one farthing, and that the labour in applying them is considerably less than with ordinary paints. In fact, from careful experiments it has been ascertained that Calley's paint brought to a working consistency with 45 per cent. of its weight of linseed-oil, covered four square yards for one penny, the labour required being only 60 per cent. of that required for lead paint. It appears from what we have been able to gather that these paints are principally made from a native oxide of iron found at Brixham in Devon. The paints produced are of two kinds. Those of the first class, which are principally adapted for iron-work, appear to be made direct from the native oxide. Variety of hue is obtained, we believe, by subjecting the ore to various temperatures.

Calley's colours of the second class are tinted by admixture with metallic substances capable of yielding the desired shades. One experiment that was made to determine the comparative resisting powers of lead paint and iron oxide paint to heat, is certainly sufficiently remarkable to deserve mention. Some sheets of iron were covered on one side with Calley's oxide paint, and on the other with red-lead. One of these, when thoroughly dry, was placed over an open coal-fire, the oxide-painted side downwards. The upper or red-lead side soon began to crack and blister, whilst the oxide-paint still adhered to the iron, and only changed its tint. In further trials it was found that at a temperature which approached a red-heat, the oxide paint was only deteriorated, while the red-lead was completely destroyed.

(From our own Correspondent.)

Paris, July 30, 1867.

Discovery of the Cromlech of El-lanic.—Ancient pottery, worked flints, and stone hatchets.

At the last meeting of the Polymatic Society of Morbihan, M. de Closmadeuc gave a description of a very fine cromlech discovered by him in a very small desert island in the gulf of Morbihan, called El-lanic, situated south of Gavara's. This cromlech is represented by more than sixty obelisks of granite, forming a vast regular circle of 180 mètres in circumference. The mean length of the blocks is three mètres. One of the blocks is of colossal size; it is broken into two fragments, and measures 5·30 mètres long by two mètres thick. A curious fact worthy of notice is that one half of the cromlech is no longer on the island but on the sea shore, so that the view of the whole circle is only attainable at low water; the sea has encroached by degrees on the island on the south side where the cromlech is found, and eaten away half the ground.

M. de Closmadeuc, who has made excavations in the neighbourhood, has discovered an incredible mass of ruins and antiquities, the nature of which cannot be doubted:—1. An enormous quantity of pottery perfectly similar to those habitually found in Celtic monuments. 2. A quantity not less considerable (several hundreds) of flints worked by man, analogous to those found in ossiferous caverns, or of Grand-Pressigny. 3. A great number of stone hatchets which have been found under the Armorican Dolmens.

The discovery of the cromlech of El-lanic is of great interest. It is most remarkable, and the most complete of the cromlechs hitherto known in the Morbihan.

Paris, Aug. 6, 1867.

Meeting of the Society of Encouragement.—Writing inks.—Decoration of porcelain.—Steam Machinery.—Industry of caoutchouc.—Reaction of oxygen on molten iron.—Researches on ozone.—Newton and Pascal.

The Council of the Society of Encouragement held its periodical meeting in Paris on the 26th ult.; M. Dumas is the chair. MM. Robert and Theurer, watch and clock makers at Chaux-de-Fonds (Switzerland), laid before the society the drawings of their great repeating clockwork and winding apparatus for clocks and watches, an appendix to the communication made by them on the 12th July.

M. Becker, Rue-de-la-Glacière, 72, Paris, offered himself as a candidate for the prize founded by M. Alexandre for the improvement of writing inks, and handed in samples of his black and violet inks.

M. Besson, ceramic lithographer made, in his name and that of M. Macé, porcelain manufacturer, a communication on the application of chromolithography to the decoration of porcelain. He showed how easy it was to transfer on porcelain and earthenware chromolithographic pictures executed on paper with the colours and varnish adapted for painting on porcelain.

M. Duprez explained a contrivance, invented by him, for a more perfect distribution of steam by a variable detent acting without the aid of an excentric. In all slide valves with a single box, according to the extent of the admission, the opening of the port is more and more narrowed, and the exhaust ports are opened sooner. M. Duprez avoids this inconvenience by the following means, the employment of which is particularly adapted for locomotives. The contrivance consists in a parallelogram fixed to the crosshead. In the model presented to the Society, the durations of the admission and the compression are sensibly the same (at equal detents), as with the much-employed system of the link valve reversed; but the new system gives the parts a greater opening by 45 per cent. An analogous combination, admitting a toothed wheel forming an epicycloidal, gives similar results, and is peculiarly adapted to stationary engines.

Mr. Balard placed before the Society the progress of the industry of caoutchouc during the last few years. Commerce has furnished, in fact, 9,000 tons of caoutchouc, the value of which is 40,000,000 francs in a raw state, and 75 to 80,000.000 francs in a manufactured state. One half of this quantity, and the purest, has come from the province of Para. The industrial demands are so important, that experiments have been made in Brazil for cultivating the tree which produces this substance, in the same way as the quinquina has been grown in the Himalaya. It is extensively used for waterproof clothing, boot soles, cards for wool, etc.; in the cards where much suppleness is required, the purest material from Para alone is used. The processes for vulcanising have also been perfected. The employment, for this purpose, of the chloride of sulphur, has become general; it has been employed either alone or dissolved in sulphide of carbon; the employment of litharge for neutralising the hydrochloric acid of moderating the sulphuration, has been better regulated. The temperature of 135°C., at which the vulcanisation takes place, has been rendered more fixed and certain by substituting, for the ordinary boiler, currents of steam. Cords of vulcanised caoutchouc are made of the pure materials, masticated with extreme care in small masses, which are kneaded together so as to form a homogeneous mass, the vulcanisation being effected by steeping the packets in water heated to a constant temperature of 135°C.

M. Troost, Professor of Chemistry, called the attention of the Society to the results obtained by properly treating cast iron at a high temperature with a current of oxygen gas. This experiment, made in 1855, by M. Henry Saint-Claire Deville, is the starting point of all the researches subsequently made. It gives the means of easily obtaining Bessemer steel, or, if it be required, a very pure soft iron. M. Troost repeated his experiment before the assembly. The cast-iron placed in a crucible of quick-lime, is fused by the combustion of a mixture of hydrogen and oxygen gases. In this state, and by increasing the emission of oxygen, the carbon, silicium, and sulphur are burnt up, and form a dross which is absorbed by the substance of the crucible; the oxygen burns the iron itself, leaving the iron in a melted state, excellently pure.

From experiments carefully made, the hydrometric commission of Lyons has established since 1852 the habitual absence of ozone in the air of that town, while its presence is constantly evident in the exterior of the city. In the month of September, 1865, the Imperial Observatory caused the same studies to be made, on an extensive scale, over all France, with a view of discovering some law, if there is one, that would explain this phenomenon. The same reagent papers were remitted to all the stations, so that they might be rigorously compared together. The observations were taken during a whole year; the results of this immense work will be, perhaps, soon known to the world. Meanwhile, M. Fournet gives the conclusion arrived at by his resumed observations at Lyons and the suburbs, with the co-operation of M. Lambert and Rassinier. While ozone was very abundant at Sauvage, on the heights of Tararæ, a range of hills separating the basins of the Loire and Rhone, traces were barely perceptible once or twice a month at Lyons. It is well known that it has been often maintained that the arrival of the cholera was coincident with the disappearance of ozone in the air. The example of Lyons does not agree well with this assertion; this city is not subject to cholera, and, at the same time, its atmosphere is always deprived of ozone.

The revelations of M. Chasles with respect to Pascal are the great topic of the day, and we wait here with impatience the news of its reception from the English press. M. Chasles is far from having furnished his last observations, as he has only placed before the public a small portion of his treasures. He is in possession of other documents which prove that Pascal had discovered before Newton, and even taught Newton, the admirable fact of the decomposition or the dispersion of light; that he was the first to make the celebrated experiment of the prism, to observe and number the seven principal colours of the solar spectrum.

The glory, also, of the employment of the word "differential," or "differential calculus," to distinguish it from the calculation of fluxions and fluents of analytical algebra properly so called, escaped Newton, to fall upon his adopted father, Pascal. Newton will not cease to be considered as a great man, but the public will cease perhaps to believe in his genius and his good faith, when it will be known that at his earliest career he was in possession, through Leibnitz, of printed and manuscript documents of Kepler; through Viviani he had the MSS. of Galileo; by Pascal, the MSS. of Descartes, even those which were brought from Sweden after his death; these latter were sunk in the Seine along with the boat which carried them, and fished up again at the old quay of the Louvre; also by Pascal, Newton had his letters, notes, thoughts on attraction, the decomposition of light, the differential method, etc. When he had such a remarkably precious store, it is not surprising that he arrived at such wonderful results, but it is astonishing that he took so long a time to put these documents in practical use. Ah! if Pascal had had the time and leisure necessary to write in French the *Principia*, with what depth and clearness would he have developed his magnificent theme!

M. Chasles possesses also a letter in which Arnaud, grieved a little at the effect produced by the first of the "Provincial Letters," wrote a most interesting word of encouragement to Pascal.

F. Moigno.

Paris, Aug. 13, 1867.

Mother of Pearl Colours applied to Porcelain.—Improvements in Calico Printing.—Engraving on Glass with Hydrofluoric Acid.—Polychromic Impressions on Porcelain.—The Spherometer.—The Exhibition Awards.—Security from Fire.—Steel Wire Drawing.—Cheap production of Oxygen.—Bleaching with Fluosilicic and Sulphurous Acids.

We have already mentioned that the Society for the Encouragement of National Industry holds, during the Exhibition, weekly instead of monthly meetings.

At the meeting on July 26, M. Maillard invited the Society to attend his experiments on the next day, at Avenue Montaigne, No. 21, Paris, to show the superiority, as regards security from fire, of his mineral roof-sheeting, compared with that of zinc or tiles. [We learn since, that the tiles and zinc gave way at once, while the new mineral covering resisted the effects of fire.]

M. Lavollée read, in the name of the Committee of Commerce, a report by M. Legentil on the manufactory of M. Teste, established at Lyons, for steel wire-drawing, in which he employs infirm workmen. This establishment has quintupled its importance in spite of the reduction of the prices resulting from the treaties of commerce, it has more extended relations with foreign countries, and in it are fabricated all important objects of steel wire-work, needles, umbrella-ribs, knitting-needles, crochets, enamelled-headed pins, pivots for different trades, springs for crinolines and corsets, etc. M. Teste announced that he employs 246 workmen and workwomen, 100 of which are employed at their homes. Besides these latter he employs about 120 young infirm females who have found a refuge in a religious institution, established near his works, and called Sainte-Elisabeth Providence of the infirm.

On the 2nd of August, 1867, M. Chevallier in the chair, a meeting was held in which M. Salvetat presented a report on mother-of-pearl colours applied to the decoration of crystal and porcelain, made by M. Briançon, decorator, Paris. He remarked that the pearl varnishes of M. Briançon were, eight years ago, objects of examination and encouragement on the part of the Society. Since then, the method has been extended to new applications, and has met with much encouragement abroad; thus we observe in the Exhibition a great number of pieces of pearly porcelain in the Belgian, Italian, Spanish, Prussian, Austrian, and Russian sections. The employment of bismuth for this purpose originated, according to Mr. Salvetat, in France.

M. Schutzenberger, professor of chemistry at the College of France, gave a very interesting lecture on his improvements in the printing of woven stuffs during the last few years. The mechanical principles adopted are the same as formerly; the means of engraving the plates have been much improved, and blocks of fusible metal are now oftener employed; also the impressions by engraved rollers have come more generally into use. Machines have been constructed for printing at one operation eight to twelve colours, and in England they go as far as twenty-four colours. The rollers are of cast iron covered with copper, and the engraving is made by the ordinary means of aquafortis; the chemical processes were the object of more extended study, since eight or twelve colours have to be fixed on the tissue by one and the same means, as they are applied simultaneously. Albumen is one of the substances most generally used. It coagulates at the boiling point of water, and retains and fixes on the tissues the colouring matter with which it is charged; it also forms a mordant for applying, on cotton, colours derived from aniline which can only be fixed by nitrogenized substances. The lecturer detailed also the processes employed in the use of garancine or madder colours, and went into a very ample description of aniline colours and their improvement, and he dwelt upon the stability and indestructibility of aniline black.

M. Peligot, member of the council, explained the processes generally in use, for engraving on glass by hydrofluoric acid. This acid in a liquid state gives a polished and transparent surface to the glass, and is useful for making decorative designs on white glass lined with coloured glass. Dull engravings on glass are obtained by the use of neutral fluorides, to which acid has been added. M. Kessler applies these processes at Baccarat; they are also used at Saint Louis. This last establishment consumes annually 800 kilog. of hydrofluoric acid; Baccarat employs more. The designs are first drawn on stone, and proofs are taken off with an ink containg wax and bitumen, on unsized paper, coated with starch first, then with gum, and thirdly with collodion. When the sheet is applied to the glass, the paper is washed off with water, leaving only the impression and the coating of collodion which covers it. A similar proceeding is employed for polychromic impressions upon porcelain, the collodion being burned off in the fire. Five hundred large sheets of glass engraved in this manner, measuring 2m. 20c. long by 60c. wide, have been made for the establishment of M. Duval, in Paris.

M. Perreux, engineer, explained the principles of his *spherometer*. It consists of a screw, the pitch of which is a quarter of a milimètre, and a circle divided into 500 parts, so as to obtain the graduation of 1-2000th of a millimètre. He also explained the construction of a self-acting machine, capable of marking, in a right line, divisions visible by the microscope, only the thousandth part of a millimètre asunder.

Among the many persons of great merit in the French section of the Exhibition who have been either badly rewarded, or passed over in silence, we cite the following: M. Collas, the pioneer of benzol, inventor of nitro-benzol, and classed by the Society of Mulhouse among the discoverers of aniline colours; he has exhibited at the Paris Exhibition two important discoveries, and the jury have not awarded him even honourable mention. What has become of the medal certainly awarded him by the jury at the first meeting?

M. Dubrunfaut, discoverer of the application of osmose to the extraction of sugars, exhibited by M. Camichel de la tour du Piu (Isere), a successful operation, greatly extolled in three lectures by M. Payen, a most excellent judge, at the *Conservatoire des Arts et Métiers*, at the Sorbonne, and at the Society of Encouragement, has been completely ignored.

The "Mille" gaz, and gazo-lamp, the former of which is remarkable for the facility with which gas is produced without any mechanical agent or fire, by an apparatus easily put up anywhere; and the latter now sold by thousands all over Paris, and in the Provinces, besides being exhibited in hundreds of different shapes and designs at the Exhibition, do not figure even in the catalogue.

A noble widow, Madame de Clercq, spent £140,000 in order to render the commune of Oignies, in the Pas de Calais, in which she resides, a terrestrial paradise. She has constructed at her expense a vast church, an asylum, a school and work-room for girls, a boy's school, a house of patronage for youth, an asylum for the aged, cheap dwellings, etc.; she has founded courses of studies for adults, Sunday-schools, a library, a club, recreation and exercise-rooms, with medical consultion, a savings' bank, etc. A group of neighbouring roads has also been organized and kept in repair, more than 400 acres have been cleared and let out to 550 families, a coal mine has been sunk, and water supply given to the town, now numbering 1800 souls, all at the expense of this lady, to whom the jury have awarded the humble prize of a silver medal!

The aim of the Agricultural Society of Entomology is to contribute to the multiplication of useful insects, and to promulgate the means of destruction of noxious ones. It is composed of honorary and titular members, without any limit as to their number. The title of honorary member can be conferred on persons who, by their publications and works, forward the views of the Society. It can be also conferred upon persons who aid the Society by their patronage or donations.

Any person, without distinction of residence or nationality, can be received as titular member and correspondent of the Society, by being presented by a member of the Council, adhering to the statutes, and paying ten francs per annum. From time to time reports are issued relative to the multiplication of insects useful to man, and the destruction of those hurtful, indicating the rational means of healthy development; in the other case the most ready method of extirpation. There are being formed a library, collections, and a museum; also, the documents and papers received are centralized and classified, and those intended for publication are indicated. An enquiry office has been instituted, in which interested parties can obtain information on the insects about which they desire it.

Every two years an exhibition is organised in Paris, of useful insects, their products, and apparatus adapted to their cultivation; and of noxious insects, the ravages caused by them and their means of destruction.

The process for the cheap production of oxygen, by M. Tessie de Motay, has perfectly succeeded in the laboratory of the Exhibition established on the banks of the Seine. The new method, so simple and efficacious, has fulfilled all expectations. Fifty kilog. of manganate of soda give 400 to 450 litres of oxygen per hour, after eighty successive reoxidations. M. Tessie de Motay has so perfected the fabrication of manganate of soda, that he is almost certain of being able to furnish it to the trade at the price of 30 or 40 centimes the kilogramme. The necessary arrangements for essaying on a large scale the oxyhydrogen light at the Hotel de Ville, in the square as well as in the interior, advances rapidly, and the generators are fitted up already in the cellars. Oxygen and common gas burned together in common burners increase the illuminating power from 1 to 8 or even 15, or can serve at the same time for burning in the chloride of magnesium lamps of M. Carlevaris, which do excellent service.

In order that the essays of spontaneous bleaching with fluosilicic acid and sulphurous acid could completely succeed, it was necessary to render as cheap as possible the production of manganate of soda. Applied to pulp of wood and straw it gave such excellent results that the manufacturers themselves scarcely knew whether they were treating rag-pulp or straw-pulp.

F. Moigno.

REPORTS OF SOCIETIES.

ROYAL INSTITUTION OF GREAT BRITAIN.

On the Absorption of Gases by Metals, By Dr. Odling, F.R.S., *etc.*

(*Continued from p.* 144.)

You observe in the case of all the metals we have as yet considered, that they are characterised by absorbing chiefly one particular gas. Platinum and palladium are characterised by absorbing hydrogen alone; and copper also, by absorbing hydrogen. Whether it absorbs any other gases has not, I believe, been determined. In the case of gold, the gas absorbed most abundantly is hydrogen, though it is certainly capable of absorbing a great many others. Now, with regard to silver, we find that it has the special property of absorbing oxygen gas. I am not referring to the temporary absorption of oxygen by melted silver, and its evolution on the solidification of the metal, which constitutes the well-known spitting of silver, but to the occlusion of the gas by the solid metal—for it is not merely an absorption, it is an occlusion, the metal retaining the gas for any length of time; and you will remember that, when speaking of platinum, I mentioned that the metal retained its hydrogen for two months, although exposed freely to the air. In different experiments, then, silver wire was found to absorb 74 per cent. of oxygen, and nearly 21 per cent. of hydrogen. Silver sponge absorbed 722 per cent. of oxygen; 92 per cent. of hydrogen; 52 per cent. of carbonic acid, and 15 per cent. of carbonic oxide. A specimen of silver leaf exposed to the air at a red heat absorbed 137 per cent. of oxygen and 20 per cent. of nitrogen. Accordingly, while ordinary atmospheric air contains 21 per cent. of oxygen, and the air absorbed by gold only 5 per cent. of oxygen, in the air absorbed by silver the oxygen amounts to 85 per cent.

Now we come to another metal, and of very great interest in relation to this occlusion of gases—namely, iron. Ordinary iron wire was carefully cleaned, heated in vacuo to drive off its natural gas, and then charged with different gases at a red heat. It was found that the wire, treated in this way, absorbed 46 per cent. of hydrogen; but there was another gas which it could absorb in a much larger proportion—namely, carbonic oxide. Just as platinum is distinguished by its copious absorption of hydrogen, and silver by its absorption of oxygen, so is iron distinguished by its ready absorption of carbonic oxide. Whereas it absorbed only 46 per cent. of hydrogen, it actually absorbed 415 per cent. of carbonic oxide.

A point of interest connected with iron, as with platinum, is that the metal, though perfectly impervious to gas at ordinary temperature, yet when strongly heated, allows the passage through it of hydrogen gas, as shown by M. Deville, and of carbonic oxide, as shown more especially by Mr. Graham, whose admirable elucidation of the nature of these transmissions, forms the subject of our consideration this evening; and who has shown that in the case of iron, as in that of platinum, the transmission of the gas is preceded by its absorption in the substance of the metal.

With regard to the natural gas of iron wire—that is to say, the gas existing in the metal as ordinarily met with,—I may direct your attention to the particulars of one or two experiments. In one experiment, the wire heated in vacuo was found to give off 7·94, which we will call 8 times its volume of gas, while in another experiment, in which the heating process was kept up for a much longer time, it gave off 12 times its volume, or 1200 per cent. of natural gas, consisting chiefly of carbonic oxide. Whence it would appear that ordinary wrought iron occludes in the forge, and continuously retains, from 8 to 12 times its own volume of natural gas.

Now iron has two distinct origins. In addition to what we may call terrestrial or telluric iron, there is another kind of iron which we may call sidereal, that is to say the iron of meteorites. Hence it becomes a question of interest to ascertain whether this meteoric iron contained any gas, and if so, what was the nature of the gas; because it would appear, from the existence in the different metals examined of what may be called natural gas, that every metal contains a residue of the gas in which it last existed at a temperature of ignition. If the metal has been last ignited in hydrogen it retains hydrogen, if the metal has been last ignited in carbonic oxide it retains carbonic oxide. Does meteoric iron then contain any gas? and if so, of what kind? For the purpose of ascertaining these points, a quantity of meteoric iron from the Lenarto fall, of about 45 grammes by weight, and six cubic centimètres by volume, was taken, cleaned, and introduced into a porcelain tube of this kind. A vacuum was made in the tube, and heat then applied, when so soon as the meteorite got red-hot, it began to give off some gas; and here in this repetition of the experiment, I hope to show you some of the gas which this meteorite has brought down from—who can say where. There we have the porcelain tube, containing the meteorite, exhausted by the air-pump and being heated by the furnace. One end of the tube is closed; the other end connected as you see with the Sprengel pump. The mercury is now falling in the Sprengel pump, and as it falls is delivering the gas. In this test-tube we are now collecting the gas extracted from the meteorite, and which the meteorite has brought down from the place where it last was at the temperature of ignition.

Now comes the question, what is the nature of this gas? Well, the 45 grammes or 6 cubic centimètres of iron were ignited in this way for two hours and a half, and during those two hours and a half, they gave off 60½ cubic centimètres of gas, which I may say at once consisted substantially, not of carbonic oxide—the gas existing in terrestrial iron which has been last ignited in a coal furnace—but it consisted substantially of hydrogen, or contained, at any rate, 85½ per cent of that gas, with a small quantity of carbonic oxide and nitrogen. Whereas, then, telluric iron contains carbonic oxide absorbed from the atmosphere in which it has been last ignited, this siderial iron contains hydrogen instead, absorbed, I suppose, from the atmosphere from which it has been last ignited. My distinguished friend, Mr. Miller, in co-operation with Mr. Huggins, has demonstrated the existence of hydrogen gas in celestial bodies by an analysis of the spectra of several of the stars; but we have got, in this test-tube, the thing itself. Here is the free hydrogen gas which has been brought down to us by one of these smaller stars. Perhaps a point of still further interest, relating to this subject, is the classification of the stars according to their spectra, which Father Secchi has recently made. He has divided the fixed stars into three classes, in one of which, typified by *a* Lyræ, the spectrum is essentially the spectrum of hydrogen. That star, then, undoubtedly contains an atmosphere, of which the prevailing constituent is hydrogen gas. Now, upon a subject of this kind, which is only three days old, it is not in human nature to avoid speculating a little; and I think we may venture to speculate thus far, that our meteoric iron probably absorbed its hydrogen from a star-atmosphere of the class which is so typified.

But there is another point bearing upon this subject. Under ordinary circumstances, we are not capable of getting into iron more than half its volume of hydrogen; but this natural iron contains nearly three times its volume of hydrogen. Now, if under ordinary atmospheric pressure, iron will absorb only half its volume of hydrogen, what sort of a pressure would it require to enable it to absorb six times that quantity? According to our present notions, it must at any rate have been exposed to an atmosphere in a very condensed state. We can not imagine that, in the interplanetary space of our ordinary solar system, there should be gas of a sufficient density to permit the absorption by this meteorite of six times the quantity of hydrogen absorbable by iron under the pressure of our telluric atmosphere.

We have now succeeded in extracting a considerable volume, many cubic centimètres, of gas from our ignited meteorite. I almost think, although the experiment is but a small one, that, as we have hydrogen brought from so great a distance for our examination, it is worth while to darken the theatre in order to witness its combustion; and having lowered the theatre-gas, we will now inflame our hydrogen of the stars. [The meteoric hydrogen was accordingly ignited.]

So much, then, for the great fact of the absorption and occlusion of different gases by metals. Now we have to consider what is the nature of the absorption. In the year 1823, Mr. Faraday established the general proposition that a gas was nothing else than the vapour of a volatile liquid, existing at a temperature considerably above the boiling point of the liquid. In the case, for instance, of liquid water, which boils at 100°, if the vapour of this water be afterwards heated up to a temperature of 150°, the water-vapour existing at the temperature of 150° is a true gas, possessing all the physical properties of a gas. If we take this water-gas existing at 150°, and cool it down to a little above the temperature of 100°, it loses some of the characteristic properties of a gas, and becomes what we more strictly call a vapour; and this vapour, if subjected to a temperature ever so little below 100°, at once becomes converted into liquid water. Accordingly, the boiling part of liquid water may be equally well called the condensing point of water-gas: and similarly, in case of other gases, their respective condensing points are merely the boiling points of the liquids producing them. But the boiling point of a liquid, and the condensing point of its gas, is not a fixed point of temperature, but is a point of temperature varying with the pressure to which the gas or liquid is subjected. For instance, under ordinary atmospheric pressure, water boils at 100°, but under the pressure of five atmospheres it boils at 150°, so, that if we take our water-gas existing at 150°, and maintaining its temperature at 150°, then subject it to a pressure of five atmospheres, its temperature will no longer be above the temperature of its boiling point, and it will cease to exist as a gas at all, and become converted into liquid water. Accordingly, we may effect the condensation of water-gas, existing at any temperature, by cooling it down to its ordinary condensing-point, or by subjecting it to a degree of pressure at which its existing temperature will not exceed that of its thereby heightened condensing-point. And so with other gases. Every one of the very many different gases known to the chemists, with about six exceptions, has been condensed into the liquid state, by a certain reduction of temperature; or by a certain increase of pressure; or by a conjoint reduction of temperature and increase of pressure, until the temperature of the gas has been brought below the heightened condensing point or boiling point corresponding to the increased pressure.

Now let us consider more minutely, for a minute or two, the effect of pressure upon these condensable gases, taking water-gas as an illustration. Suppose we take water-gas existing at 150°, and contained in a cylinder closed in by a movable piston, which is balanced, or weightless. Supposing the area of the piston to be one square inch, the water-gas is subjected to a single atmospheric pressure, which is equivalent in this case to 15lb. Now if we place a 15lb weight upon the piston, we shall double the pressure, and consequently halve the volume of our water-gas. In order to reduce this gas to one-fifth of its original volume, we shall have to quintuple the original pressure, by placing four such weights upon the piston. But at a pressure of five atmospheres the temperature of the gas, or 150°, would cease to be above the temperature of its condensing point; and the gas, instead of being merely compressed into one-fifth of its original volume, would cease to exist as gas at all, but become converted into a scarcely appreciable volume of liquid, and the piston would descend to the very bottom of the cylinder. Accordingly water-gas at 150°, being condensable by a pressure of five atmospheres, it is impossible to reduce it to one-fifth of its bulk without liquefying it. And, similarly, with other gases. Ordinary ammonia gas being, at ordinary temperatures, condensable by a pressure of seven atmospheres, it is impossible to reduce it to one-seventh of its volume without liquefying it. Take, again, the case of sulphuretted hydrogen gas. That being condensable at ordinary temperatures, under a pressure of 15 atmospheres, it is impossible to reduce it to one-fifteenth of its bulk without liquefying it. Hydrochloric acid gas being condensable at ordinary temperature by a pressure of 40 times the ordinary atmospheric pressure, it is impossible to compress this gas into one-fortieth of its bulk without reducing it to the liquid state. And, in general, a gas maintained at some definite temperature, cannot be reduced to a bulk less than that corresponding to the pressure necessary to liquefy it, without its becoming liquefied. Conversely, the reduction of any gas to a bulk less than that corresponding to the pressure necessary to liquefy it, must be taken as evidence of its liquefaction.

Now, although oxygen and hydrogen gases have never yet been reduced to their liquid state by mere pressure, there is strong reason to believe that these and other gases, absorbed by heated metals, really exist as liquids, or, at any rate, do not exist as gases within the substance of the absorbing metals. For these two gases have never been reduced to less than $\frac{1}{800}$th of their bulk by mere pressure, but when absorbed by massive iron, platinum etc., they are seemingly reduced in bulk many thousand-fold. Moreover, all gases, even the most insoluble, are certainly liquefiable by solution. Here I have an experiment arranged to show the liquefaction of gases by solution and absorption, with a view to suggest the analogy subsisting between the absorption of gases by liquids and by heated metals. This sealed tube is filled with ammonia gas, and on breaking the end of the tube under coloured water, you observe that the gas is absorbed in an instant, the water rushing up to the very top of the tube. Now ammonia is one of the most soluble of all known gases, but, notwithstanding its great and rapid absorption, which you have just witnessed, it is in reality less soluble in, or absorbable by water, than hydrogen is absorbable by palladium. At mean temperature one volume of water absorbs about 780 volumes of ammonia, whereas one volume of palladium absorbs 643 volumes of hydrogen measured at 17·5, which would exceed 800 volumes measured at 97°, the temperature of the experiment.

Now, this absorption of soluble gases is manifested, not only in the case of liquids, but also in the case of soft solids; those which Mr. Graham has denominated "colloids." Accordingly, if we take a little hard white of egg, a substance which is not wet in the ordinary sense of the word, and if we pass it up into ammonia gas, you see that the white of egg absorbs the ammonia gas with almost as much rapidity as did water itself. Now one possible view, in connection with the absorption of gases by heated metals, is that these metals are in what may be termed a colloid state—that is to say, that they are soft bodies, or imperfect solids, and that a species of low form of chemical action takes place between the metal and the gas, allied somewhat to the low form of chemical action which leads to solution, or to the absorption of gas by soft solids, such as white of egg and india-rubber.

There is one further point to which, in the few minutes that remain to me, I wish to draw your attention, and that is the absorption of gases by such substances as charcoal, for it is a question whether the absorbtion of gases by heated metals may not be allied in some degree to this well-known phenomenon; and certainly there are some facts, at any rate, which seem to indicate a very close relationship between the two acts of absorption. If we take a piece of compact charcoal, and pass it under metallic mercury into a cylinder of ammonia gas, we see in this case a somewhat rapid absorption of the gas; and I have no doubt that, in the course of a few minutes, the piece of charcoal will absorb the whole of the gas originally existing in the cylinder. Now, we know that this absorption of gas by charcoal varies considerably with the temperature of an experiment, the nature of the gas, and above all with the texture of the charcoal employed, just as does the absorption of gas by metal. Thus, in the case of platinum, the fused metal absorbs but a very small quantity of hydrogen, the spongy metal absorbs a larger, but still a comparatively small proportion; whilst the feebly porous metal, in the form of wrought platinum, alone manifests the property of absorption in the highest degree. Again, it is well known that the gases absorbed into charcoal have their chemical properties intensified, just as I have shown you with regard to the hydrogen absorbed by palladium. Further, the property of charcoal to absorb different gases is intimately related to its property of exerting a selective absorption of liquid and dissolved substances. Now we find this property also manifested by palladium. Thus 1,000 volumes of palladium foil were found to absorb 1 volume of water, 5½ volumes of alcohol, and 1½ volumes of ether, results showing a special relation of palladium to those different liquids, corresponding to the action manifested by charcoal. Moreover, this selective absorption of different liquids by a sort of capillary affinity, brings the absorption of gases by palladium into relation with some very familiar phenomena, at first sight of a widely different character, namely, the selective absorbtion of dyes by different tissues, and the action of mordants; the property which charcoal has of abstracting various matters from solution, being closely allied to that by which certain tissues and mordants abstract colouring matter.

There are many other points of very considerable interest, in relation to this subject, which I should be glad to bring before you, were not my time already expired; but there is one point of practical importance, connected with the absorption of gases by iron, which I ought not to omit, namely, the bearing which these facts have upon the conversion of iron into steel. Steel is manufactured, as we all know, by the application of charcoal to bar-iron at a certain temperature. The charcoal, it is true, gets partly converted into carbonic acid and carbonic oxide, but it is only the surface of the iron which is in contact with either the charcoal or its oxides; nevertheless the conversion of the metal into steel takes place through its entire mass. Now, it has hitherto been a subject of great difficulty to explain this penetration of the carbon, or carbon-oxides, into the centre of the iron bars. But we now see that iron, like a colloid or porous substance, has the power of absorbing carbonic oxide gas to its very centre. And it would seem that the process of acieration—the conversion of iron into steel—really consists in an absorption of carbonic oxide by the metal, and a subsequent decomposition of the absorbed carbonic oxide, into carbon, which effects the conversion of iron into steel, and into carbonic acid gas, which escapes from the surface of the metal, and gives rise to the blisters by which freshly made steel is characterised. The eliminated carbonic acid then takes up more carbon, to become reconverted into carbonic oxide, which the metal again absorbs, and so on continuously until the process is completed.

In conclusion, it only remains for me to express what I am sure we must all feel—our sense of indebtedness to Mr. Graham for his admirable investigations, which have not only added largely to our knowledge of the transmission of hydrogen through ignited platinum and iron, observed by M. Deville, but have gone very far to explain the nature of the phenomena, by showing that they differ altogether in character from the phenomena of diffusion, but are preceded by an occlusion and probable liquefaction of the gas in the substance of the metal, somewhat similar to the occlusion of soluble gases by water, or of absorbable gases by charcoal, in virtue, probably, of a low form of chemical affinity subsisting between the gas and the metal. They have brought to light the startling fact of the occlusion of some of the lightest gases by some of the heaviest metals, to the extent of several times their volume, and, in the case of palladium, to the extent of several hundred times its volume. They promise, moreover, to throw great light upon a very important branch of manufacturing art—namely, the conversion of iron into steel. They have also given us a new illustration of the strange relationship so frequently existing between apparently the most remote phenomena, as the acieration of a piece of iron and the dyeing of a piece of silk. And, lastly, in the case of the meteoric iron, they have afforded us a further demonstration of the oneness of the universe, of the extension of one chemical system throughout the entire cosmos.

A Course of Four Lectures on Spectrum Analysis, with its Applications to Astronomy. By William Allen Miller, M.D., F.R.S., *etc.*

Lecture IV.

(Concluded from page 142.)

Spectra of the Fixed Stars.—Mode of Observation.—Double Stars.—Variable Stars.—Temporary Bright Star in Corona.—Nebulæ.—Clusters.—General Conclusions.

A great number of other stars have been examined. I cannot, however, attempt to give you any idea of their composition in detail, but must refer you to a list which you will see here, giving the names of the more important stars, which we have examined more or less completely. Among the most interesting of the stars we have examined are Sirius, Arcturus, Capella, Vega, Pollux, Castor, Cygni, Procyon, and α β and γ Andromedæ, Rigel, Spica Virginis, α Aquilæ, Cor Caroli, Regulus, and others. The general result of these investigations shows that the stars are bodies, formed upon the same plan as our sun, each differing in composition from its fellows, but all apparently containing matter, some portion of which is identical with that composing a part of our own globe.

One or two of these stars may be advantageously referred to more fully. Among these a red star β Pegasi is much like the star α Orionis in its general character, but it is much fainter. It is one of the third magnitude. Here, we have been able to measure only twenty lines. We can see that it is full of lines, but the uncertainty of the atmosphere and the difficulty of seeing these lines with precision have prevented us from accurately fixing the places of a larger number. It contains sodium and magnesium—two of the same bodies which are present in Aldebaran; and besides that, it is probable, though we have not been able to verify all the principal lines, that barium, iron, and manganese are present.

Let me now call your attention to the colours which the stars exhibit. It is a matter of familiar observation that the stars differ in their tints. This difference appears to be caused, in many cases, by the atmosphere outside the photosphere, which causes an absorption of certain colours contained in their light, and in consequence of this we have a difference in the tint. It is remarkable that in some of the red or orange stars, like β Pegasi, and others, hydrogen is absent, whilst in the white stars this element is predominant. In the spectrum of the important star called Vega (α Lyræ), sodium, hydrogen, and iron have been found. It might be supposed that the star Sirius, the brightest of all the stars, would have given us more information than all the others; but it is not so. I will throw its spectrum upon the screen, and you will see that it is remarkable for the absence of strong lines. The light of Sirius is white. There are only three important lines in its spectrum, and these correspond exactly in position with the lines of hydrogen. With these exceptions, the lines of Sirius are feeble. I do not mean to say that there are not other bodies in the stars besides hydrogen, but the proportion of those other bodies must be so small, and the vapour so dilute, that the whiteness of the light is not subdued by them in any remarkable degree. Here are lines which correspond to F and C; here is a line in the blue; and here is one which we constantly find in all the stars, the double sodium line D. Here is the magnesium line; and here is one which is probably due to iron.

Let me now call your attention to the varieties in colour which the stars exhibit. Sirius, as I have said, is a white star. There are stars which have an orange, or yellow, or ruddy tint; and, again, there are others which are blue, or green, or purple.

The examination of these coloured stars is often a matter of great difficulty, because in general their luminosity is small, and frequently these coloured stars occur in pairs, constituting what are known as double stars. It often happens that a bright orange-coloured star has a faint blue or green star as its companion; and this companion is often so close that it is a matter of considerable difficulty to separate the spectra of the two stars, though we can see them in the telescope distinctly enough. In order to separate the spectra of two stars, it is needful so to arrange the motions of the telescope that the spectra shall be always at right angles to the line joining the two stars, so as to maintain the two spectra parallel to prevent them from overlapping, and so interfering with one another. I will take first an orange-coloured star, the brighter of the double stars which constitute Hercules; in the spectrum of this star on the screen, you will see the orange and yellow predominating, whilst other portions of the light are subdued in consequence of the presence of a large quantity of absorbent matter, which stops certain rays in the blue and violet portions of the spectrum. You must bear in mind that in throwing these images upon the screen we are under a considerable disadvantage, because though they are tinted to give you an idea of the relative position which these lines occupy in the spectrum, they do not by any means represent with accuracy the colours which the star itself would show. You will see in the red part of the spectrum there are three or four strong lines, but in the orange and yellow there are comparatively few, so that the star shines with an orange light in consequence of the absorption of the green and blue portions.

I will now take one of the double stars, β Cygni, and project an image of the orange star, with its blue companion, on the screen. The orange star is the most brilliant one. The blue star is so faint upon the screen that you can hardly see it from a distance. You may imagine from this what is the difficulty in seeing, and still more in measuring the position of such faint lines as these. Let us now examine the spectra of the two stars. That of the orange star is characterised by a large number of bands or lines in the blue and green portions, and comparatively few in the yellow portions. In the spectrum of the blue star we have scarcely any lines in the blue, but a large number of lines in the yellow and orange, and in some parts of the red.

We have here, then, examined two classes of stars—some which have a considerable brilliancy, and others in which we have them varying in colour; but there are other variations in the stars, respecting which the spectroscope, we may hope, will at some time or other afford information. Some of the most remarkable amongst the stars are variable in their lustre. At times they shine out with a high brilliancy, and at others they become reduced and almost disappear from view. One of the most interesting problems demanding solution in the nature of stellar bodies, is the explanation of this singular periodical variation. Sometimes these periods are tolerably regular, occurring after a few days, or a few months, or sometimes after years. At other times the periodical variations in the star are irregular; that is to say, though the light of the star waxes and wanes in intensity it does so at irregular intervals. One single observation seems to show, so far as it goes, that there is hope of further progress in this direction. The star α Orionis, one of the first whose spectra I projected on the screen, is a variable star. It is one, however, of which the variation is not very wide. It varies, perhaps, half a degree in magnitude. We found, on making observations when it was at its maximum, that a certain group of lines which were observed two years before, when it was at a low state of illumination, had disappeared. Their position had been carefully measured during a period of minimum, but these lines were not found when the star was at its maximum.

There are, however, others falling under this class of variable stars which are still more remarkable. They have been called temporary stars. One of the most prominent of these was noticed on November 7, 1572, when Tycho Brahé, in returning one evening from his observatory, saw persons gazing at a star which he knew was not visible half an hour before. It continued to increase in brightness for some weeks, but in the course of a year it gradually dwindled away. This star has been altogether lost sight of, and no one knows its true position. In October, 1604, a star, equally bright, appeared in Serpentarius. It almost rivalled Jupiter in brightness. It afterwards faded away and disappeared. In the year 1848 Mr. Hind observed a smaller star, in Ophiuchus, of a ruddy colour, which came out and disappeared in the same sort of way. Lately we had an opportunity of watching one of these stars, which has thus blazed out, and rapidly died away. It was even more brief in its blazing forth, and more rapid in its disappearance, than any of the others; but, happily, at the moment the opportunity offered, we were prepared to examine its light by means of the spectroscope. Mr. Birmingham, of Tuam, first saw it in Ireland, on the 12th May, 1866. He informed my friend Mr. Huggins, who received the news of its presence on the 16th May, and by the same post, Mr. Baxendell, of Liverpool, directed his attention to the star. No time was lost. On the very same evening on which the intelligence arrived, as it happened to be a fine night, we directed the telescope to the spot, and we were able to discover a most remarkable state of things. I shall project on the screen the image of this star. It blazed forth in Corona. About the time Mr. Birmingham saw it, it was of the second magnitude. It occupied the position of a star of the ninth or tenth magnitude noted by Argelander. You will observe that the spectrum of this star is crossed as usual by a number of black lines in the luminous coloured portions; but, besides that, you will see some very bright bands of light—four strong lines, and a fifth much fainter in the blue. We had never seen such lines in any solar or stellar observation before, and they are evidently connected with a new, and, as it turned out, a transient state of things. Two of these lines occupied the position of the lines of hydrogen. One corresponding to the line C in the solar spectrum, and another to the line F. There are two strong brilliant lines in the blue, the nature of which we were not able to ascertain. The duration of the outburst till it dwindled down to a star of the seventh or eighth magnitude, was but a week, so that the opportunities of making observations and measurements were but few. In this star, then, we have three different spectra. First, we have a photosphere giving out a continuous spectrum, and over that we have the usual state of the solar and stellar atmospheres; that is to say, an atmosphere filled with luminous vapours, capable of absorbing a part of the light behind: and then, outside that, more brilliant still, is a luminous spectrum of glowing gas, as though the star was suddenly involved in the flames of hydrogen combining with some substance the nature of which we do not know, but which may be—probably is—connected with the two unexplained brilliant lines in the blue. I say this star, at the time of this outburst of light, was in a condition in which a sudden and violent action upon the hydrogen occurred, in consequence of which, probably the whole mass of the star was raised in temperature, and its luminosity consequently was increased, but the temperature of the mass of the star was not brought up to the same point as the temperature of the hydrogen by which that incandescence appears to have been produced. What the origin of this hydrogen was, of course it would be vain and idle to speculate. But the presence of hydrogen is certainly revealed by the observations just described. Moreover, the hydrogen must have been in a state of active incandescence which may be supposed to be the cause of the luminosity of the star, and just as a glowing ball of lime is heated up in the nearly lightless flame of the oxyhydrogen jet, so the nucleus of this star was lighted up by an outburst of hydrogen suddenly brought to an intensely incandescent condition. It is clear that this observation could not have been made without the aid of the spectroscope, for on viewing a star through the telescope all that can be seen is limited to variations of colour and brilliancy; we can never magnify a star so as to produce anything more than a point of light. We can never get a disc. We have simply an increase in the intensity of the light as we increase our telescopic power. Among the most remarkable of the variable stars is η Argus, the spectrum of which seems to demand careful study.

I must now pass on to a totally different series of objects —the *nebulæ*. Scattered over different parts of the heavens are a number of remarkable bodies which look like patches of light or luminous clouds. In some cases they are collected into rings; in others into spirals, whilst in other instances they assume still more definite forms. These nebulous masses of light have ever since their discovery excited in a high degree the wonder and curiosity of those who have examined them. The interest they awaken is perhaps still further increased by a remarkable speculation concerning them put forth by Sir William Herschel, when he asked whether it was not possible that these nebulæ might be the primordial forms of matter from which stars and suns and their attendant planets had been produced. Notwithstanding minute and careful observations by the telescope, nothing was known of the physical condition of the matter comprising these nebulæ. It was not even known whether they were aggregations of stars so infinitely distant from us that we could not discern the separate stars, or whether each nebula was a separate luminous object of a nature entirely different from the stars. Mr. Huggins has been enabled in several instances to show that the nebulæ are not stars, but that they are composed of glowing gas; and, farther than that, he has been enabled to give some hint as to what this gas may be. I must now ask you to look at the photographs of some or these bodies, which I shall throw upon the screen; many of them have been taken from the beautiful drawings of Lord Rosse. When Mr. Huggins was examining one of these nebulæ with his spectroscope for the first time, he observed what appeared to be a single vertical line of light, this on closer inspection was seen to be accompanied by two fainter lines in the more refrangible portion. This observation immediately gave him the key to the nature of these bodies. The first nebulæ that he examined was a comparatively brilliant one in Draco (37 H iv.). The nebulæ are usually referred to by letters and numbers, which represent the position of these bodies in certain catalogues drawn up by astronomers of eminence. This nebula in Draco, then, instead of giving a continuous spectrum, crossed by black lines like the stars, was found to give three lines of light only, and the greater part of the light was concentrated into one of these three lines. Indeed, had it not been for this circumstance, the light of the nebula is so excessively faint that it would have been impossible to see it at all as spread out in a continuous spectrum, and, in fact, my friend scarcely expected that it would be possible to get any accurate observation in consequence of the faintness of the light. The result, however, amply rewarded him for his trial. You observe upon the screen a spectrum consisting of three lines only. They are here exaggerated very greatly in brilliancy, for the purpose of rendering them visible at all. Below the representation of the spectrum of the nebula on the diagram are the bright lines which correspond most nearly with its lines. This line F corresponds exactly with the faintest of the three lines in the nebula. It is very difficult in diagrams of this kind to preserve the true relation in brilliancy of these lines to each other. At the less refrangible end there is no line corresponding to the red line of hydrogen. An important observation made by Plücker upon rarefied gases in tubes may have a bearing on this point. He found that as hydrogen becomes rarefied, its red line disappears. Hence the absence of the red hydrogen line in the nebula may be connected with the attenuated condition of its gaseous materials. In the diagram you will see three other lines occurring in the solar spectrum. Those are lines which correspond to the group of magnesium lines. The brightest and most refrangible line in the spectrum of the nebula corresponds to one of the brightest lines of nitrogen. It is remarkable, however, that the other lines of nitrogen are absent. This second line in the nebular spectrum does not correspond to any of the lines that are known to us, but the nearest known to it is one of the lines of barium. It must not be supposed that all the nebulæ are exactly like this one. Of the sixty which Mr. Huggins has examined, about twenty exhibit the bright lines due to matter in its gaseous state, all of which contain the bright line corresponding to that of nitrogen; in some the other fainter lines are not seen. Such spectra, therefore, are not produced by a group of stars; not by a substance like any others that we have seen hitherto in the heavens; they must be furnished by masses of glowing gas, which are giving out light of these three particular degrees of refrangibility.

Besides these true gaseous nebulæ there are a vast number of others, but in some of them the luminous material is connected in such a way that they appear in the telescope like clusters of minute stars, or they look as if condensed into little points of greater luminosity than others. Now, when such clusters are examined by the spectroscope they are found to furnish continuous spectra crossed in some cases by the same light lines produced by the nebulæ which are wholly gaseous, so that here the nebulæ appear to consist of two portions, one still gaseous, the other undergoing a sort of condensation. One dares hardly to speculate upon these matters in the present state of our knowledge. It is so easy to speculate and get wrong, and thus to fix notions in one's mind that it is not easy to get rid of again. The truly philosophical course here is simply to form such a hypothesis upon the facts observed as shall serve to guide us in our further investigations.

My friend, Mr. Huggins, has placed a large number of his beautiful drawings of nebulous bodies at my disposal. I can only select one or two for exhibition on the screen. There is a nebulæ which has a remarkable shape like that of the planet Saturn. All these nebulæ have a particular faint bluish green light. Here there are the same three lines. Here, again, is a remarkable spiral nebula (80 H iv.), which has been very carefully observed by Lord Rosse. You will see the style and character of its construction. But it is also remarkable for its brightness in comparison with some of the others. Besides the three lines previously noted we observe a further bright line which is more refrangible than the others. The gaseous character of this nebula is distinctly seen. I will now take a nebula which is not gaseous, and which is visible to the naked eye—the well-known nebula in Andromeda. There is a concentration of light in its nucleus around which is expanded into a longitudinal form. It does not give a spectrum with bright lines, but a continuous spectrum with certain peculiarities. The orange and red portions are almost absent, and the other portion has a mottled appearance, which it is scarcely possible adequately to represent upon the screen. Here is a table which will give you an idea of the character of the nebulæ examined by Mr. Huggins, and the results of his observations compared with the purely telescopic observations of Lord Rosse, as furnished to Mr. Huggins by Lord Oxmantown:—

	Continuous spectrum.	Gaseous spectrum.
Clusters	10	0
Resolved or Resolved?	5	0
Resolvable or Resolvable?	10	6
Blue or green, no resolvability,	0	4
or resolvability seen	6	5
	—	—
	31	15
Not observed by Lord Rosse	10	4
	— 41	— 19

Here are the results which were arrived at by Lord Rosse by observation with his colossal reflecting telescope. First of all are ten clusters, and amongst these clusters all give a continuous spectrum. None of them appear in the spectroscope to have a gaseous spectrum, so that the observations of the telescope and spectroscope correspond. The second line represents to us a certain class of bodies which, though they have not actually been resolved into separate stars as the others have been, yet appear to be resolved. None of the bodies exhibit gaseous spectra. On the other hand, amongst those which have a blue or green colour there are none which are not gaseous. Then there is a group which are not resolvable, some of which give a continuous spectrum and others a gaseous spectrum.

I have thus endeavoured, however imperfectly I have succeeded, but still so far as time and opportunity admitted, to bring before you some of these spectrum discoveries. The more we pursue studies of this matter the more is the mind carried forward with the desire for further knowledge. Whilst thus from time to time we are permitted to get fresh glimpses into the constitution of the universe, and to trace something more of the infinite mind which has designed the whole, so should we feel the more that we are bound to use the fresh knowledge thus placed at our disposal, not for the glory of man, but, as far as may be, for the glorifying Him who made all things and upholds all things by the word of His power.

BRITISH MEDICAL ASSOCIATION.

Twenty-fifth Annual Meeting, 1867, held in Dublin.

Dr. Charles A. Cameron read a paper, in the Physiological Section, on the "*Assimilation of Gelatine.*"

The author re-opened the question of the nutritive value of gelatine. He considered that the French Commissioners on this subject had gone too far in denying the alimental value of this substance. He pointed out as strong arguments in favour of his views as to the assimilation of gelatine, the facts—that that substance when largely used as food passes off in the form of urea, and is not found in the fæces. As gelatine passes rapidly from the stomach into the circulating fluids of the body, and becomes decomposed therein, it is impossible not to assign to it some useful functions. The author then described some experiments which he had made on this subject. He employed the dog and the white mouse, the latter animal being peculiarly well-adapted for such experiments. He found that these animals could not live on food consisting of gelatine, fat, butter, sugar, starch, and mineral substances. This was the result the author had expected, for the following reasons. In the nitrogenous portion of animals there exist sensible amounts of organic sulphur and phosphorous compounds ("alloxidic"). Gelatine does not include these elements, and is therefore incapable of forming muscles and nerves. The addition of mineral sulphates and phosphates to gelatine does not increase its nutritive value, because animals are not endowed with the power of organizing mineral sulphur and phosphorus, although this function is the attribute of plants.

In some of the fats of the brain and nervous tissue generally there are notable proportions of organic and unoxidised phosphorus and sulphur. By combining these phosphorised and sulphuretted fats with gelatine, and adding thereto some other non-nitrogenous and mineral substances, Dr. Cameron formed a food on which mice lived for forty-two days in a very healthy state, although the said food was deficient in albumen and fibrin.

Taken under ordinary circumstances, gelatine, according to the author, is most probably employed as a calorifacient, and perhaps also as a source of motive power. In an agreeable form it was a food very much relished by invalids, and there was a good reason underlying the popular practice of making gelatine a common article of food.

Mr. Tichborne read a paper, in the same section, on the "*Organic Matter in Potable Water.*"

He dwelt at some length upon the difficulties attendant upon water analyses. The point of difficulty in the examination of potable waters being not so much their quantitative as the qualitative examination—particularly as regards their organic constituents—you may have a water rich in organic matter yet harmless, or a water containing but a minimum of organic matter of the most deleterious nature. He remarked that all the following points in an analysis had an important bearing upon the state of the organic matter, viz. nitrates, nitrites, ammonia, total nitrogen, odour, taste, colour, hardness, chlorides, iron, gases dissolved, microscopic examination, etc. The author gave a quick method for estimating the nitrites in potable water, based on the conversion by heat of nitrite of ammonium into nitrogen and water—the loss being estimated by a volumetric solution of permanganate of potassium. Mr. Tichborne then described a tube or convenient instrument for viewing the colour of water, and for examining the state of oxidation of the iron salts or matter of some importance in connection with the organic matter, and a point not so easy to determine in very diluted state—as the microscope magnifies the form, so this instrument magnifies the colour, or colour tests, so as to be recognisable to the eye. When the iron bears any *considerable proportion* to the organic matter, very little of the latter will be found (if we except ammonia), providing the iron is in the state of a ferric salt. If, however, the iron is present as a ferrous salt, it will be found to have exerted little or no influence on the organic matter, and frequently such waters (except chalybeate springs) will be found to contain large quantities of soluble organic matter.

The author remarked that it had been stated that charcoal that had been used some time, so far from taking from the water the organic matter, gives up again a certain amount. That water on analysis before and after passing through old charcoal was more contaminated with organic matter, after having been passed through the said filtering medium. The *modus operandi*, however, by which charcoal acts, is not so much by any attractive power that it possesses,—that is to say, it does not absorb the mass of the organic matter, but acts as one of the most powerful oxidisers. The oxygen condensed within the charcoal acting more energetically than the available oxygen we can apply in the form of permanganate of potassium. A charcoal filter was found from actual observations to work wonderfully well, and to retain its vitality for a long period, with the following provisions. That the water passed through it did not contain a very large percentage of organic matter. That it was not continuously at work,—that is to say, that the charcoal was exposed to the atmospheric oxygen (with as little of the dust as possible) the better part of each twenty-four hours. That the water was passed through a filtering medium before it came in contact with the charcoal. If not, the charcoal acted as a mechanical recipient to the insoluble organic matter, which at last accumulates to such an extent as to enter into a state of fermentative change; the activity of the charcoal being by this time exhausted, or at least only sufficient to supply a minimum of oxygen, would only assist such a decomposition. This state of the case would be simply the putrefaction of a mass of organic matter independent of the charcoal, not the rendering back from the charcoal of something it had absorbed from the water.

Mr. Tichborne exhibited a table of the action of permanganate of potassium upon organic substances, which illustrated the worthlessness of that substance as a measure of fermentable matter. He also stated that peroxide of hydrogen, if it could be procured *pure*, and at a reasonable price, might be used with advantage, for the purification of water.

The only other papers read at these meetings, possessing a chemical bearing, were: Dr. Protheroe Smith "*On the Mode of Detecting Impurities in Tetrachloride of Carbon,*" and Dr. O'Leary "*On the Thermal Value of Food,*" *etc.*

ACADEMY OF SCIENCES.

JULY 22, 1867.

(FROM OUR OWN CORRESPONDENT.)

Sir D. Brewster on liquid films and figures of equilibrium.—Storms exceptionally caused.—Proportion of iodine in mineral waters.—Classification of meteorites.—Discovery of universal gravitation, letters of Pascal to Boyle; Discussion; Newton and Pascal in correspondence.

SIR DAVID BREWSTER addressed copies of his pamphlets (reprinted frm the Roʎoal Sietocy of Edinburgh) on the colours of thin liquid films and their figures of equilibrium, of which we have already published the analysis. The illustrious professor, foreign associate of our Academy of Sciences, at the age of 85 years, retains his memory so fresh and bright that the two memoirs in question seem to be the offspring of a youthful mind.

M. Fournet of Lyons addressed a note on the exceptional storms caused by the south-west winds.

We understood, vaguely, that there were communications on geological sections of the Alençon railway—of a memoir by M. Thibaut on inundations—of an efficacious method of treating croup, by Dr. Abièlle—of researches on electricity by M. Girard—of a means of ascertaining the proportion of iodine contained in mineral waters and in alkaline salts—of a memoir by M. Humbert on the resistance of a voltaic pile, etc.

M. Daubree read the second part of his memoirs on the classification adopted for the collection of meteorites in the Museum of Natural History. His principal divisions of solid and coherent meteorites are as follows:—1st Class: *Siderites*—meteors containing iron in a metallic state; subdivision, containing stony substances. 1. *Holosiderites*—Meteoric iron properly so called; second subdivision containing iron and stony substances. 2nd Group: *Syssiderites*—iron in the form of a continuous mass. 3rd Group: *Sporadosiderites*—iron in the form of disseminated grains; 1. Sub-Group of sporadosiderites, *Polysiderites*, when the quantity is considerable. 2. Sub-group: *Oligosiderites*, containing a small quantity of iron. 3. Sub-group: *Cryptosiderites* in which the iron is invisible to the eye. 3rd Class: *Asiderites.* 4th Class: *Asiderates.*

M. Le Verrier called upon M. Chasles relative to the letters and notes of Pascal printed in the Comptes Rendus, and of which we give, before resuming this long discussion, a few important passages: —8th May, 1652, Pascal to Boyle—"J'ay pour le prouver un bon nombre d'observations de toutes sortes dont personne m'a encore parlé, et partant en connaissance, tant sur l'attraction et de ses lois avec les phenomènes. Je viens vous en faire part. Vous trouverez cijoint les experiences, au nombre de plus de cinquante.

"PASCAL."

2 Sept., 1654 or 1655.—Again to Boyle.—"Dans les mouvements celestes, la force agissant en raison directe des masses et en raison inverse du quarré de la distance suffit à tout, et fournit des raisons pour expliquer toutes les grandes revolutions qui animent l'univers. Rien n'est si bon selon moy; mais quand il s'agit des phenomènes sublunaires, de ces effets que nous voyons de plus près et dont l'examen nous est plus facile, la vertu attractive est un Protée qui change souvent de forme. Les rochers et les montagnes ne donnent aucun signe sensible d'attraction. C'est, dit-on, que ces petites attractions particulières sont comme absorbées par celles du globe terrestre, qui est infinement plus grands.

"PASCAL."

Note to Boyle.—"Le corps en vertu de la tendance au mouvement que l'attraction lui imprime est capable de parcourir un espace donné dans un temps donné. La vitesse initielle sera donc proportionelle a l'intensité de l'effort ou de la tendance imprimée par la puissance attractive; et cette intensité sera elle-même proportionelle à la masse attirante a egale distance, et (à) differentes distances, comme la masse attirante divisée par les quarrés de ces distances.

"Les observations atronomiques apprennent que toutes les planètes se mouvent dans une courbe autour du Soleil, et qu'elles sont accelérées dans leur movement à mesure qu'elles approchent de ce globe, et qu'elles sont retardèes à proportion qu'elles s'en eloignent, tellement qu'un rayon tiré de ces planètes au Soleil décrit des aires ou des espaces égaux en temps égaux.

"PASCAL."

"J'ai dit que la force de projection qu'on nomme force centrifuge varie continuellement, parce que l'attraction est plus ou moins grande suivant que les planètes s'approchent ou s'éloignent du soleil. Pour concevoir comment cette révolution s'opère, supposons qu'une planète soit à la partie de son orbite (ou de l'ellipse qu'elle parcourt) la plus proche du soleil, la force attractive est dans cet état plus grande que dans toute autre situation, à proportion que le quarré de la distance est moindre. Elle devroit donc faire tomber la planète sur le soleil, mais la force centrifuge produite par le mouvement circulaire autour du soleil augmente en plus grande proportion.

"PASCAL."

"La puissance qui agit sur une planète plus proche du soleil est ordinairement plus grande que celle qui agit sur une planète plus éloignée, tant parce qu'elle se meut avec plus de vitesse qu'à cause que son orbite est moindre et qu'elle à plus de courbure. En comparant les mouvements des planètes, on trouve que la vitesse d'une planète plus proche est plus grande que la vitesse d'une planète plus éloignée, en raison de la racine quarrée du nombre qui exprime la plus grande distance à la racine quarrée de celuy qui exprime la moindre distance.

"PASCAL."

"On peut conjecturer et même inférer qu'il n'y a une puissance semblable à la gravité des corps pesants sur la terre, qui s'étend du soleil à toutes les distances et diminue constamment . . . Le même principe de la gravité doit avoir lieu dans les satellites qui circulent autour de la terre, de Jupiter et de Saturne. . . . Car ils ne pourroient avoir un mouvement aussi régulier qu'ils ont s'ils n'étoient assujettis a l'action de la mesme puissance, etc.

"PASCAL."

The intimate acquaintance of Newton with Pascal is proved by letters written by Newton in most excellent French. Newton was then only eleven years old

The question that M. Leverrier at last put forth, after endless circumlocution, was this:—Is M. Chasles in possession of letters and notes which demonstrate undoubtedly that Pascal knew, not only the laws, but the demonstration of universal gravitation? M. Chasles hesitated to answer, because he would wait till the termination of the discussion of that which he had already spoken of to the Academy; he consented, however, to acknowledge that it results from the authentic autographs in his possession that Pascal had determined centrifugal force in the curvilinear motion of the planet; thus, this determination implies the demonstration of the elliptical nature of the curve described under the influence of attraction.

MM. Duhamel, Elie de Beaumont, Pouillet, Faye, Leverrier and Chasles took part successively in the discussion, but on subjects not bearing directly on the question. M. Duhamel supported the adversaries of Newton; he could never understand how he gave, at so late a period, the mathematical expression of the law of gravitation in inverse proportion of the square of the distance. By replacing the ellipse by its osculating circle, he had early found the geometrical expression of the attraction, but it was necessary to deduce from synthetic considerations the value of the radius of curvature of an ellipse, which he could not do later. This geometric determination is very remarkable, and we shall shortly give further accounts of it.

It was well seen with what care M. Chasles avoided pronouncing the name of Newton in his paper. It is not generally known that when Newton was only fourteen years old, Pascal was in correspondence with Newton, and it was at his advice that Mrs. Newton sent her son to Cambridge. This unexpected announcement is most important, especially when these extraordinary documents prove that Pascal was aware of the laws of universal gravitation. Certainly M. Chasles ought to be left at full leisure to prepare his dissertation, which is waited for impatiently by the scientific world.

M. Chevreul laid on the table two pamphlets relative to Natural History and the Gobelins; the first is a most complete work describing the physical, chemical, and organic properties and relations of all bodies.

JULY 29, 1867.

Pascal's letters forgeries.—Rain-fall in Alsatia.—Experiments on Induction currents.—Surgical instruments from Pompeii.

THE Abbé Zantadeschi presented a copy of a work on the climate of Catania, the conclusions of which are that this country is one of the spots in Italy where the climate is most mild. and it is exactly the place where persons suffering from pulmonary complaints should be sent.

M. Faugére, sub-director of the Minister of Foreign Affairs, who had devoted his life to the subjects of the illustrious Pascal, and the history of his family, who had the good fortune to discover the several precious unpublished documents entitled "Penseés, fragments et lettres de Blaise Pascal," was struck with the account of the letters and notes presented and given to the Academy by M. Chasles; he wished to see these surprising autographs, and was convinced that the signature of the letters deposited by M. Chasles is not that of Pascal, but that they are simply forgeries. He requested the Academy to appoint a commission for as scrupulously as possible inquiring into the authenticity of these documents.

M. Chasles replied that he had no doubt whatever on the validity of the letters and notes presented by him, and that he hailed with pleasure the appointment of the commission demanded by M. Faugére. The members are MM. Chevreuil, president; Delaunay, V.P.; Elie de Beaumont and Coste, secretaries; Chasles, Duhamel, Pouillet, Faye, and Leverrier. M. Chasles disputed the assertion of M. Duhamel, "In admitting the authenticity of the letters deposited by M. Chasles, and supposing even that they had been published before the *Principia*, they would not give the right of priority to Pascal for the law of universal gravitation. The glory will ever rest with Newton." M. Chasles maintains, on the contrary, that the glory of the discovery, and the demonstration of universal attraction proportional to the masses is in the inverse ratio of the distance, belongs to the immortal Pascal. He proved by calling to mind the following of the letters and notes of the illustrious philosopher, and which will create a profound sensation in England.

M. Becquerel presented a note on the temperature of water currents, by M. Ch. Grad.

In two former communications he gave his researches on the fall of rain in Alsatia, and the relations which exist between the flow of water in the Ill, and the rain-fall in its basin; to-day he submitted the result of the running waters in the same region. These observations, repeated twice a day, at 7 A.M. and 4 P.M., were made principally on the Fecht. The Fecht is a sort of torrential river, taking rise in Hautes Vosges at a height of 3,300 feet above the level of the sea. The following are the comparisons made between the temperature of the water and that of the air:—The mean temperature of the water from July, 1866, to June, 1867, was found to be 10°·5 C. inferior, by 0°·8, that of the air at Turckheim. The difference between the temperature, maxima and minima, of the year was 23°·7. The greatest variation was during the month of May, 13°·3, and the greatest diurnal variation, 7°·6, occurred the 14th July. The amplitude of the oscillations is less for water than for air; the temperature of water attains its diurnal maximum and minimum only after the maximum and minimum of the air have been reached. In summer the rains lessen the temperature of the Fecht, and in winter it is raised. Also the range of variations was found to be more decided in summer than in winter; weaker under an overcast sky than in a clear one, and it becomes more pronounced according as we recede from the sources, with the increase of volume of the water-course. This last observation confirms those of M. A. Bertin, on the temperature of the Ill at Strasbourg, and the Lower Rhine at the Kehl Bridge, as shown by the following results:—During the year 1858 to 1859, the average temperature of the air at Strasbourg was 10°·2 C; that at Kehl Bridge 10°·5; the water of the Rhine at Kehl Bridge, 10°·9; the Ill at Strasbourg, 11°·2. These results give to the Ill and the Rhine a higher temperature than that of the Fecht; and this general law can be deduced, that in the *same region* the temperature of running waters increases with their volume.

M. Regnault presented, in the name of M. Blaserna, professor of the University of Palermo, results of experiments on the passage of induction currents. His conclusions are the following:—1. The time elapsed between the closing or the rupture of the circuit and the apparition of the current of induction, or the attraction of the armature for the bobbin of induction is inappreciable, less than the fiftieth part of a second. 2. The current of induction, feeble at its commencement, increases little by little, then diminishes, and is extinguished in an interval difficult to determine.

M. Elie de Beaumont gave official notice of the election of M. Wurtz in place of M. Pelouze. late master of the Paris Mint, and called upon him to take his chair.

Dr. Scoutteten, principal physician of the army of France, read a paper on the surgical instruments, probes, specula, etc., found in the ruins of Pompeii. He mentioned, especially, one instrument, a probe, presented by Gallien to a magistrate named Erastitrate, and which seems preferable, in a practical point of view, to those new in use.

AUGUST 5, 1867.

Fall of Aerolites.—Ozonometry.—Oxide of Thallium a Test for Ozone—Theory for Solar Spots.—Wax of the Fig Cochineal.—Moulting of Fishes.

THE minister of public instruction transmitted, on behalf of the Algerian Government, several documents relative to the fall of aerolites which took place on the 9th of May, 1866, preceded by the appearance of a *bolide*, or meteor, which exploded with very loud reports.

Dr. Berigny presented a memoir on ozonometry, being a *resumé* of observations made during a period of nine consecutive years with iodized paper much more sensitive than that of Schönbein, and a scale of shades drawn up in concert with M. Salleron. The following are the principal results obtained at Versailles; the month of May is that during which the maxima are absolute, in November the absolute minima take place. At the equinoctial periods, March and September, ozone attains its relative maxima. The importance of the months is ranged in the following progressive order: May, March, April, June, August, July, September, January, December, October, February, and November. The comparison of the ozonometric curves with the meteorological charts of the observatory show that a maximum of ozone corresponds with the presence of a storm in Europe, or the Atlantic, or on the coasts of France and England. Certain minima follow the same law; but then it happens always that the storm is thrown back towards the south before reaching the meridian of Paris, and that it traverses Spain and the Pyrenees to extend itself over the Mediterranean. The coloration is generally very strong when the storm traverses France and England; it is also produced when it passes very high in the north. It varies with the intensity of the atmospheric movement, and with the distance at which the centre of this movement passes.

Dr. Besigny laid on the table a copy of the researches made on ozone, read by M. Schönbein at one of the meetings of the Scientific Association of France, held at Metz. The following is an extract:—Ordinary oxygen is without action upon the protoxide of thallium, while ozonized oxygen combines rapidly with this oxide so as to form the peroxide of thallium, which is brown. Paper steeped in a solution of oxide of thallium and exposed to free air, would be an excellent ozonometric paper if the carbonic acid of the air did not transform the oxide into carbonate, which passes more slowly to the state of peroxide, and blackens with difficulty under circumstances where strips of paper, iodized and starched, become coloured at the end of a few minutes in an atmosphere which contains a 1-2,000,000th part of ozone. The comparison between the two papers has at least the advantage of proving that the coloration of the iodized paper is really produced by the atmospheric ozone.

In an answer made by Mr. Faye to the objections urged, in April last, by M. Kirchhoff, against his theory of solar spots, he stated the question in the following terms:—"Admitting that the solar spots are simple 'daylights' (they are assuredly only cavities), in the luminous clouds which constitute the photosphere, to explain how it happens that we do not perceive through the cavity the whole body of the sun (150,000 leagues thick), the internal face of the photosphere on the opposite side of the celestial body in all its brightness." Again taking up the discussion, M. Faye calculates the tajectory of the luminous ray starting from that portion of the photosphere opposite to the eye which regards the spot, and thinks that he can prove by a very simple analysis, by supposing the obscure mass of the sun to be perfectly transparent, this portion of the photosphere would remain invisible. The reason of this is that the luminous ray would describe a spiral curve without ever coming out. Examining, then, the English theories, which pretend to explain the spots on the sun by its atmosphere, the influence or position of the planets, the intervention of cometary or meteoric matter, he demonstrated that these three causes are totally insufficient and inadmissible, inasmuch as they imply the existence round the sun of an atmosphere the thickness of which is one-third of the solar radius.

M. Blanchard presented a note from M. Targioni Tozzetti, professor at Florence, on the wax produced by the fig cochineal (*coccus coricæ*), which contains half its weight of ceroline, and can be abundantly procured and used in the industrial arts.

M. Blanchard also presented a note on the moulting of fishes, by M. Baudelot. Tubercules are often observed on the skin of fishes, accompanied by the falling off of the scales; these were sometimes considered a characteristic of a new species of fish. M. Baudelot has found that they are periodical, and are to be found in certain seasons of the year, thus constituting a true moulting.

NOTICES OF BOOKS.

Observations upon a New and Simple Process for the Preservation of Meat, Fish, Poultry, and other Varieties of Animal Food. London: Simpkin, Marshall, & Co.

A NEW process for the preservation of meat by means of a solution of bisulphite of calcium, has been lately patented by Messrs. Medlock and Bailey; and the pamphlet we are about to discuss has been written with the object of describing the process and its application. It is somewhat unfortunate that the writer adopts the style usually displayed in the commencement of such descriptive pamphlets. He proves satisfactorily to himself that all the processes hitherto employed for the purpose to which this patent is applied are lamentably defective. It is needless to mention the objections raised in each particular case, and it is much to be regretted that the author does not confine himself to a statement of the undoubted advantages possessed by the special process which he advocates, without drawing invidious comparisons.

The preservative employed by Messrs. Medlock and Bailey, is undoubtedly a very powerful one, and although its application to the purpose of preserving food may be new, the antiseptic properties of sulphite of calcium have long been recognized; it is, indeed, one of the chief components of McDougall's powder. In the patent now brought before us, however, the more soluble bisulphite is employed. This compound possesses several advantages over other sulphites, which will strike chemists at once. It is easily obtained free from sulphate, and if any sulphate should be afterwards formed by oxidation no unpleasant taste would be noticed by the consumer; these points have probably operated against the extended use of sulphite of sodium. The low equivalent of calcium is also somewhat in its favour. Messrs. W. Bailey & Sons have engaged to manufacture the bisulphite of calcium, and guarantee to deliver it absolutely pure. With regard to the results that have been obtained, as set forth in the pamphlet, they are remarkably good. The ordinary preservative solution is made as follows:—Dissolve about a pint of common salt in four gallons of clear cold water, to which add half-a-gallon of the bisulphite, and mix well: if the meat etc., to be treated is required to be preserved for a very long period, a little solution of gelatine or white of egg may be added with advantage. All kinds of meat may be kept perfectly sweet by simply soaking the joints in the above preservative solution for ten minutes, and then hanging them up, wetting them again with the solution once a day.

It is stated that beef, mutton, lobsters, etc., treated by this process, kept good for twelve days, with the temperature varying between 80° and 110° F., the original odour and flavour remaining unimpaired at the end of the time. In twenty-six hours portions of the same animal matter, unprepared, were absolutely putrid,

The process seems likely to prove valuable both to the public and the patentees.

Gas Manipulation. By the late HENRY BANNISTER. Enlarged by WILLIAM T. SUGG. London: Henry Sugg, 32, Henrietta Street, Covent Garden. 1867.

OUR most eminent chemical authorities have lately interested themselves so deeply, not only in the actual quality of coal-gas, but also in practical details whose consideration is necessary for understanding how impurities appear, persist and accumulate, that, as a result, gas manipulation has become a branch of study that may be called chemical jurisprudence. Thus, within a very few years, by laborious investigations of chemists, the subject of gas supply, with the impurities of gas, has been raised to a degree of accuracy equal or even superior to that of the sister subject of water supply, with its impurities. These two subjects, with histories so very different, have reached a nearly identical climax, in that the chemist and the engineer will, for the future, have nearly an equal weight of evidence to give in the witness box, on questions touching either subject. A branch of the science is thus steadily growing in importance, and we should welcome with much pleasure a short text-book on this department of chemical jurisprudence for the busy practical man.

Such a work would now, thanks to the enterprise of publishers, and the increasing number of text-books on all these subjects, be a mere matter of compilation, giving in one volume information of the most valuable character, now scattered in scores, or even hundreds of books and pamphlets, which matter, we have much reason to fear, will be quite lost to the profession if not properly secured in time.

In the absence of such a book, however, Bannister's, or rather Sugg's gas manipulation (so greatly has the original book been enlarged and improved), will for many years be a valuable text-book for the chemical jurist; this could not so justly be stated of the former edition.

With regard to some minor points, in fairness to our readers, we must make a few remarks and extracts;—thus the use of turmeric paper for testing the presence of volatile alkali, is advocated without any mention of the fallacies that attend its use; the very great superiority of red litmus paper in this respect has generally been insisted upon, especially by Mr. Bowditch. Again, for the quantitative estimation of ammonia, the decimally divided imperial liquid measure is used, which, however good in its way, has not the sanction of extensive use by chemists. With an English and French method in vogue of nearly equally extensive use, there is no room even for "a golden mean" system, as its advocates would call it. In any estimates made in legal statements, a very simple question would become infinitely involved by the necessary explanation that one cubic inch = 36·06543 septems, and that 1 litre = 2·2 deci-gallons. We think that by quoting one example, the volumetric testing by this method will be seen to be decidedly inferior to more recent methods.

"For testing ammonia by oxalic acid"—"the following solutions are required." 1. A solution of oxalic acid, 100 septems of which are equal to 10 grains of ammonia; 2. A solution of ammonia of which 100 septems contain 1 grain of ammonia. 3. Tincture of hematine; for this septem-burettes, etc., are used; the results thus obtained are not at all readily calculated either into grain or gramme measures.

In another part of the book, fluid ounces and drachms are used for volumetric purposes.

A valuable portion of the work is devoted to the question of photometry, which is most skilfully handled. Two full-paged illustrations of the photometers of Bunsen and Dr. Letheby respectively are given in white lines on a black ground, and the doctrine of irradiation as connected with luminous impressions, is very markedly illustrated on a large scale by these beautifully clear prints. 15 others engravings are also given on toned paper, equally artistically executed; and we have no hesitation in saying that this is the most perfectly illustrated scientific work that we know of, published in England.

We leave to our readers the pleasure of reading the details of the self-registering photometer, by which with the aid of photometry the varying pressure of gas may be registered during the 24 hours.

It has been named after the workers out of the idea, Messrs. Kirkham and Sugg, who acknowledge the valuable assistance rendered by Mr. Glaisher.

To those of our readers who have studied Mr. Bowditch's experiments on the value of illuminating candles, which seemed of such a convincing nature, and have remembered Dr. Frankland's remarks on the same subjects, the following extract from the preface will be of interest;

"The variations so frequently found to occur when using the standard candle in conducting experiments, are shown to be in a great measure attributable to a defective mode of application; and suggestions, based on actual practice, are offered, the adoption of which will ensure as great an approximation to the truest results as the present Parliamentary regulations will admit of."

Propriétés Desinfectants des Permanganates Alcalins. Henry Bollman Condy. Paris: J. B. Baillière et Fils. 1867.

The readers of the Chemical News doubtless remember the tenor of some remarks made in this journal under the heading, "French Academicians and English discoverers;" from our later numbers also they have learnt that Sir Isaac Newton himself is not to escape; and although we are sorry that Mr. Condy should have occasion to assert his right of priority of discovery, we congratulate him heartily at finding himself in such good company, and ourselves for this opportunity of learning the whole history of the application of the permanganates to disinfection, afforded by the necessity of publishing such a claim.

Mr. Condy observes truly, that although M. Marguerite in 1850, and MM. Bussy, Florès-Domonte, and Péan de Saint Giles still later, advocated the application of the permanganates and manganates for oxidizing purposes; they did so merely for purposes of chemical analysis; and the proof of commercial application being still unrecognized, is found in there being no exhibition of these salts at the French Exhibition of 1855. Towards the end of this year Mr. Condy first showed these disinfecting powers. We have in Appendix A the most complete verification of Mr. Condy's views from the highest authority, Professor Hofmann; this document is dated July 21, 1856. In spite of this M. Castex claimed, in 1863, from the Academy of Sciences, a recognition of his discovery of these properties. Mr. Condy thereupon, by Dr. Mitchell, addressed to the Academy a most unanswerable reply to M. Castex's claims. This called forth in reply a letter addressed to the Academy by a Dr. Blache (March 24, 1864), and this will be for some time memorable in the history of invention as an unexampled specimen of cool impertinence; we quote—

"Et puis nous ferons remarquer que ce n'est pas l'idée seule *qui fait le merite* d'un travail, surtout d'un travail qui a un but pratique; c'est la poursuite de cette idée dans les faitt . . . . M. Castex a experimenté par lui-même; il nous fais connaître des faits puisés dans sa propre pratique. Ces faits sont bien à lui. Ces faits ne sont pas ceux de M. Condy; et s'ils confirment ceux de M. Condy, tant mieux"!!

Dr. Blache admits the discovery, but disputes the full demonstration by a series of experiments, in effect, if not directly, by attributing such a series of experiments to somebody else.

The history of this case may be taken as a pattern of the flimsy way in which French chemists have lately been in the habit of making discoveries. We can discover nothing in Mr. Condy's language that shows anything but the strictest courtsey, truth, and confidence in the justice of his claim.

Elements of Chemistry, Theoretical and Practical. By William Allen Miller, M.D., LL.D., Treasurer and Vice-President of the Royal Society, Vice-President of the Chemical Society, Professor of Chemistry in King's College, London, etc., etc. Part I., Chemical Physics. 4th Edition, with additions. 1867. (Longmans.)

For the last dozen years Professor Miller's book has been the most important of English manuals of chemistry. Smaller and cheaper works have indeed very frequently supplied its place to the beginner and the non-professional student; and the advanced chemist has been constantly compelled to resort to the elaborate, though somewhat inconvenient, handbook of Gmelin. But for ordinary use by the advanced student, the manufacturer, the general reader, and the professional chemist, "Miller" has been the almost invariable guide. It is looked upon by most English chemists with a kindly regard, derived partly from familiarity, but even more from gratitude. No one can estimate the importance to the progress and spread of a science, of a well-written, lucid, and comprehensive manual. Sciences which are not so provided are sure to languish, if not in development, at any rate in popular estimation and general cultivation.

Very peculiar qualifications are necessary for the successful compilation of such a manual. The author must be a man of original genius as well as of deep and varied knowledge. He must be unprejudiced and tolerant in his judgment, able to see the fragmentary truth in two contending views, and able, also, to sift contending evidence—the amount of which is unfortunately always immense,—and, without dogmatising, to note the probabilities to which it points. And, finally, to render his labours of much avail, he must be a man of mark in his science—one whose researches and experience are sufficient to stamp his opinions with some authority. It is not good that he should be too closely bound to any one of the fleeting systems of science. He must be an exponent, not an advocate, using new theories and crude, half-proven facts with the cautious hand of one who knows how soon they may be swept away. His duty it is to write,

> "Not clinging to some ancient saw;
> Not mastered by some modern term;
> Not swift, nor slow to change, but firm
> And in its season bring the law
> That from Discussion's lip may fall—"

surrendering the most cherished of his convictions, the most habitual of his modes of expression, the moment the growing mass of scientific fact shall point it out as desirable.

It is unecessary to point out how well Professor Miller fulfils these hard conditions. There is probably no chemist in England less dogmatic in his tone of thought or less wedded to any particular system. The present edition of his "Manual" affords, we are happy to say, a most convincing proof of this, if any proof were still wanting. Just at the juncture when the interests of chemistry required it, when the revolution which began with the petty insurrection of $\Theta=16$ against $O=8$, is completing itself, and when something like consistency is once more apparent in the language of chemical journals, this new edition appears, and we find that the author, instead of endeavouring to patch up an effete doctrine, has given in his adhesion to the new, and formally ranged himself under its banners.

Every chemist knows the book so well that it would be absurd to offer more than a few passing comments upon it. The present volume, occupied almost entirely with physics, is but little changed by the alteration in nomenclature and notation. The introductory chapter has been chiefly re-written, and although the author, with characteristic caution, stops short of the extreme development of the most modern theorists, his sketch of the laws which govern the operations of chemical force is perfectly in accordance with modern doctrine, while it preserves the admirable lucidity which distinguished previous editions. As in Hofmann's little book, the term "equivalence" is used instead of "atomicity," which is decidedly barbarous, and is, moreover, open to the charge of implying more than can be proved.

The general plan of the volume has been but little altered, though a good many additions have been made to it. The section on the photographic action of light has, very wisely, been transferred from the second volume, and is a most valuable summary of the present state of knowledge upon the subject. Spectrum analysis is, of course, enlarged, and is prefaced by a useful sketch of the history of the subject. The recent researches of Dale and Gladstone, and of Landolt, upon the connection of optical properties with chemical composition, are shortly described. The wonderful results of Graham on the absorption of gases by metals have been inserted, although his last extraordinary experiment, in which hydrogen was separated from a meteorite, was not announced until the volume was in print. In the chapter on electricity we find clear descriptions of the magneto-electrical machines of Holmes and Wilde, although the curious electrical machine of Holtz is unaccountably omitted. The conclusions of the British Association committee on the standard of electrical resistance have been inserted in some detail, well warranted by their extreme importance.

The above are, we believe, the most important of the additions which distinguish this edition. Of course there are a few omissions to notice: it would be a strange thing if there were not in a work upon so wide a plan. We regret the absence of Stokes's beautiful and simple methods for the spectroscopic examination of liquids, and their extensions by Sorby, though perhaps the latter belongs more properly to microscopy. And, to select another illustration, it would surely have been well to have given some account of Balfour Stewart's striking experiment on the rotation of a disc *in vacuo*, for although its present explanation must still be regarded as hypothetical, it is in the highest degree suggestive. These, and a few more of the same kind, are, however, but specks in a most excellent work, and we cannot conclude without congratulating the author on the care and skill with which he has moulded the successive editions of his book into harmony with the rapid strides of science.

Germinal Matter and the Contact Theory. James Morris, M.D., Lond., Fellow of Univ. Coll., Lond. London: John Churchill and Sons, New Burlington Street. 1867.

The author of this small pamphlet of twenty-three pages in the first enters his plea in the following skilful manner —"Depreciation due only to baseless theories is too often extended to theory in general, but even a theory that does no more than harmonize a large number of facts, is a useful aid to further progress, and is often the means by which we arrive at a law." As a rule the largeness of the number of facts is in direct ratio to the looseness of the theory, as opposed to law. A medical theorem is then worked out *seriatim*, by what the author calls three easy steps; we may call them his axioms.

Axiom I. "Air floats with ease and for a considerable time and distance, light, and small masses of organic matter." Well-known facts are quoted for this, but a certain vagueness of expression somewhat tends to embarrass the reader, *e. g.*, "Seeds of *all sizes* sail in the air, from the thistle or taraxacum, with their parachutes of bristles, down to the smallest floated by its thread of cotton." A rigidly accurate reader might feel disposed to quibble about this statement, as is his wont; seeds of all sizes include horse-chestnut and cocoa-nut seed, and evidence of their being floated by air is still wanting.

Axiom II. Minute portions of organic matter are constantly thrown off by animals and men.

Axiom III. These are received into the body, and some pass into the lungs so as to reach the blood.

These are not to be disputed, and the theorem follows, "Light little masses from the body of one individual are constantly received by other individuals so as to reach their blood."

An interesting number of facts are in the next place cited, to show what diseases are known to be caused by essentially molecular causes, those known to be caused by organic (*i. e.* organized) agencies; thirdly, the grounds for suspecting their presence in other diseases. Dr. Beale's researches are also quoted in terms of the highest praise; "to him we owe the outline of what I conceive to be a generalization, hereafter probably to rank as a landmark in medical science." The pamphlet as a whole we esteem highly, more especially for its clear ordination of facts, and the plainly stated facts for the axioms necessary to work out a theorem. Dr. Morris is an author whom it is very easy to follow in argument, and it is a pleasure to read a pamphlet written on his plan; these good points are so much enhanced by comparison with works that now-a-day abound with theorems without axioms, based neither upon experiment with full details, nor upon high authority with references, judiciously selected.

CORRESPONDENCE.

Magnetism and Gravitation.

To the Editor of the Chemical News.

Sir,—In reply to A. D., I cannot admit that "both the induced poles of a body undergoing magnetic induction may be regarded as at the same distance from the inducing pole thereby rendering the attraction nil." The side nearest the inducing pole being attracted, and the side furthest repelled, the attraction will always predominate over the repulsion. This is true for an inch, or a foot, and why not for any distance.

When a small needle, previously magnetised, is gently laid on the surface of water so as to float thereon, it arranges itself in the magnetic meridian, although the distance of one end of it from a magnetic pole may be only a fraction of an inch more than that of the other in a total distance of several thousand miles. Again, we know that if a magnet be placed at some distance either above or below the pan of a balance, it diminishes or increases the weight of any magnetic substance placed in the pan. Also, if a magnetised needle be introduced into the pan of a balance, below which a magnet is placed, the apparent weight of the needle varies according as we arrange it with its poles similar or opposite to those of the magnet placed beneath it. The apparent weight of a magnetic substance may then be altered at pleasure, or even reduced to nothing. Thus, when a small needle is attached to the ground by thread fixed to one, or preferably, to both ends, and a horse-shoe magnet is held above it, the needle may be suspended in the air at a distance of an inch or more from the magnet, its apparent weight being thus practically reduced to nothing.

Is there anything wonderful in a magnet being able to attract substances at a distance equal to its own length? Is it not, on the contrary, a thing of common occurrence? Why then should we consider it impossible for the magnet we inhabit to attract bodies on its own surface?

In "Fownes' Chemistry" (p. 94, 9th ed.) we find the following passage:—"Of late the march of the daily variations of declination has been carefully compared with the positions of the sun as well as the moon at the corresponding period. This enquiry, suggested by General Sabine, and carried on for a number of years in several localities, has led to the remarkable result that these celestial bodies exert a definite influence upon the magnetic needle, and must therefore be considered as magnets, like the earth itself."

If, then, the moon, as a magnet, is capable of exerting a "definite influence" at a mean distance of 237,000 miles from the earth,—if the sun, as a magnet, can manifest its power at a mean distance of 94½ millions of miles from our globe, we are driven to the conclusion that magnetism is a force which like gravity itself, though of a much more feeble character, is common to all bodies existing in space, and is capable of being exerted at immense distances.

It may be said that if the celestial bodies act as magnets on each other, certain oscillations and other changes in the relative position of the various planets, etc., would be from time to time produced, which could not be explained by the action of gravity alone, in union with the centrifugal force. Such changes, indeed, compared with those produced by gravitation, would be of a minute and trifling character; and the fact, therefore, that they have not hitherto been recognised, is no proof that they do not exist.—I am, &c.

John A. R. Newlands, F.C.S.

The British Seaweed Company.

To the Editor of the Chemical News.

Sir,—There is a slight error in your correspondent's notice of our case in the Paris Exhibition, which please permit me to correct.

"No. 2 from Bardarrig is at present sold for bleaching," this should be *blacking*.

No thorough scientific examination of the acid, basic, and neutral products of the destructive distillation of seaweed has yet been made; these have been under investigation for some time, but the well-known difficulty of completely separating these interesting bodies in a state of purity has long delayed the publication of results.

I may state, however, that the products of distillation, as well as the charcoal from this source, in their chemical composition present little analogy with those of wood, peat, or coal, and can only be chemically classed with those of bone.—I am, etc.,

Edw. C. C. Stanford.

Glasgow, July 30, 1867.

Ozone.

To the Editor of the Chemical News.

Sir,—The following are the more salient points in the development of atmospheric ozone during the past three months:—

In April there was a marked period of ozone from the 4th to the 11th. Considerable amounts were present from the 1st to the 3rd, and on the 14th, 15th, and 21st. No ozone was found on the morning of the 18th, and very little on the 12th, 16th, 17th, aft. of 22nd, 23rd, 25th, and 27th to the 29th.

In May there were marked periods of ozone on the 14th, and 15th, and 25th. Considerable quantities were present on the 4th, 16th, 21st, 27th, and 28th. No ozone was found on the 10th, 20th, and 30th, and very little from the 1st to the 3rd, 6th to the 13th, 17th to the 25th, and 29th to the 31st.

In June there were two marked periods of ozone—the first from the 4th to the 8th, and the second from the 24th to the 28th. Considerable amounts were found on the 2nd, 12th, 20th, and 21st. Very little on the mornings of the 3rd, 10th, 13th, 14th, 16th, 18th, 19th, 22nd, 25th, 29th, and 30th, and throughout the day on the 27th.—I am, etc.,

R. C. C. Lippincott.

Bournemouth.

Gas from Charcoal.

To the Editor of the Chemical News.

Sir,—Your correspondent "G. L.," in No. 372 of your journal, asserts that I have been anticipated by Drs. Blumstrett and Reichardt in my discovery of the fact that the gas is nitrogen which escapes from recently ignited charcoal when immersed in water,—in answer to which I would desire to advise "G. L." that a notice of the receipt of my paper appeared in your Journal for December 7, 1866. Allowing, therefore, for the length of the time required for the transmission of postal information from here to your office, it will be perceived that not only was it impossible I could acquaint myself with the researches of these chemists, but that were I so inclined, I might reasonably contend for priority of discovery.

One question I beg to be allowed to ask of "G. L.," being unable as yet to examine the periodical alluded to,—Have Drs. Blumstrett and Reichardt proved—as I have—that *incandescent* charcoal absorbs nitrogen from the air?

Thanking your correspondent for his information in regard to my paper on "Soluble Vegetable Fibre," and for his courtesy throughout, but taking exception to the application of such terms as innumerable and exhaustive to the description of the researches of *any one*, however distinguished.—I am, etc.,

William Skey.

Wellington, New Zealand, May 17, 1867.

Preservation of Food.

To the Editor of the Chemical News.

Sir,—In reference to our pamphlet upon the preservation of food, will you permit us to state that with the mass of evidence at our disposal we could readily have shown a long list of the so-called "preservative processes" which have proved utter failures, and could have conclusively exhibited the causes of their non-success, but such a course would have necessitated the mention of numbers, dates, and names, together with comparisons which might possibly have been considered invidious, and we preferred, while giving the public much general information relating to what has hitherto been attempted in meat preservation, to keep out of the treatise anything likely to be considered personal, or to give offence.

We beg to draw your attention to the sixth edition of the pamphlet, containing much new matter, and to remark that the practical success of Medlock and Bailey's new patent process, as evidenced by the results obtained by various well-known metropolitan butchers, meat-salesmen, fishmongers, etc., has been quite beyond our expectation for the short period that has elapsed. The Canadian experiments were conducted by Mr. Collett, and those in this country, under the superintendence of Dr. Henry Medlock, and Mr. Wentworth Scott.—We are, etc.,

William Bailey and Son.

Remarks on the Earth's Density.

To the Editor of the Chemical News.

Sir,—My attention has again been directed to the difference existing between the earth's density and the mean specific gravity of the minerals constituting its crust, by the following paragraph, occurring in a recent lecture by Mr. T. Sterry Hunt:—"We may suppose an arrangement of the condensed matters at the centre (of the earth) according to their respective specific gravities, and thus the fact that the density of the earth as a whole is about twice the mean density of the matters which form its solid surface."

On the same subject, W. R. Grove, Esq., in his address to the British Association, 1866, makes the following remarks:—"Surprise has often been expressed that while the mean specific gravity of the globe is from five to six times that of water, the mean specific gravity of the crust is barely half as great. It has long seemed to me that there is no ground for wonder here. The exterior of our planet is to a considerable depth oxidated; the interior is in all probability free from oxygen, and whatever bodies exist there, are in a reduced or deoxidated state; if so, their specific gravity must be higher (?) than that of their oxides, chlorides, etc."

This theory of Mr. Grove, although plausible, is scarcely satisfactory, as it will be seen that many of the substances which go to form the great bulk of the earth's crust, are actually lower in specific gravity as metals than they are when oxidised; while others differ but little in density whether as metals or oxides. The metals whose densities are much lower than their oxides are the metals which form but a small proportion of the earth's crust, and the oxidation or deoxidation of which could make but a trifling difference in our earth's density. Any such difference would be more than counteracted by the opposite tendency of those substances, which constitute the great bulk of the earth's crust, as shown in the following table:—

Metals	Sp. gr.	Oxides.	Sp. gr.
Silicon........	2·49	Silica.........	2·66
Calcium	1·57	Lime	3·08
Magnesium....	1·74	Magnesia	3·40
Aluminium....	2·56	Alumina*	4·00
Sodium	0·97	Soda..........	2·00
Barium	1·50	Baryta	4·00
Potassium.....	0·86	Potassa	2·10
Strontium.....	2·50	Strontia.......	3·90

These examples prove that supposing the earth, beneath the crust to which we have access, to consist of the same metals as above, but in an unoxidised state, the density of the earth would actually be less than the specific gravity indicated by an average of the minerals existing on the surface. Unless, indeed, a far greater proportion of the heavier metals exist in the interior of the earth than on its surface. That the heavy metals do exist in much greater proportion towards the centre of the earth I think is undoubtedly the case, and the only means of solving the difficulty.

During the cooling down of the molten planet, the heaviest metals would naturally tend towards the centre, and hence if we take a table of specific gravities of the metals, we find a singular relationship to exist between the *density* and *scarcity* of a metal.

Thus we find platinum and gold to stand almost at the head of the list in point of density and scarcity, while aluminium, calcium, magnesium, etc., stand conspicuous for their great abundance, and for the lowness of their specific gravities. The metals mercury, zinc, lead, etc., may appear at first sight not to fit in with this supposed law, but when we take into account either their ready volatility or their avidity for sulphur or oxygen, as the case may be, their comparative abundance at the earth's surface may be explained notwithstanding their high densities. For instance, mercury and zinc would be among the very last of the solid substances, volatilized by the earth's heat, to condense; and would in all probability come into contact with large quantities of sulphur vapor, and would naturally take that form in which we find these metals most abundantly.

Another exceptional circumstance may be instanced as operating against the tendency of the metals to follow this law of specific gravities, in the case of iron and tin. Iron has a sp. gr. of 7·8 while tin is only 7·2. The apparent sequence to this, according to the foregoing argument, would be that tin should be more plentiful than iron. We must here again, however, take into account the contingent circumstance that iron is much more readily oxidized than tin, and when once brought into that state has the low sp. gr. of 4 to 5. This will account for the fact that iron, notwithstanding its higher sp. gr. than tin, is much more abundant at the earth's surface than tin. These I give only as examples of the exceptional circumstances that require to be considered in framing the general proposition that *the scarcity of a metal is in the ratio of its density.*

These considerations seem to me to point distinctly to the presence of the heavier metals in much greater abundance in the interior of the earth than at its surface, and especially when taken in conjunction with the fact that the earth has a density twice as great as the minerals of which its crust is composed, and that notwithstanding the probability that it contains many great cavities filled with water or gases.—I am, etc.,

John Sutherland.

Glasgow.

Gas Absorbed by Charcoal.

To the Editor of the Chemical News.

Sir,—Your correspondent from New Zealand, Mr. William Skey, asks me, in No. 400 of the Chemical News (*Amer. Reprint, Oct.* 1867, p. 195) whether Drs. Blumtritt and Reichardt have proved—as he has—that *incandescent* charcoal absorbs nitrogen from the air?

They certainly have done so; their researches extend to a great many varieties of charcoal, both old and freshly ignited, and contain many full analyses of gas. The date of the first notice about them in the *Chemische Centralblatt* is September 12, 1866, and the paper is a reprint from the *Zeitschrift für Deutsche Landwirthe.* The original publication must, therefore, be somewhat older, especially as the *Centralblatt* is generally several months behind; but at all events their priority of publication to Mr. Skey's communication cannot be doubted. However, it would be exceedingly unfair to say that this gentleman, living at such a distance from Europe, *could* in all probability have come across a German periodical, which would hardly have time to reach him in the interval between the publication; I expressly remarked in my letter in No. 372, that, "their paper has most likely not yet been noticed in English chemical periodicals." No one can have the slightest doubt that Mr. Skey has independently rediscovered the property of charcoal in question, although his opportunities in New Zealand could hardly allow him to extend his researches so systematically as those of the European *savants.* I am, etc.,

G. L.

August 3, 1867.

Magnetism and Gravitation.

To the Editor of the Chemical News.

Sir,—I should not trouble you with a second letter on the

---

* After being heated strongly.

above subject had not Mr. Newlands in his reply to my former note misquoted one of my sentences, and thereby entirely altered its meaning. The sentence should stand thus: "If the distance between the pole of a magnet, and a magnetic body be very considerable, compared to the size of the latter, both the induced poles may be regarded as at the same distance," etc., etc.

A careful reconsideration of this sentence may perhaps convince Mr. Newlands, that it is in perfect harmony with all examples advanced by him, and may show him, that he is confounding the merely (or at least almost exclusively) directive action of one magnet, or pole, on another magnet at a great distance, with the attraction or repulsion exerted between two opposite poles. Thus, a small magnetized needle, when floated on water, places itself in the direction of the magnetic meridian; but why, if it does so by virtue of the superior attraction of the North pole, does it not also move in the direction of such pole? Does not the very fact of its remaining at rest in the direction of the meridian prove that the two forces of attraction and repulsion are sensibly equal? I would, in conclusion, recommend Mr. Newlands to make the following calculation: What is the difference between the forces of attraction and repulsion exerted on the poles of a magnetic needle 1 inch long by a magnetic pole several thousand miles distant, the needle pointing towards such pole? If, after having performed this calculation, according to the well-known law that the force of attraction or repulsion varies inversely as the square of the distance, he still believes that such difference may be a measurable amount, why—I hope that he will soon furnish the world with a description of the instrument by means of which he thinks to accomplish it.—I am, &c.,

A. D.

Commercial Analyses.

To the Editor of the Chemical News.

Sir,—Some months ago we heard a little on this matter in your columns. Are we always to be annoyed by these discrepancies? I have just incurred two fees to two separate chemists of large experience in analysing artificial manures, one "high" and the other "low." These two gentlemen have operated on portions of the same fairly drawn sample of superphosphate; the difference between them is only seven per cent.! As a seller, of course I shall believe the "high" analyst, and when I am purchasing goods I shall, for as obvious a reason, employ the "low" man.—I am, etc.,

Simon Simple.

Equivalence, Quantivalence, and Chemical Value in Exchange.

To the Editor of the Chemical News.

Sir,—In an able review of an excellent book (Dr. Miller's "Elements of Chemistry") published in your current number (*Amer. Reprint, Oct.,* 1867, *p.*193) I observe an error, evidently inadvertent, yet of a kind so frequently made, and tending so much to confusion of thought, on topics which it is essential to keep clear in the mind, that a few lines of your valuable space may not, I think, be misapplied in its correction.

In the absence of my friend Dr. Hofmann, this duty devolves, I feel, upon me; because the error in question consists in the misquotation of a term proposed by us, in lieu of the vague, and, as your critic justly calls it, "barbarous" expression *atomicity*. The substituted appellation is not, as your reviewer writes (doubtless by a mere slip of the pen), "*equivalence*," but "*quantivalence*."

As *both* these expressions are retained by us, each having, in your view, its special scientific value, and only the meaningless word "atomicity" being abandoned, it is absolutely essential to philosophical precision that the two words in question should be scrupulously employed, each only in its peculiar sense, as contra-distinguished from the appointed meaning of the other.

Now, the term "equivalence" is set apart to denote the *molecule-forming* power of an element, while "quantivalence" is expressly reserved to signify its *atom-fixing* capacity.

Both these are essentially ponderal values—capable of being numerically expressed as combining-weights, in terms of the same standard unit, viz., $H=1$.

The molecule-forming power of any element corresponds with the minimum-weight thereof, relatively to hydrogen as unity, capable of taking part in the formation of a compound molecule.

The atom-fixing capacity of any element is proportional to the minimum-weight thereof capable of engaging, or conversely, of expelling and replacing, one standard atom; $H=1$ being the standard.

The former of these two values constitutes, for each element, its *equivalent*, in the ordinary acceptation of the word, which is synonomous with *atom-weight*, and *combining number*. It will be remembered that, for the volatile elements, these equivalents correspond (*exceptis excipiendis*) to the respective *gas-volume-weights*, or, *vapour-densities*, relatively to hydrogen taken as unity.

The latter of the two values, as I have elsewhere pointed out, might properly be represented, for each element, by attaching to it a second ponderal equivalent, or representative number, most frequently a fraction of the former,—a moiety, a third, or a fourth,—to denote the smallest quantity by weight capable of fixing, or replacing one standard atom.

This mode of notation would give us two parallel sets of minimum-weights, or combining numbers, each element possessing a pair; and the two being distinguishable, in most cases, as the *major* and *minor* equivalent.

I say "in most cases" because, for chlorine and its congeners, the two equivalents would coincide.

These duplicate equivalents, for the typical elements, would be, respectively, in terms of hydrogen as unity:—

	Major.	Minor.
For oxygen	16	8
" nitrogen	14	4·66
" carbon	12	3

while, for chlorine, 35·5 would represent both values.

It is, however, obvious, that the extension of such a duplicate system to 62 or more elementary bodies, would severely tax the memory and the attention (faculties of sadly limited scope, always to be most studiously husbanded), besides also seriously impairing the succinctness of the symbolic notation, so invaluable as our chemical "short-hand."

This short-hand, as commonly practised, represents each element by its initial letter, with which we associate, by a comparatively easy habit of the memory, its molecule-forming minimum-weight, or ordinary combining equivalent.

The symbol, so far prepared, is ready to have engrafted on it the further conception of *quantivalence*; which we can now exactly define.

"Quantivalence" is the atom-fixing power of the respective elements, denoted, for each typical group, by a special coefficient, whereof the index is attached, for each member of the group, to the initial letter with which is associated its name and ordinary equivalent number; the result being an exceedingly concise symbol, embodying, together with a Name, two distinct conceptions of chemical Value, with their respective Quantities.

As all the simple bodies fall naturally, in respect of their quantivalence, into four typical groups, headed respectively by the four elements cited above, the members of each group, how different soever their molecule-forming weights, correspond with each other as *uni*valent, *bi*valent, *tri*valent, or *quadri*valent, in their atom-satisfying relations.

We have only, therefore, to bear in mind this easily-remembered fourfold classification, with its simple quantivalential indices (dashes or Roman numerals at choice), in order to have constantly at hand for use all the information that the long lists of duplicate equivalents, so burdensome to the memory, must otherwise have been employed to supply.

The disburdenment of memory, and the terseness of symbolisation, thus simply attained, acquire additional value from the happy circumstance that the atom-fixing and volume-condensing powers of the elements advance *pari passu*; so that, (*exceptis excipiendis* once again) the coefficients of quantivalence serve to condense into our symbols and so to keep impressed upon our minds, besides the several ranges of facts above mentioned, this further collateral information.

Confining attention, however, to the main conceptions, distinct though allied, of equivalence and quantivalence as above defined, it is by carefully coupling these together, yet as carefully avoiding to confuse them, that we are enabled to contemplate accurately *in exchange* in its two opposite aspects, that which I have ventured to term *Specific chemical value.*

Nothing, I think, can now be clearer than that this value, so frequently misunderstood, presents itself in two aspects, and as of two kinds, accordingly as we contemplate it relatively, on the one hand, to the formation of molecules, or, in the other, to the fixation or displacement of atoms.

Adopting the first stand point, we clearly perceive that 12 parts by weight of carbon are "worth" (in financial parlance) as much as 14 of nitrogen, 16 of oxygen, 35·5 of chlorine, and so forth.

Placing ourselves at the second point of view, we as clearly see that an atom of any element in the quadrivalent group is "exchangeable at par" for four atoms of any element in the univalent group, and for three and two atoms respectively, of any element in the trivalent and bivalent groups.

I should trespass unduly on the hospitality of your valuable columns, were I to prolong these elucidations, otherwise I would fain trace out a little further the nature and consequences of these admirable relations. Among other such illustrations on which I would gladly dwell is one that I was permitted to adduce in a little book which you were pleased to notice with approval on its first appearance, and which your reviewer cites. I allude to the curious and beautiful quantivalential equipoise, or symmetry, which I have observed to prevail among the five members of the nitroxygen series.

These singular relations will be found displayed at pp. 181, *et seq.*, of the work referred to, in a diagram expressly constructed to show the characteristic feature of this remarkable series; with its central body in the self-balanced structural condition, which I have ventured to call *equiquanticity*; while its two wings form exact quantivalential counterparts, equal, though conversely reflecting one another, as each departs, by opposite grades of declension, from that self-centred archetype.

I forbear, however, from a dissertation which would tempt me too far; and, reverting to the purpose with which I set out, I beg, in conclusion, by way of summary—while acknowledging your reviewer's manifest ability—to deprecate the confusion which his *lapsus calami* (nor his alone) tends to introduce between conceptions so fundamentally distinct as those of chemical *equivalence* and *quantivalence*; signifying as they do, two opposite forms or aspects of "chemical value in exchange,"—those, namely, which we trace, respectively, in the *molecule-forming* and *atom-fixing* powers of the elements. I am, Sir, etc.,

F. O. Ward.

London, Aug., 1867.

Magnetism and Gravitation.

To the Editor of the Chemical News.

"A. D." admits that the force of magnetic attraction varies inversely as the square of the distance, and yet arrives at the singular conclusion that "when the distance between the pole of a magnet and a magnetic body is very considerable as compared to the size of the latter, the body will not be attracted." According to "A. D.'s" views, therefore, if a small particle of iron, say a single molecule, were placed at a distance of a quarter of an inch from the pole of a magnet, it would not be attracted, because the distance of a quarter of an inch would be inconceivably great compared with the length of a molecule of iron. Now, if this holds good for one molecule of iron, it holds good for any number of molecules, for the attraction of a mass may be regarded as made up of the attraction of the molecules of which it is composed. So that "A. D.'s" process of reasoning, legitimately carried out, leads to the remarkable result that a piece of iron would not be attracted by the pole of a magnet placed at the distance of a quarter of an inch from it, "which," to quote a well-known author, "is absurd."

It must be borne in mind that all I have contended for is that, theoretically speaking, just as a magnet placed under a balance increases or diminishes the apparent weight of certain substances introduced into the pan of the balance, so the weights of substances, at ordinary temperatures, on the earth's surface are not their absolutely true weights. That is to say, they are not entirely due to the force of gravitation, but a part, it may be an inconceivably small part, of the apparent weight, is due to the magnetic attraction and repulsion of the earth, and of the atmosphere by which it is surrounded.

On this point I would quote "Watts's Dictionary," vol. iii., p. 774, where we read that "a cubic metre of oxygen gas would act on a magnetic needle with the force of 54 centigrammes of iron, and a cubic metre of air with the force of 11 centigrammes of iron. The whole atmosphere is consequently equal in magnetic power to a shell of iron covering the whole earth to the thickness of 0·1 millimètre."

If bodies were weighed at high temperatures, that portion of their weight which was due to the magnetic attraction of the earth, would almost entirely disappear, and hence the weight of a given quantity of iron would be, theoretically, less if weighed at a red heat than if weighed at ordinary temperatures.—I am, etc.,

John A. R. Newlands, F.C.S.

Laboratory, 19, Great St. Helen's, E. C., Aug. 13, 1867.

Co-operative Chemical Club.

To the Editor of the Chemical News.

Sir,—It occurs to me that in these days of co-operation, it might be a good thing to form "chemical and physical clubs" in various parts of the country wherever sufficient numbers of co-operators could be obtained. There are many, I daresay, like myself engaged in commerce, manufactures, or the professions, who love science for its own sake, and from various causes cannot acquire a laboratory worthy of the name, who would gladly contribute a considerable sum yearly for the privilege of having the use of a well-stored laboratory, and intercourse with others having similar tastes and pursuing similar studies. If there were clubs of this sort in all our large towns, I am sure they would tend greatly to advance the cause of science and to spread a *correct* knowledge of it in many quarters where at present it is but little understood.

It would tend to success were some influential gentleman to bring the subject before the British Association next month, and have clubs instituted under its auspices.

If you consider this idea at all practicable, I shall be obliged by your inserting it in your widely-circulating journal.—I am, etc.

Wm. Durham.

Currie, near Edinburgh, August 17, 1867.

[The idea is excellent, and we shall be happy to give it all the support in our power.—Ed. C. N.]

Specific Gravity Problem.

To the Editor of the Chemical News.

Sir,—Your correspondent "C. H. P." may well be astonished at the reduced sp. gr. of the iron as obtained by his calculation (1·10506). He will find, however, that this lowness is due entirely to the mistake which has been allowed to creep into the method adopted for ascertaining it, and not at all to any error in the observations of *Sideros*, for, working the problem out correctly from them, the sp. gr. is obtained nearly double what it should be, if the substance is iron! The system he has adopted is a proper one, and that given in "Fownes" (in the 8th ed., page 8), for finding the sp. gr. of substances lighter than water, and may be applied to this case thus:—

Weight of iron and olivine in air.............. 50 gns.
" " " " water............ 41·8 "

Weight of water equal in bulk to iron and olivine 8·2 "

Weight of olivine in air...................... 20· "
" " " water.................... 13·75 "

Weight of water equal in bulk to olivine....... 6·25 "

Weight of water equal in bulk to olivine and iron.. 8·2 "
Weight of water equal to olivine alone......... 6·25 "

Weight of water equal in bulk to iron.......... 1·95 "

$$\frac{\text{Weight of iron in air}\ldots\ldots\ldots\ldots 30}{\text{Weight of water equal in bulk to iron}\ldots\ 1{\cdot}95} = 15{\cdot}384 =$$

= sp. gr. of the iron according to *Sideros'* proposition, which brings it to the same result as in my calculation of last week. "Lloyd" would have obtained a more accurate result also with his ingenious formula, had he not unfortunately made a mistake in multiplying 3·2 × 6·1 × 30, which is 585·6, and not 575·6. as given in his solution.

The method worked out above is accurate, but it may be interesting to consider the reasoning by which a shorter method for working out all problems of this character may be arrived at.

Supposing a body weighing A grains in air weighs in water.......... *a* grains.
And another body weighing B grains in air weighs in water.......... *b* grains.
Then the two together, " A + B. " " will weigh in water.......... *a*+*b* grains.

This is self evident, it only requires stating to be perceived, for the body which weighs A in air will displace the same bulk of water when attached to or mixed with that weighing B, that it did when by itself, and the same holds tru eof that weighing B; consequently their united action on the balance must be the sum of the weights obtained when immersed separately. This established, it will be easy to see that if a body weighing A + B in air, weighs in water *a*+*b*, and one of its components weighs A in air, and in water weighs *a*, then the second component weighing B in air, must in water weigh *b*. This last term, which is all that is wanted for obtaining the sp. gr. of the second component, being obtained by subtracting *a* from *a*+*b* in the same way that its weight in air was obtained by deducting A from A+B.

Bearing in mind the nature of the difference between those substances which are heavier and those which are lighter than water, this system becomes applicable to the solution of the interesting problem of finding the sp. gr. of substances of the latter class; all that is required being great attention in the use of the signs during the calculation.

As an example, let us take that in "Fownes," already alluded to. The problem is to find the sp. gr. of a piece of wax, this is attached to a piece of brass in order to make it sink in water.

Brass and wax together in air weigh	183·7	grns., in water		38·8
Brass alone in air weighs.........	50	"	"	44·4
Subtracting we obtain—	——			——
Wax alone in air................	133·7	"	"	−5·6

But to find the sp. gr. of a substance, we must deduct the weight in water from that obtained in air, and this difference becomes the denominator of a fraction representing the sp. gr. of the substance, and of which the enumerator is its weight in air, *i.e.*—

In this case the sp. gr. of wax =

$$\frac{133{\cdot}7}{133{\cdot}7-(5{\cdot}6)}=\frac{133{\cdot}7}{133{\cdot}7+5{\cdot}6}=\frac{133{\cdot}7}{139{\cdot}3}=0{\cdot}9598.$$

F. J. R. C.

To the Editor of the CHEMICAL NEWS.

SIR,—I send you herewith particulars of two experiments, which for my own satisfaction I have made, and in case you may think them worthy of record they are at your service.

The olivine employed was in pure translucent yellow fragments from Vesuvius—the iron being common iron wire. The sp. gr. of both were determined with care.

A. Weight of some fragments of olivine, sp. gr. 3·19, was.......................... 20·33 grs.
Do. of iron wire, sp. gr. 7·51........... 30·21 "

Consequent weight of iron + olivine was.. 50·54 "

And the sp. gr of same taken together was found to be 4·87.

Supposing the problem put in the form proposed by *Sideros*, the formula of "F. J. R. S." would give—

Weight of iron+olivine in water=
=50·54—(50·54+4·87)=...................... 40·17
Deduct waight of olivine alone in water=
=20·33—(20·33÷3·19)=...................... 13·96

Gives weight of the 30·21 grs. iron in water=...... 26·21

and 30·21+(30·21—26·21)=7·55 sp. gr. of iron required. Acing to "Lloyd's" formula.—

$$\frac{3{\cdot}19\times 4{\cdot}87\times 30{\cdot}21}{(3{\cdot}19\times 50{\cdot}54-(4{\cdot}87\times 20{\cdot}33)}=\frac{469{\cdot}321413}{62{\cdot}2155}=$$

=7·54 sp. gr. of iron required.

B. Another experiment gave following results:—

Weight of olivine of sp. gr. 3·20 employed=....... 20·32
" of iron " 7·87 " =....... 33·55

Total weight of iron+olivine being................ 53·87

The specific gravity of which conjointly was 5·09. As before "F. J. R. S's." formula gives—

Weight of iron+olivine in water=
53·87—(53·87+5·09)= .... 43·29
Deduct weight of olivine alone in water=
=20·32—(20·32+3·20)=...................... 13·97

And 33·55+(33·55—29·32)=7·93 specific gravity of iron. Or by "Lloyd's" formula—

$$\frac{3{\cdot}20\times 6{\cdot}09\times 33{\cdot}55}{(3{\cdot}2\times 53{\cdot}87)-(5{\cdot}09\times 20{\cdot}32)}=\frac{546{\cdot}46240}{69{\cdot}9552}=$$

=7·81 sp. gr. of iron.

The difference being probably in part at least due to errors of observation.

D. F.

CHEMICAL NOTICES FROM FOREIGN SOURCES.

Ammoniacal Cobalt-bases, Modes of Formation of.—C. D. Braun. 1. To a solution of cobaltic chloride or nitrate is added solid ammonic chloride and aqueous ammonia, the mixture is well shaken, plumbic peroxide added, and the whole boiled for half-an-hour. The solution then contains chiefly hexammonio-cobaltic chloride, which is precipitated in crystals on addition of chlorhydric acid.

2. A moderately strong solution of cobaltic nitrate is

well shaken with strong ammonia, pure indigo-blue (prepared according to Fritzsche's method) added, and the mixture boiled; on cooling, crystals of pentammonio-cobaltic chloride are obtained.

3. Tetrammonio-cobaltic oxychloride, hexammonio-cobaltic chloride, and aqueous ammonia, heated together under pressure, form pentammonio-chloride, according to the following equation:—

$$(A=NH_3)$$

$$3\left\{\begin{matrix}Co_2A_4\\Cl_3\end{matrix}\right\}+\left.\begin{matrix}Co_2A_4\\Cl\end{matrix}\right\}\Theta\Bigg\}\times{}_4\left.\begin{matrix}Co_2A_6\\Cl_3\end{matrix}\right\}=$$

$$=8\left.\begin{matrix}Co_2A_6\\Cl_3\end{matrix}\right\}\times\left.\begin{matrix}Co_2A_5\\Co_2A_5\end{matrix}\right\}\Theta_3$$

4. The reaction, by which from tetrammonio-oxychloride plus chlorhydric acid, is obtained hexa- and pentammonio salt (Schiff, Fremy) the author explains by the following two equations, the first representing the action of strong chlorhydric acid, the second that of diluted:—

$$\text{I.}\quad 4\left\{\left\{\begin{matrix}Co_2A_4\\Cl_3\end{matrix}\right\}+\left.\begin{matrix}Co_2A_4\\Cl\end{matrix}\right\}\Theta\right\}+2HCl=$$

$$={}_4\left.\begin{matrix}Co_2A_5\\Cl_3\end{matrix}\right\}+{}_2\left.\begin{matrix}Co_2A_6\\Cl_3\end{matrix}\right\}+Co_4\Theta_3+H_2\Theta$$

$$\text{II.}\quad 10\left\{\left\{\begin{matrix}Co_2A_4\\Cl_3\end{matrix}\right\}+\left.\begin{matrix}Co_2A_4\\Cl\end{matrix}\right\}\Theta\right\}+2HCl=$$

$$=10\left.\begin{matrix}Co_2A_6\\Cl_3\end{matrix}\right\}+3\begin{matrix}Co_2A_5\\Cl_3\end{matrix}+3Co_4\Theta_3\times H_2\Theta$$

5. Penta- and hexammonio compounds may also be obtained by acting upon xantocobaltic salts with strong ammonia, or by dissolving in them ammonic chloride and heating.—(*Ann. Chem. Pharm.*, cxlii. 50.)

Toluolsulphurous Acid.—R. Otto and O. V. Gruber. Toluolsulphurous acid ($\text{Ꞓ}_7H_8S\Theta_2$) according to these, is prepared by acting upon sulphotoluolic chloride in ethylic or benzolic solution with sodium-amalgam, and decomposing the sodic salt, thus formed, with chlorhydric acid. It crystallizes from water in large, white, rhombic plates, resembling benzoic acid; dissolves readily in alcohol, ether, or benzol; fuses at 85°, and decomposes above 100°. Exposure to moist air converts it into toluolsulphuric acid.

$$(\text{Ꞓ}_7H_8S\Theta_3)$$

The salts of toluolsulphurous acid are crystallisable and mostly soluble in hot water or alcohol; its ethylic ether is an oily liquid.

Bromine substitutes an atom of hydrogen, forming bromsulphotoluol

$$\left.\begin{matrix}\text{Ꞓ}_7H_7\\S\Theta_2\\Br\end{matrix}\right\}$$

Chlorine acts in a like manner, furnishing a chloride identical with that obtained in the ordinary manner (from phosphoric chloride and sodic sulphotoluolate), and giving with nascent hydrogen Märker's metabenzylsulphhydrate.—*Ann. Chem. Pharm.*, cxlii. 92.)

Argentic Hydrates.—C. Weltzein. Silver dissolves in a neutral solution of hydric peroxide with disengagement of oxygen and formation of a small quantity of a blueish gray precipitate. The solution contains argentous hydrate ($Ag_2=216$), and is not precipitated by sulphuretted hydrogen nor immediately by chlorhydric acid; the precipitate formed after some time consists of a mixture of chloride and metallic silver; potassic hydrate produces a dark brown precipitate. When left exposed to air, the solution assumes a reddish brown colour, and becomes turbid from silver being separated. When evaporated to dryness and extracted with water, a residue of metallic silver is left, and a solution obtained which now contains argentic hydrate ($Ag=108$). This solution has a slightly alkaline reaction, and is at once precipitated with chlorhydric acid. The nature of these reactions is shown in the following equations:—

$$\text{I.}\quad 2Ag_2+H_2\Theta_2=2HAg_2\Theta$$

$$\text{II.}\quad 2HAg_2\Theta=2HAg\Theta+Ag_2$$

$$\text{III.}\quad 2HAg_2+2HCl=2H_2\Theta+2AgCl+Ag_2.$$

(*Am. Chem. Pharm.*, cxlii. 105.)

Oxidation of Alcohol.—C. F. Schönbein. The presence of certain hydrocarbons in alcohol greatly accelerates the oxidation of the latter. When absolute alcohol is mixed with pure oil of turpentine, or a similar substance, and exposed to sunlight and atmospheric oxygen, hydric peroxide is soon formed. The nature of this process the author believes to be this:—The hydrocarbon polarises the oxygen of the air, that is to say, causes it to split up into $\overset{-}{O}$ and $\overset{+}{O}$. $\overset{-}{O}$ is used for the formation of resin, formic acid, etc., and $\overset{+}{O}$ combines on the one hand with the hydrocarbon, forming a compound similar in constitution to hydric peroxide: on the other oxidizes the alcohol, forming hydric peroxide.—(*Jour. Pr. Chem.*, c. 469).

Oil of Bitter Almonds, combination with Acetic Anhydride.—Hübner. The researches of Limpricht and of Neuhof have proved the identify of diacetic benzol, derived from benzoylic chloride and argentic acetate, with the compound obtained from chlorinated toluol; but it remained to be decided whether this compound is also identical or only isomeric with the body, first obtained by Geuther from oil of bitter almonds and acetic anhydride, the latter having been obtained as an oil, differing in that respect from the true diacetic benzol which crystallises readily. Hübner has repeated Geuther's experiments, and confirmed the latter chemist's statements, but adds, that the smallest fragment of a crystal of diacetic benzol brought in contact with the oil, causes it to crystallise immediately, and after several recrystallisations from ether it shows the melting point (44°—45°) and all other properties of diacetic benzol $\text{Ꞓ}_7H_6(\text{Ꞓ}_2H_3\Theta_2)_2$.—(*Zeitschr. Ch.* N.F. iii. 277.)

Pyrrol, Preparation and Oxidation of. — M. Goldschmidt prepares pyrrol by heating ammonic mucate with glycerine in a retort to 180°–200°. At that temperature the decomposition of the mucate into ammonic carbonate and pyrrol takes place with great regularity. This method is likewise preferable to that of dry distillation, on account of the larger quantity and superior quality of the product obtained. Pyrrol reduces argentic oxide, being converted thereby into a well defined acid which is soluble in water, alcohol, or ether, forms precipitates with silver and lead, and sublimes in needles.—(*Zeitschr. Ch.* N.F. iii. 280.)

Gallic acid, Bromo-derivatives of.—M. E. Grimaux. Bromine substitutes readily one or two atoms of hydrogen in gallic acid, forming mono- and di-bromgallic acid$=\text{Ꞓ}_7H_5Br\Theta_5$ and $\text{Ꞓ}_7H_4Br_2\Theta_5$. Both are readily soluble in boiling, sparingly in cold water, soluble in alcohol and in ether.—(*Comptes R.* lxiv. 976.)

Tantalum, Atomic Weight and Compounds of.—R. Hermann believes the composition of tantalic chloride to be $TaCl_2$, for with this assumption the vapor-density, as found by Deville, 9·6 agrees with the calculation, which requires 9·66. The atomic weight of tantalum based upon this formula is 860. To these considerations is added a compilation of all known compounds of tantalum, including several new ones, in regard to which the reader is referred to the original paper. —(*Journ. Pract. Chem.* c. 385.)

Toluol, Chloro-derivatives of.—O. Pieper. Toluol, saturated with chlorine, separates on standing crystals of the composition $\text{Ꞓ}_7H_8Cl_8$. They fuse at 150°, are insoluble in water, sparingly soluble in alcohol, more so in ether, but they dissolve readily in carbonic bisulphide. Sodic hydrate in alcoholic solution decomposes the new compound readily at 110°, and on adding water to the product of decomposition a brown oil separates, the aqueous solution containing small quantities of an acid which seems to be dichlordracylic acid, $Ꞓ_7H_4Cl_2\Theta_2$. The oil after purification is colourless, distils at 280°—290°, and has the composition $Ꞓ_7H_4Cl_4$.—(*Ann. Chem. Pharm.* cxlii. 304.)

Specular Iron made at Biber (Hepia), and used, with great success, for the production of cast steel cannons, being dissolved by electrolysis in hydrochloric acid, was found to contain (according to Bagh) the following substances:—

Carbon	3·758
Iron	87·997
Manganese	6·555
Phosphorus	0·578
Silicon	0·497
Sulphur	0·171
Calcium	0·127
Copper	0·120
Arsenic	0·118
Magnesium	0·052
Antimony	0·027
Silver, lead, bismuth,	trace.

—(*Ann. d. Chem. u. Pharm*, bd. 140, p. 180.)

Blast Furnace.—To increase the production of iron blast furnaces six-fold, Morgan gives them greater dimensions; for instance, 9½ métres in diameter, blowing into the furnace by 12 tuyeres. A hollow cone is besides constructed in the middle part of the bottom of the furnace, by means of which a blast is also introduced into the furnace.—*Revus Univers. X.* ann., 4 livr., p. 62.

Influence of a Current of Gas on the Decomposition of Compounds.—D. Gernez. When a current of an indifferent gas, as nitrogen, hydrogen, or air, passes through a solution of baric, calcic, or potassic bicarbonate, carbonic acid is given off; alcalic sulphhydrates under these conditions lose sulphuretted hydrogen, and acid sulphites and acetates are converted into neutral salts. These phenomena of decomposition are considered due to dissociation.—(*Comptes R.* lxiv. 606.)

Thallium-amalgam.—J. Regnauld. Thallium combines with mercury with evolution of heat, and the amalgam is electro-negative compared with thallium; this is a further proof of the fact that, whenever a metal dissolves in mercury and heat is given off, the metal is electro-positive in comparison with the amalgam.—(*Comptes R.* lxiv. 611.)

Propyle-benzol, Action of Bromine on.—E. Meusel. The action of bromine on cumol at ordinary temperature gives rise to the formation of two substitution-compounds, monobromcumol $Ꞓ_9H_{11}Br$, boiling between 218° and 220°, and a crystalline compound of the composition $Ꞓ_9H_7Br_5$, melting at 99°—100°. At high temperatures and in presence of water an acid has been obtained which has the composition of dibrombenzoic acid.—(*Zeitschr. Chem.* N. F. iii. 322.)

Phosphorous Acid, Action of Bromine and Iodine on.—G. Gustavson. Equal molecules of bromine and phosphorous acid act upon each other in sealed tubes at 100° and below, according to the following equation:

$$PH_3\Theta_3+Br_2=2HBr+\underbrace{BH\Theta_3}_{\text{Metaphosphoric acid.}}$$

Four molecules of acid and three of bromine decompose in the following manner—

$$4PH_3\Theta_3+3Br_2=3PH_3\Theta_4+3HBr+PBr_3$$

Iodine combines less readily with phosphorous acid; the reaction between eight molecules of the latter and five of iodine takes place as follows:—

$$8PH_3\Theta_3+5I=6PH_3\Theta 4+2HI+PH_4I+PI_2$$

—(*Akad. Petersb.* xi. 299.)

Thallium.—Wöhler separates this metal from flue-dust in the following manner:—The material is repeatedly extracted with boiling water, slightly acidulated with sulphuric acid, and the filtrate, without previous concentration, precipitated with chlorhydric acid. The chloride is converted into sulphate, and the latter in aqueous solution reduced by zinc or by an electric current derived from a single cell: the metal is finally fused under a layer of potassic cyanide.*—(*Ann. Chem. Pharm.* cxlii. 263.)

Phosphorus, Combinations of.—H. Wichelhaus. The compound $PCl_2(\Theta Br)$ has been obtained (*Menschutkin Ann. Ch. Ph.* 139, 343), by acting upon absolute alcohol with phosphoric terchloride, and substituting in the compound $PCl_2(\Theta Ꞓ_2H_5)$ thus formed, bromine for ethyle. The author has by means of this reaction, but employing chlorine instead of bromine, prepared the compound $PCl_2(\Theta Cl)$, which, he finds, is identical with the ordinary phosphoric oxychloride. This synthesis has some not unimportant consequences. It will be possible, starting from oxychloride or ethylephosphorous chloride, to prepare the compound $P\Theta(Ꞓ_2H_5)_3$ which, supposing it to be identical with the oxide of triethylephosphine, will thus prove the constitution of the latter to be analogous to phosphoric oxychloride—

$$\overset{'''}{P}\left\{\begin{array}{l}\Theta Cl\\ Cl\\ Cl\end{array}\right. \text{ and } \overset{'''}{P}\left\{\begin{array}{l}\Theta Ꞓ_2H_5\\ Ꞓ_2H_5\\ Ꞓ_2H_5\end{array}\right.$$

Further, the constitution of phosphoric acid will be

$$P\left\{\begin{array}{l}\Theta\Theta H\\ \Theta H\\ \Theta H\end{array}\right.$$

showing it to be the mono-oxy-acid of phosphorous acid, and the existence of di- and tri-oxy-acids consequently may be predicted. The other believes to have already obtained the compound

$$\overset{'''}{P}\left\{\begin{array}{l}\Theta\cdot\Theta H\\ \Theta\cdot\Theta H\\ \Theta\cdot\Theta H\end{array}\right.$$

(*Zeitschr. Chem.* N. F. iii. 321.)

Neurin.—A. Baeyer. The constitution of neurin is that of the hydrate of trimethyle-oxethyle-ammonium

$$=N(ꞒH_3)_3\left\{\begin{array}{r}(Ꞓ_2H_4(H\Theta)\\ H\end{array}\right\}\Big\}\Theta$$

The auro-chloride of this base

$$=NꞒ_5H_{14}\Theta Cl,AuCl_3$$

is soluble in hot water, and crystallises in beautiful yellow needles. The hydriodate of neurin has the formula

$$N(ꞒH_3)_3(Ꞓ_2H_4(\Theta H))I.$$

but if neurin is heated with an excess of hydriodic acid, water is eliminated, and the iodide $N(ꞒH_3)_3(Ꞓ_2H_4I)$ I formed, which on being treated with argentic oxide yields a base, the auro-chloride of which has the composition $NꞒ_5H_{12}Cl,AuCl_3$, showing thus a conversion of neurin into a vinyle-compound. The platino-chloride contains one molecule of water. In order to remove any doubt as to the existence of oxethyle $=Ꞓ_2H_4(\Theta H)$ in neurin, the action of acetylic chloride upon the latter had been tried, and a body obtained of the composition

$$\left.\begin{array}{r}N(ꞒH_3)_3(Ꞓ_2H_4)(\Theta,Ꞓ_2H_3\Theta)\\ H\end{array}\right\}\Theta$$

which is neurin wherein an atom of hydrogen is replaced by acetyle.—(*Ann. Chem. Pharm.* cxlii. 322.)

Oenanthylidene and Caprylidene.—E. Rubieu. Oenanthylidene, $Ꞓ_7H_{12}$, is prepared by heating oenanthylenic chloride with twice its volume of a strong alcoholic solution of potassic hydrate, first under ordinary pressure, and afterwards in sealed tubes to 150°, repeating the treatment again and again, until the greater part of the oil, which which separates on addition of water, distils below 120°. From this portion the pure oenanthylidene is obtained after

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* This is identical with the process by which I prepared considerable quantities of thallium four years ago. For full details see Chemical News, vol. viii., p. 159.—W. C.

repeated fractional distillations, boiling at 106°—108°. It is a colorless, thin liquid, lighter than water, rapidly volatilising at ordinary temperature, burning with a luminous flame, and dissolving in ether, alcohol and benzol. Bromine gives rise to two substitution-compounds—$C_7H_{12}Br_2$ and $C_7H_{10}Br_4$, the latter only having been obtained pure. Caprylidene, C_8H_{14} is obtained from caprylenic bromide by the same process, the reaction taking place somewhat more readily. Bromine produces the compound $C_8H_{14}Br_4$, which on being treated with alcoholic potassic hydrate is converted into $C_8H_{11}Br$.—(*Ann. Chem. Pharm.* cxlii. 294.)

Trichlordracylic acid.—P. Janasch. A boiling mixture of potassic bichromate and diluted sulphuric acid (1 acid to 1 water) converts the solid trichlortoluol (fusing at 75°—76°) into trichlordracylic acid, $C_7H_3Cl_3\Theta_2$. The acid melts at 160°, is readily soluble in alcohol and ether, sparingly so in hot, almost insoluble in cold water. The baric salt crystallises in long needles, and has the composition $C_7H_4Cl_3\Theta_2)_2Ba''$.—(*Ann. Chem. Pharm.* cxlii. 301.)

Trixylylamine.—P. Janasch. This body is formed when chlorxylol (boiling at 200°) is heated with alcoholic ammonia in sealed tubes to 100°. For its separation, the alcohol has to be boiled off, and ammonic chloride removed by washing with water; the remaining oil is then treated with chlorhydric acid, and extracted with ether, the latter dissolving an oily body of, as yet, unknown nature. Trixylylaminic chlorhydrate is insoluble in ether or water, but soluble in alcohol, fuses at 203°—204°, and has the formula $(C_8H_9)_3N,HCl$. Sodic hydrate precipitates the free base, trixylylamine, as a thick oil, heavier than water, not solidifying at −15°.—(*Ann. Chem. Pharm.* cxlii. 303.)

Methylsalicylic acid, formation of.—C. Graebe. The similarity in the behaviour of the hydroxyl-group in phenol and in gaultheria-oil led to the belief that analogous to the synthesis of salicyclic acid (Kolbe and Lautemann) oxyphtalic acid might be obtained by acting upon gaultheria-oil, with sodium and carbonic anhydride, the reaction might be expected to proceed according to the equation:

$$C_6H_4\left\{\begin{matrix}\Theta H\\ C\Theta_2CH_3\end{matrix}\right. + C\Theta_2 = C_6H_3\left\{\begin{matrix}\Theta H\\ C\Theta_2H\\ C\Theta_2CH_3\end{matrix}\right.$$

No acid of the composition of oxyphtalic acid, however, was formed, but salicylic and methylsalicylic acid instead. As carbonic anhydride does not take any part in the reaction, the effect of sodium alone on methylic salicylate had been tried, and it was found that at ordinary temperature the methylic ether of sodiosalicylic acid

$$=C_6H_4\left\{\begin{matrix}\Theta Na\\ C\Theta_2CH_3,\end{matrix}\right.$$

and at 200°—220° sodic methylsalicylate

$$=C_6H_4\left\{\begin{matrix}\Theta CH_3\\ C\Theta_2Na\end{matrix}\right.$$

besides a small quantity of methylic methylsalicylate and sodic salicylate is formed.—(*Ann. Chem. Pharm.* cxliii. 327.)

Hydrophtalic Acid.—C. Graebe and O. Born. Benzol, although a saturated compound, is capable of uniting with two, four, or six monovalent elements, or groups of elements, thus forming the so-called products of addition of the aromatic series which possess the common property of being more or less easily reconverted into compounds of the benzol type. None of these bodies, however, with the exception of quinic acid (*Ann. Chem. Pharm.* cxxviii., 197), have been sufficiently studied to afford a clear insight into their constitution. The present contribution contains the results of the author's researches on the addition of hydrogen to phtalic acid.

Hydrophtalic acid is prepared by adding sodium-amalgam to a solution of 1 part of phtalic acid, 1 of crystallised sodic carbonate, and 8 of water; the acid is then precipitated with chlorhydric acid, recrystallised and decolorised by means of animal charcoal. It is sparingly soluble in cold water and ether, more readily in alcohol and hot water; it may be heated to 200 without decomposition; being a bibasic acid, it forms neutral and acid salts, several of which are described. The decomposition which hydrophtalic acid undergoes with various reagents are shown in the following equations:—With soda-lime:

$$C_6H_6(C\Theta_2H)_2 = \underset{\text{Benzol.}}{C_6H_6} + H_2 + 2C\Theta_2$$

with phosphoric pentachloride:

$$C_6H_6(C\Theta_2H)_2 + 2PCl_5 =$$
$$= \underset{\text{Benzoylic chloride.}}{C_6H_5(C\Theta Cl)} + C\Theta + 3HCl + 2P\Theta Cl_3$$

with sulphuric acid:

$$C_6H_6(C\Theta_2H)_2 + H_2S\Theta_4 = \underset{\text{Phtalic acid.}}{C_6H_4(C\Theta_2H)_2} + 2H_2\Theta + S\Theta_2$$

$$\text{and } C_6H_6(C\Theta_2H)_2 = \underset{\text{Benzoic acid.}}{C_6H_5(C\Theta_2H)} + C\Theta + H_2\Theta$$

with bromine:

$$C_6H_6(C\Theta_2H)_2 + Br_2 = C_6H_5(C\Theta_2H) + 2BrH + C\Theta_2$$

with fusing potassic hydrate:

$$C_6H_6(C\Theta_2H)_2 = C_6H_5(C\Theta_2H) + C\Theta_2 + 2H$$

with nitric acid, or potassic bichromate and sulphuric acids benzoic acid is principally formed. Heated by itself to above 200° hydrophtalic anhydride is obtained. Chlorhydric acid acting upon an alcoholic solution of hydrophtalic acid does not produce the ether of the latter, but benzoic ethide.—(*Ann. Chem. Pharm.* cxlii. 330).

Thallic Acid.—According to E. Carstanjen, thallic acid is formed when a current of chlorine passes through a hot solution of potassic hydrate containing thallic oxide in suspension, the liquid at the same time assuming a crimson colour. The solution may be evaporated and even filtered through paper, without undergoing decomposition; acids decompose the new compound, setting free oxygen. Further details are promised. —(*Journ. Prakt. Chem.* ci. 55).

Molybdates.—F. Ulik finds that there are six series of molybdates, corresponding to the general formulæ:—

1. MO,MoO_3+nHO
2. $MO,2MoO_3$
3. $3MO,7MoO_3+nHO$
4. $MO,3MoO_3+nHO$
5. $MO,4MoO_3+nHO$
6. $MO,8MoO_3+nHO$

Those hitherto known belong to series 1, 3, and 4. The salts of the 4th and 5th series appear in two modifications, a crystalline and an amorphous.

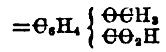
It is further shown that a decided analogy exists between molybdic, chromic, and sulphuric acid, the first forming a magnesic salt analogous in composition to the corresponding sulphate; and in the double salt $KO,MgO,2MoO_3$, one half of the molybdic acid may be replaced by chromic acid.—(*Journ. Prakt. Chem.* ci. 61).

Methoxybenzoic Acid.—C. Graebe and O. Schultzen. It has been shown (*Ann. Chem. Pharm.* cxxxix. 134) that the body obtained by acting upon gautheria-oil with potassic hydrate is potasso-salicylic methide, and that the latter may be converted into methylesalicylic acid; it has likewise been proved that paraoxybenzoic acid, by the same reaction, gives rise to the formation of sodioparoxybenzoic ethide and anisic acid. The object of the present communication is to show that oxybenzoic acid, as regards the derivatives mentioned, behaves like its two isomers.

Oxybenzoic acid, prepared with unimportant modifications according to Fischer's method (*Ann. Chem. Pharm.* cxxxvii. 137), is heated together with potassic hydrate and methylic iodide in sealed tubes, to 140°, when the following reaction takes place:—

$$C_6H_4\left\{\begin{matrix}\Theta H\\ C\Theta_2H\end{matrix}\right. + 2CH_3I + 2KH\Theta =$$
$$= C_6H_4\left\{\begin{matrix}\Theta CH_3\\ C\Theta_2CH_3\end{matrix}\right. + 2KI + 2H_2\Theta$$

The methoxybenzoic methide thus formed is treated with potassic hydrate, and thereby converted into methoxybenzoic acid.

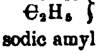


$$=C_6H_4\left\{\begin{matrix}\Theta CH_3\\ C\Theta_2H\end{matrix}\right.$$

The acid is readily soluble in hot water, from which it crystallises in long white needles; it is also soluble in alcohol and ether; it fuses at 95°, and sublimes without decomposition. The argentic salt $=C_8H_7Ag\Theta_2$ is obtained as a white precipitate, soluble in hot water.—(*Ann. Chem. Pharm.* cxlii. 350.)

New Series of Sulpho-compounds.—A. Saytzeff.—Sulphamylic oxide, $(C_5H_{11})_2S\Theta$, fuses at 37—38°. Zincic ethide, ethylic, or amylic iodide are without action upon it; when heated with hydriodic acid in sealed tubes to 100° a brown oil, insoluble in water, is formed. The acid liquor remaining, after sulphamylic oxide has been separated, contains a small quantity of amylsulphurous acid.

Butylic sulphide, $(C_4H_9)_2S$, is formed by heating butylic chloride with an alcoholic solution of potassic sulphide; it separates from the mixture on addition of water as an oily liquid which, after having been dried and rectified, boils between 176 and 185°; it is insoluble in water, soluble in alcohol and ether. Amylethylic sulphide

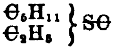


$$\left.\begin{matrix}C_5H_{11}\\ C_2H_5\end{matrix}\right\}S$$

is obtained by treating sodic amyl-mercaptide.

$$C_5H_{11}NaS$$

with ethylic iodide as a transparent liquid, insoluble in water, and boiling at 158—159°. Treated with fuming nitric acid, the oxide



$$\left.\begin{matrix}C_5H_{11}\\ C_2H_5\end{matrix}\right\}S\Theta$$

is formed, besides a trace of an acid, containing sulphur. These observations differ from those of Carius, who gives the boiling point of the sulphide at 132—133·5°, and who has obtained ethylsulphurous acid as the only product resulting from the action of nitric acid.

Sulphethylic oxide $(C_2H_5)_2S\Theta$, is obtained by the action of nitric acid on ethylic sulphide; it is a thick, nearly colourless liquid, which crystallises on cooling. Reducing agents reconvert it again into ethylic sulphide; further oxidation produces diethylsulphan.

Sulphomethylic oxide $(CH_3)_2S\Theta$, is prepared, like the ethyle-compound, by acting upon methylic sulphide with strong nitric acid; the same reaction gives rise to the formation of nitrate of sulphomethylic oxide

$$(CH_3)_2S\Theta.HN\Theta_3$$

which is a crystallisable, deliquescent salt. If a solution of the methylic sulphide in nitric acid is heated for several hours in a sealed tube methylsulphan is formed, which is soluble in water, fuses at 109° and boils at 238°. The action of methylic iodide on amylethylic sulphide was expected to result in the formation of the compound $S(CH_3.C_2H_5.C_5H_{11})I$: instead of this, however, iodide of trimethylsulphin $S(CH_3)_3I$ was formed, besides ethylic and amylic iodide.—(*Zeitschr. Chem.* N. F. iii. 358.)

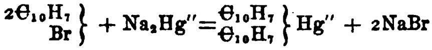
Sodic Hydrate, Crystallised.—E. Schöne. A hot saturated solution of sodic hydrate begins to crystallise when cooled to between 40 and 50°. The crystals have the composition $Na_2\Theta+5H_2\Theta$ or $Na_2H_2\Theta_2+4H_2\Theta$. They are very deliquescent, but after being thoroughly dried upon a porous surface under the desiccator, do not fuse below 80°.—(*Zeitschr. Chem.* N. F. iii. 383.)

Thionessal.—M. Fleischer. The formula of thionessal according to the author is $C_{28}H_{20}S$, instead of $C_{26}H_{18}S$ as stated by Laurent and Märker. Bromine substitutes three atoms of hydrogen forming $C_{28}H_{17}Br_3S$ which may be obtained in crystals from its solution in petroleum (of a high boiling point) fusing at 265°—267°. This compound again treated with bromine is converted into $C_{28}H_{16}Br_4S$. Potassic chlorate and chlorhydric acid oxidise all the sulphur of thionessal into sulphuric acid, converting it thereby into the compound $C_{14}H_{10}\Theta$, which fuses at 214°. Phosphoric chloride forms C_7H_5Cl (or χC_7H_5Cl), fusing at about 130°. Fuming nitric acid first produces the nitro-compound $C_{28}H_{16}(N\Theta_2)4S$ which subsequently is converted into a body, free from sulphur, probably $C_{14}H_{10}(N\Theta_2)_2\Theta_3$, and finally into nitrodracylic acid. Fuming sulphuric acid dissolves thionessal with disengagement of sulphurous acid, and formation of the acid $C_7H_6S\Theta_4$, the baric salt of which has the composition $(C_7H_5S\Theta_4)_2Ba4H_2\Theta$. The compound produced by the action of soda-lime seems to be tolallylic sulphide $C_{14}H_{10}S$.—(*Zeitschr. Chem.* N. F. iii. 376.)

Mercuric Naphtide.—R. Otto. Sodium-amalgam was made to act upon monobromnaphthalene, diluted with several times its volume of benzol (boiling between 120 and 140°) in the hope of getting dynaphtyl

$$\left.\begin{matrix}C_{10}H_7\\ C_{10}H_7\end{matrix}\right\}$$

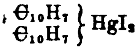
The reaction, however. proceeded differently—mercuric naphtide being formed according to the equation:—



$$2\left.\begin{matrix}C_{10}H_7\\ Br\end{matrix}\right\} + Na_2Hg''=\left.\begin{matrix}C_{10}H_7\\ C_{10}H_7\end{matrix}\right\}Hg'' + 2NaBr$$

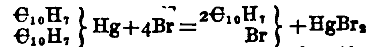
Mercuric naphtide, which may thus be prepared with ease, and in any quantity, crystallises in white needles, soluble in hot benzol, sparingly so in alcohol, insoluble in water; they fuse at 248°.

When heated with lime naphtalene principally is formed, but no dinaphtyl. Iodine combines with mercuric naphtide, forming mercuric di-iodmercaptide,



$$\left.\begin{matrix}C_{10}H_7\\ C_{10}H_7\end{matrix}\right\}HgI_2$$

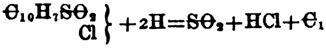
which crystallises from hot alcohol in beautiful silky needles. Bromine added in excess acts in the following manner:—



$$\left.\begin{matrix}C_{10}H_7\\ C_{10}H_7\end{matrix}\right\}Hg+4Br=2\left.\begin{matrix}C_{10}H_7\\ Br\end{matrix}\right\}+HgBr_2$$

Naphtalenic bromide.

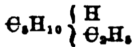
By the action of sodium-amalgam sulphonaphtalenic chloride is resolved into naphtalene and sulphurous acid:—



$$\left.\begin{matrix}C_{10}H_7S\Theta_2\\ Cl\end{matrix}\right\}+2H=S\Theta_2+HCl+C_{10}H_8$$

(*Zeitschr Chem.* N. F. iii. 377.)

Isomer of Ethylamyle, and Observations on Mixed Ether.—Reboul and Truchot. When hexylic chloride is acted upon by alcoholic potassic hydrate, hexylene C_6H_{12} is formed, (Pelouze, Cahours), but besides this ethylhexyl; heptylic, octalyc, and decylic, chloride likewise are converted into both the hydrocarbon and mixed ether; amylic chloride gives chiefly ethyl-amyle, but also some amylene, even ethylic bromide yields besides ethylic ethide, ethylene. This reaction, therefore, is a general one, the members of the series differing in this respect, that the lower ones give rise to the formation of mixed ether principally, while those of higher atomic weight yield hydrocarbon more abundantly. By the action of alcoholic potassic hydrate upon amylenic bromhydrate a body is obtained of the composition of ethyl-amyle, but differing from the latter in boiling-point by about 10°, as also in its behaviour towards hydrobromic acid,—ethyl-amyle forming ethylic and amylic bromide, the isomer ethylic bromide and amylenic bromhydrate. The authors propose for the isomeric compound the formula:—



$$C_5H_{10}\left\{\begin{matrix}H\\ C_2H_5\Theta\end{matrix}\right.$$

(*Comptes R.* lxiv. 1243.)

MISCELLANEOUS.

Intercolonial Exhibition, 1866-7.—The shadow of the Paris Exhibition has, it is true, obscured somewhat the *éclat* of the similar event of the Southern World, but the history of the latter is well worthy of a short record. A few months ago, the Governor of Victoria publicly received the report of the jurors, and formally declared their awards at Melbourne. Up to the middle of February the number of admissions had been 242,892, which speaks well for the interest taken by the inhabitants of a thinly inhabited district in their national undertaking. A few notices of these awards will give a very fair notion of the character of this Exhibition, and will serve as a basis for comparison between it and similar European festivals. The public spirit shown in the Southern hemisphere cannot be mistaken, for it is most probable that this national work will remain as a permanent exhibition, while in England at the present time no decided step has been taken to secure any of the marvels of the Paris Exhibition for the public, the more valuable of which have already been bespoken by foreign governments. The most interesting department of course is that of the ores and non-metallic products, in which medals are allotted for nuggets, ingots, and granulated samples of gold and silver auriferous quartz and colonial gems. Antimony was exhibited in abundance, and native copper of a very superior character. Mr. G. Milner Stephens, F.R.S., received a medal for gems and precious stones; tin-ore, slate, and limestone, were exhibited, but by far the greatest interest is attached to the coal from Newcastle (neither upon Tyne, nor under Lyne), the shale from the Hartley Kerosene Oil and Paraffin Company, and the kerosene from the Western Kerosene Oil Company. Similar shales were sent from New South Wales and Tasmania, also coals from both; from the latter country also the most valuable topazes were sent. South Australia carries off the palm for the following valuable minerals: copper ores, bismuth, plumbago, cobalt and its ores. The ores from Western Australia, for which medals are awarded, were copper only. New Zealand produces, in common with the other colonies, gold and coals, with plumbago and *Titaniferous iron ore*, with novelties in the way of chromium ores and alum. The absence of all mention of platinum would lead us to the conclusion that it is almost the only metal of commercial importance not found plentifully in Australia. The chemical products, when contrasted with the brilliancy of the preceding, are rather meagre, the leading items for which medals are awarded, being "a beautiful sample of higher tarascaci," colonial soft soap, and fluid magnesia. Several awards were made for photographic chemicals, one for commercial mineral acids from Victoria, two for colonial made ink, these with Moulder's coal-dust colonial made ink, and fine quality blackings, exhaust the products for which awards were made in this section of "Chemical and Metallurgical Products and Processes." In the section for horns, hoofs, bones, etc., we notice stearine candles, silicated soaps, anti-corrosive composition for ships' bottoms, superphosphate of lime, deodorising powder, Victorian guanos. The native oils and waxes possess a special interest, with sperm oil, black whale oil, spermaceti, etc. In another department many medals were given for preserved meats, and essence of beef, chiefly from New South Wales. Of general interest will be the awards of medals to Mr. Allport for salmon two years old, smolt and perch, to the Acclimatization Society for the Angola goats, alpacas, and llamas: we mention these especially to show the spirit that exists in the colony. The following are prepared in Australia now also—formerly imported from foreign countries—arrow-root, coffee, and spices in great abundance, starch, maizena, granulated potatoes, "a very valuable article for long voyages;" maccaroni and vermicelli are also commended as a good specimen of a new industry. The Netherlands India Society received medals for tea, coffee, nutmegs, and cloves. New Caledonia is very ably represented, thanks to the exertions of the eminent scientific workers, who have prepared specimens of gluten, starch, and sugar, from the various native plants of that colony. A separate class is formed for chemical and philosophical apparatus. We might prolong this notice to an indefinite length, but will allude only to the sections for native wines and liqueurs, glass manufacture, and photography, with photographs of the Aborigines. In conclusion, we may add that the interest taken by neighbouring States in it, has given to this Melbourne Exhibition quite an international character; every one of our Australian colonies exhibited, and products were also sent from the Mauritius and the French settlement of New Caledonia. The Australian Maraschino, brandy, Hermitage, and Burgundy, may perhaps attract attention even at Paris, where one of the best conducted English departments is imported directly from Melbourne; we allude to that of Messrs. Spiers and Pond, who, as recognized representatives of England at Paris, bear witness to the rapid strides in civilisation made by our Australian colonies.

"Artificial Gold."—The following alloy, "artificial gold," as it is called, has been lately the subject of correspondence in the *Mining Journal*, etc., and is hailed as a grand discovery likely to serve the Cornish copper and tin mines, by introducing a new demand for these metals. The description here quoted is from the *Engineer*, of July 19, 1867, as follows:—"It is stated that an American has discovered a beautiful alloy, which has been most successfully applied as a substitute for gold; it is composed of pure copper, 100 parts; pure tin, 17 parts; magnesia, 6 parts; tartar of commerce, 9 parts; sal ammoniac, 3·6 parts; and quicklime, 1·6 part. The copper is first melted then the lime, magnesia, sal ammoniac, and tartar are added, little at a time, and the whole is briskly stirred for about half-an-hour, so as to mix thoroughly, after which the tin is thrown on the surface in small grains, stirring until entirely fused. The crucible is now covered, and the fusion kept up for about thirty-five minutes, when the dross is skimmed off, and the alloy found ready for use. It is quite malleable and ductile, and may be drawn, stamped, chased, beaten into powder, or into leaves, like gold-leaf. In all of which conditions it is not distinguishable from gold even by good judges, except by its inferior weight. The alloy has already been largely applied in the United States, and requires only to be known in Great Britain to become a favorite."

The Metric System.—Interpretation of the Act of 1864.—During the past year a subject of some importance, involving the legal construction of the Metric Act of 1864, was brought to the notice of the Board of Trade. One of the inspectors of weights and measures for the county of Surrey stated that he had seized some metric weights in a tradesman's shop in Southwark, as being illegal. On his bringing the matter before the magistrates at the Newington Sessions House, the defendants alleged that the Metric Weights and Measures Act, 1864, 27 & 28 Vict. c. 117, permitted the use of metric weights, but gave the inspector no power to examine them. The magistrates dismissed the information, observing that the Act was loosely drawn, and they advised the inspector not to seize metric weights, as the defendants were justified under the Act in using them. Upon this subject the Board of Trade directed a case to be prepared for the opinion of the law officers, who gave their opinion that, notwithstanding the provisions of the Metric Act, a person *using* material metric weights and measures is liable to have them seized, and to conviction and forfeiture of the weights, under the Act 5 & 6 Will. iv. c. 63.

Pharmaceutists and the Jury Lists.—We have been requested to remind members of the Pharmaceutical Society, and others who may be entitled to claim exemption from serving on juries, that the churchwardens and overseers receive, during the month of July, the precept to return lists of persons qualified to serve on juries for the ensuing year; and that in August such lists are prepared and affixed to the church doors, etc. Pharmaceutical chemists should see that their names are not inserted in such lists, and, if inserted, should attend on the day of appeal and present their legal certificate of registration and exemption; such certificate may be obtained from the registrar of the Pharmaceutical Society, 17, Bloomsbury Square, on payment of 1s.

Quekett Microscopical Club.—The second annual general meeting was held in the library of university college on Friday the 26th ult. Mr. Ernest Hart, President, in the chair. The report of the committee showed that the society now numbers 273 members, of whom 130 were elected during the year; that many papers of microscopic interest had been read, field excursions successfully carried out, and class instruction in the uses of the microscope afforded to the younger members. The treasurer's report gave a satisfactory balance and in every way the club was in a very prosperous state. The president delivered an address, in the course of which he congratulated the members on the remarkable success which had attended the operations of the year, and on papers having been read which would bear comparison with those of any other club. He urged the members to remember that the microscope was not only a source of amusement but an instrument of research, and it was its real use which ought rather to be cultivated. Amusement and research were not incompatible, and the contemplation of minute forms was in itself a means of recreation, but the true microscopist is he who looks through form and structure to discover uses and laws, who is never contented with endeavouring to ascertain what are its relations, merely with a view to systematizing, but as a means to an end. Whether we consider the study of the microscope as an intellectual amusement or look into it as teaching some of the very highest truths relating to law, order and power in the universe, we yield only to the convictions which are taught us whilst looking upon structure in relation to the causes which modify it. The following officers were elected for the ensuing year:—President, Mr. Arthur E. Dunham, F.L.S. Vice-Presidents — Dr. Tilbury Fox, M.R.C.P.; Mr. Ernest Hart; Mr. William Hislop, F.R.A.S.; Mr. John K. Lord, F.Z.S.; Treasurer — Mr. Robert Hardwicke, F.L.S.;—Hon. Secretary—Mr. Witham M. Bywater;—Hon. Secretary for foreign correspondence — Mr. M. C. Cooke;—Committee—Mr. W. J. Arnold, Mr. N. Burgess, M. S. J. McIntire, Mr. J. Slade.

Petroleum as Fuel.—According to Adam's trials with petroleum for heating boilers, the following advantages, in comparison to coal, resulted:—Quicker steam generation, smaller dimensions of fire-place and boiler, constant firing, no smoke, ashes, or residua (these amount from 7 to 16 per cent. when using coal); possibility of having an intense fire at once without the assistance of increased draught (this is of great importance to steam vessels); easier working, and considerably smaller store-rooms than coal would require.—*Revue Univers* X. ann., livr., p. 206, with drawing.

Fusibility of Aluminates containing a large amount of lime.—(Fremy.) Mixtures of 80 parts lime and 20 alumina and 90 parts lime and 10 alumina become liquified when heated in crucibles in a wind furnace. Mixtures of 93 lime and 3 alumina still frit at such temperature. The cooled masses have crystalline fracture, alkaline reaction, swell up in water, and may be used in metallurgy on account of their affinity for sulphuric and phosphoric acid.—(*Dingl. P. bd.* 177, p. 376.)

A New Science Scholarship.—On Friday, 26th ult., was celebrated the 30th anniversary of the City of London School, and upon that occasion a report was presented detailing the progress made towards the foundation of a *Testimonial Scholarship* in honour of the Rev. G. W. F. Mortimer, D.D., late head master, whose eminent services in the cause of education, and especially in the successful conduct of the school for a period of more than a quarter of a century, were deemed worthy of public and permanent recognition. For the purpose of giving effect to this resolution a committee, composed of old pupils of the school, and a few leading members of the Corporation, took the matter in hand shortly after Dr. Mortimer's retirement, and invited subscriptions, which already amount to about five hundred pounds. This sum will, it is believed, be further augmented, so that an annual grant of at least twenty pounds may be realised. It has now been proposed by Mr. Ernest Hart, one of the secretaries, to devote the proceeds to the encouragement of the study of science in the school, and, by granting an annual premium, assist in supporting a pupil either at the Royal College of Chemistry, or other scientific educational establishment of Great Britain. This proposal met the hearty approval of the subscribers, and its expediency was strongly urged by Mr. Thomas Hall, B.A., who for twenty years past has conducted the science classes in the City of London School. When this suggestion has been finally decided upon we will inform our readers; in the meanwhile, it should be mentioned that there is already a medical scholarship (tenable for three years at St. Thomas's Hospital), and that Mr. Alderman Hale presents a silver medal for proficiency in chemical science, to be competed for annually by the pupils, all of whom, to the number of six hundred, are now taught chemistry as a branch of general education in the school. We notice the names of Mr. W. H. Perkin and Mr. J. Spiller among those acting on the committee, and in the list of subscribers to the Testimonial Fund are to be seen the names of several other chemists who received their early scientific training in the school. The treasurer, J. Sharp, Esq., LL.D., was also formerly one of Dr. Mortimer's pupils.

Composition and Quality of the Metropolitan Waters in July, 1867.—The following are the Returns of the Metropolitan Association of Medical Officers of Health:—

Names of Water Companies.	Total solid matter per Gallon.	Loss by ignition.*	Oxydisable organic matter.†	Hardness.	
				Before boiling.	After boiling.
Thames Water Cos.	Grains.	Grains.	Grains.	Degs.	Degs.
Grand Junction.	18·67	0·75	0·82	1·25	4·0
West Middlesex.	17·44	0·60	0·71	12·5	3·5
Southwark and Vauxhall.	18·16	0·42	0·71	12·5	4·0
Chelsea.	17·80	0·59	0·71	13·0	4·0
Lambeth.	17·51	0·72	0·80	12·5	3·5
Other Companies.					
Kent	28·49	0·50	0·42	15·5	7·5
New River	17·00	0·49	0·39	12·5	5·0
East London	17·83	0·49	0·46	12·5	5·0

* The loss by ignition represents a variety of volatile matters, as well as organic matters, as ammoniacal salts, moisture, and the volatile constituents of nitrates and nitrites.

† The oxydisable organic matter is determined by a standard solution of permanganate of potash—the available oxygen of which is to the organic matter as 1 is to 8; and the results are controlled by the examination of the colour of the water when seen through a glass tube two feet in length and two inches in diameter.

The amount of ammonia in the water and of that derivable from organic nitrogen did not in any case exceed the 0·0094 of a grain per gallon of water, and there was no organic nitrogen or ammonia in the Kent water. It was, therefore, absolutely free from organic matter of an animal origin.

H. Letheby, M.B.

The State of the Thames.—In a letter to the *Times* dated Aug. 7th, "Y" writes, that last night during low water the characteristic stench of the Thames was distinctly perceived by several independent observers on the terrace at the Houses of Parliament, though in a less degree than in former times. As the water of the river is chemically examined from time to time by persons appointed by the Board of Health, it would be interesting to learn from them whether either the proportion or condition of the organic matter in the water will explain the unwelcome fact.

Economy of Light in Dark Alleys.—If in a very narrow street or lane we look out of a window with the eye in the same plane as the outer face of the wall in which the window is placed, we shall see the whole of the sky by which the apartment can be illuminated. If we now withdraw the eye inwards, we shall gradually lose sight of the sky till it wholly disappears, which may take place when the eye is only *six* or *eight* inches from its first position. In such a case the apartment is illuminated only by the light reflected from the opposite wall, or the sides of the stones which form the window; because, if the glass of the window is *six* or *eight* inches within the wall, as it generally is, not a ray of light can fall upon it. If we now remove our window and substitute another in which all the panes of glass are roughly ground on the outside, and flush with the outer wall, the light from the whole of the visible sky and from the remotest parts of the opposite wall will be introduced into the apartment, reflected from the innumerable faces or facets which the rough grinding of the glass has produced. The whole window will appear as if the sky were beyond it, and from every point of this luminous surface light will radiate into all parts of the room. In order to explain the superior effect of roughly ground glass, let us suppose that the ordinary window is replaced with a single sheet of the best glass inserted flush with the outer wall. The whole of the light from the visible portions of the sky will fall upon its surface, but at such an obliquity that four or five-sixths of it will be reflected outwards, and the other two or one-sixth, which is transmitted, will fall on the floor or on the shutters, and be of no value.—*Sir D. Brewster.*

The Earliest Universal Exposition of which we have any record, was held at Rome in the days of Nero. The philosopher and moralist, Seneca, gives the following account of it: "I was present, the other day, at a solemn exhibition of the wealth of Rome; where I saw statues which were marvels, perfect masterpieces; exquisite stuffs and draperies, and costumes brought from countries even beyond the Roman frontiers," etc.

Nitro-Glycerine in Blasting.—A correspondent of the Nevada *Gazette*, who has recently visited the summit tunnel on the Central Pacific Railroad, writes that the contractor thinks they are going ahead with the tunnel fully twenty-five per cent. faster by the use of nitro-glycerine than they could by using powder. The small holes required for the oil can probably be drilled in less than one-third the time required far larger ones necessary in using powder. The oil does much more execution than powder, as it always breaks the rock from two to sixteen inches beyond the hole, and also throws out a much larger body. The oil in hard rock shows a strength five times greater than powder, pound for pound. It is made upon the spot, and is considered much stronger, as well as safer, than that imported. They have now been using it for several months, and have never yet had a premature explosion, or any other accident, and not a single blast has missed fire since the Chinamen commenced filling the cartridges. The work upon this road seems to have fully set at rest the superiority of nytro-glycerine over powder, both for economy and safety. Of course this applies to the oil made upon the spot, and not to the imported article.

Clarifying Action of Sulphate of Alumina on Turbid Water.—Whatever be the nature and quantity of the earthy substances held in suspension in turbid water, it becomes fit to drink in from seven to fifteen minutes if to each litre there be added ·04 grammes of finely powdered alum—care being taken to agitate the liquid when the alum is introduced (this is about ¾ lb. per ton of water). If potash alum is used the alum is decomposed into sulphate of potash, which is all dissolved by the water and sulphate of alumina, which, by its decomposition, purifies the water. The alumina separates in an insoluble form, and carries down with it as it precipitates the matters which render the water troubled, and the organic matter. The acid attacks the alkaline and earthy carbonates, and transforms them into sulphates. The water becomes slightly richer in bicarbonates and free carbonic acid, whilst all organic matter is destroyed. Seven parts of sulphate of alumina will purify as much water as ten parts of rock alum or potash alum, and the sulphate of alumina does not introduce any alkaline sulphate into the clarified water.—*Technologiste*, vol. xxiv., p. 197.

What is Fame!—The following extract from an *American* scientific paper somewhat surprises us. The only explanation we can offer is, that the paragraph was possibly translated from the *French.* "Offers of knighthood have been made and refused by several of the distinguished mechanics and men of science in Great Britain, ; James Watt refused knighthood, as did Michael Faraday. The latter commenced life as a poor mechanic, and worked up to the head of his profession; is an honorary member of the Institute of France, Fellow of the Royal Society, an able author on engineering subjects, and is the inventor of the cellular hollow girder system upon which the Britannia tubular bridge is built."

How to Produce Stoneless Fruit.—At a late meeting of the Agricultural Society in India, the Rev. Mr. Firminger communicated a plan by which the stones of fruit may be reduced or made to disappear, and the pulp increased in size and flavour. At any time during the cold season select a branch that is to be used afterwards for inarching. Split it up carefully somewhat less than a span long. From both halves of the branch thus split scoop out cleanly all the pith; then bring the split halves together again, and keep them bandaged till they have become thoroughly united. At the usual time, the beginning of the rains inarch the branch thus treated upon suitable stock, taking for the place of union the portion of the branch just below where the split was made. Upon a branch of the tree thus produced a similar operation is performed, and so on for successive seasons, the result being that the stone of the fruit becomes less and less after each successive operation. This process has been applied likewise to the grape vine at Malaga, and plants thereby have been produced which bear the finest fruit, without the slightest vestige of a stone within them.—*Mining Press.*

Explosion of Gun-cotton.—On Monday afternoon an explosion occurred in a building used for the purpose of drying gun-cotton at the works of Messrs. T. Prentice and Co., Stowmarket. The building was a two-storied one, the bottom being built of clay lumps, and the upper part of wood with a lining, the space being filled up with sawdust packing. The roof was of slate. The building was situated near the bank of the river on the railway side, at the further end of the works. The custom has been to place the manufactured cotton in the building moist, and to subject it for a time to heat varying from 90° to 120°. This heat was imparted by means of pipes from an adjacent shed, in which were a series of stacks of horizontal pipes generating heat from steam conveyed to them by a pipe across the river. About 3 p.m. a loud. dull report was heard, followed by another sharper report, and the building attacked was levelled with the ground, while the ruins were in a blaze. The flames spread to another drying shed, in which was a quantity of the sheets of paper used in cartridges, hung up for drying. The roof of this shed was blown up, and the flames thence extended to some large tanks containing cotton in process of manufacture. Some of the covers were burnt, but the cotton was only blackened in places. Some trees on the railway side of the works were burnt and scorched, and some cattle belonging to Mr. Arnold Scuttle were badly burnt. No person was in the shattered buildings when the explosion took place, although two men named Conham and Broom had been in the drying shed a few seconds before the accident happened. The occurrence is attributed to over heating; the sun made the roof very hot, and the pipes forcing in more hot air, the temperature was raised to 170°, at which gun-cotton explodes.—*The Times.*

[If 170° refers to Fahrenheit's scale it is entirely wrong. Professor Abel places the exploding point of gun-cotton at about 150° C., which is equivalent to 302° F.—Ed. C.N.]

Legalised Poisoning.—It is high time that steps were taken to compel the Metropolitan Railway Company to supply their passengers with air which can be inhaled without danger to life. Travelling daily along that line, we have noticed for some months past that the atmosphere, especially between Baker Street and King's Cross, has been getting more and more foul, until at the present time it is almost unbearable. Sulphurous acid is almost habitually present in such quantities as to render breathing painful, and to induce a feeling of suffocation. That these statements are not exaggerated is proved by the shocking death which actually took place at one of these stations one day last week. The inquest was held by Dr. Lankester. The deceased, Sarah Dobner, whilst at the station, complained of great difficulty of breathing, and said she was in great pain. A medical man advised her removal to the hospital, but it was then believed she was dead. Mr. Anderson, one of the surgeons at St. Mary's Hospital, who made the *post mortem* examination, said the deceased was labouring under disease of the bronchial gland, and undoubtedly the suffocating air of the Underground Railway had accelerated death. The coroner said he had experienced the depressing effect of that railway, and therefore avoided it as much as possible. The tunnels and stations should be ventilated, but he supposed that would not be done until some shocking loss of life from suffocation had occurred. The jury returned a verdict of "Death from natural causes, accelerated by the suffocating atmosphere of the Underground Railway." The Underground Railway is, however, not the only place where a neglect of the most ordinary dictates of common sense has occasioned loss of life. The *British Medical Journal* last week chronicled the poisoning of a young clerk by the bad atmosphere of a small telegraph-office room, ill ventilated, and with four gas-burners, of which "all the clerks had complained." This is only a rapid and compressed view of a tragedy constantly being worked out more slowly in work-rooms and offices, and approximately imitated by the poisoning and illness due to the bad ventilation of our gas-lit theatres, churches, and ball-rooms.

Double Sesquichloride of Iron and Sodium.—I produced this combination, which, as far as I know, is a new one, by the action of hydrochloric acid upon artificial ultramarine. The silicate becomes separated, and if water be added and the solution filtered, the double chloride in question, which is colourless, will be found in the filtrate. The following equation will serve as a further explanation:—

$$\left(\left.\begin{matrix}Na_2\\Al\\Si_2\end{matrix}\right\}O_8+Na_2O+FeS_2\right)+8HCl=$$

$$=\left.\begin{matrix}Na_2\\Al\\Si_2\end{matrix}\right\}O_8+H_2O+3H_2S+\left.\begin{matrix}Na_2\\Fe\end{matrix}\right\}Cl_8$$

By writing upon paper with the solution and afterwards warming it, the letters become black, just as in the case of some sympathetic inks. As the writing does not disappear again by the action of water, it is to be supposed that sesquichloride of iron and sodium is decomposed by heat—*J. Landauer.*

The Standard Pound.—It appears from the reports of the Comptroller-General of the Exchequer that the Exchequer standard avoirdupois pound is actually at the present time in a most unsatisfactory condition, "oxidated on the surface, practically erroneous on the face of it, and known to be erroneous;" and as all the Exchequer standard weights are made of brass, a metal stated to be peculiarly liable to oxidation in the atmosphere of London, and described by Professor Miller as "quite unfit, unless well protected by gilding, to be used in the construction of weights having that degree of accuracy which is required in secondary standard," it is to be feared that others of these Exchequer standard weights may be wanting in accuracy.

Petroleum as Fuel.—The idea of employing petroleum as a substitute for coal in the generation of steam has for some time engaged the attention of scientific men in America, and recently, under the auspices of the Navy Department, a series of experiments on the subject has been carried on at Boston, and there seems a fair prospect that the investigations will result successfully. A gunboat called the Palos was used for the experiments. She had been built for the Government, to make a speed of eight knots an hour, and with coal could never be forced beyond that. First, she was tied to the dock, and the possibility of getting up steam with petroleum was demonstrated. She was then sent on a trial trip down the harbour. Steam was got up with petroleum in 25 minutes, and the Palos steamed down the harbour and back, a distance of 25 nautical miles, in 1 hour and 55 minutes. In making this trip she consumed but four barrels of petroleum! The fires are reported to be kindled and extinguished with nearly the same ease as lighting and extinguishing a gas-burner. The furnaces of the Palos, originally built for burning coal, were fitted at comparatively small expense with burners to which the petroleum was led by pipes from the tanks on deck. The burners, by their own heat, turn the petroleum in the pipes into gas, and in this form it is burnt. The flames produced are intensely hot, and the petroleum burnt on the trip produced as much steam as 20 times its bulk in coals—a great saving of room in ocean voyages. The dangerous properties of the petroleum appear to be the only drawback to its use in this way, for coal-burning furnaces can be adapted to its use at but a trifling expense. The supply of petroleum is now so much greater than the demand that, even with three-fourths of the wells in the producing regions abandoned it can be bought for 2d. a gallon. Its cheapness is therefore another strong inducement to use it for generating steam.

The Queen's English at Paris.—The following is a literal copy of a handbill which has been extensively circulated in the Exhibition by a Spanish firm: "Blacking, ocly and resinous, titled the emperor of the blackings black ink and of allcolours to write with of D. J. G. . . . member of the national academy of Great Britain. This Blackings is knoconed to be the most useful for the conservation of the shes, for its brilliancy, solidity, and complete discomposition of the black animal. Mr. J. G. dus a present of £20 sterling to the person that will present hum a blacking in paste, that will reunite the same conditions, as the Emperor of the Blackings."

Popular Scientific Information.—The following paragraph, from the pages of a weekly contemporary which makes great pretensions to accuracy, shows the kind of science on which the mining public are fed:—"An invention has been provisionally specified by Mr. Bonneville, of Paris, for obtaining white lead direct from the ore, by pouring the molten metal into cold water, to render it as porous and bulky as possible; it is then dissolved in sulphuric acid, and the sulphate is treated with pyroligneous or oxalic acid, combined or not with tincal dissolved in water, and next dried over the fire on ways. The vessels employed are either made of stone or wood, lined with lead, which become coated with a protecting covering of lead."

Red Lead, according to Barton, may be produced by heating oxide of lead to redness with nitrate of soda, or by heating at the same temperature a mixture of 1,894 parts of sulphate of lead, 665 parts of carbonate of soda, and 177 parts of nitrate of soda. The resulting mass is to be washed. —*Berggeist*, No. 50.

Poisoning by Chlorine Vapour.—Professor Maisch says that a direct antidote to the poisonous effects of the inhalation of chlorine is sulphuretted hydrogen, the halogen combining instantly with the hydrogen, liberating sulphur. The professor has tried it himself after accidentally inhaling chlorine, and obtained immediate relief. The same remedy would doubtless be effectual in cases of bromine poisoning.

The Poisonous Action of Phosphorus.—Contrary to the current doctrine that death in case of poisoning by phosphorus results from fatty degeneration of the liver, produced by phosphorous acid, M. Dybkowsky states in a recent memoir that the toxic result is entirely due to the formation of phosphuretted hydrogen gas, which in passing through the blood completely uses up the oxygen present. Hence he concludes that death from phosphorus is nearly equivalent to death by asphyxia.—*Medical Times and Gazette.*

Methylated Spirits.—After the 1st of October next the duty on licenses to retailers of methylated spirits is to be reduced to 10s. per year.

Anti-incrustation Mixture for the prevention of the formation of sediments (strongly adhesive) in steam boilers. 125 kilos. of crystallised chloride of barium dissolved in 50 kilos. of water with addition of 25 kilos. of hydrochloric acid (specific gravity 1·20). To every 1000 litres = 1 cubic mètre = 35·5 cubic feet English, 15 litres of this acid solution should be applied.—*Elsner's Chemisch. Technisch Notizen.* 1867.

Technical Education.—The following extract from *The Times'* account of the "safe contest," forms an appropriate commentary on the remarks on Technichal Education in the supplement to last week's Chemical News (*Amer. Reprint for Oct.*, 1867, p. 175):—"Mr. Herring's workmen were three intelligent Germans—one of them a man of marked ability—he spoke three languages—and went about his business in the most scientific way. Mr. Chatwood's men were three Lancashire men, who represented brute force rather than intellect. One of them had a wonderful touch both for power and precision; but that was the only thing remarkable in the party. Any one who saw the contest between these two sets of workmen would carry away a very vivid idea as to the nature of the race which is now being run between the manufactories of England and those of the Continent. It is an admitted fact that the Continent has made an immense stride in advance, that the progress which we in England have made in the last 15 years is as nothing compared with that which our continental rivals have made, and that it will cost us a good deal in the future not merely to hold our own, but to save ourselves from being disgracefully beaten. The chief reason which the most intelligent observers assign for this change in our position as manufacturers is to be found in the superior education of the continental workman. He has gone through a regular course of instruction at some polytechnic school, he has been trained to appreciate principles, and he brings the exercise of brain to his work. An English foreman is of another stamp. He has had no special education; he has risen from the ranks; he knows his business by rote and rule of thumb; long practice has given him a certain mechanical facility of touch, but science he has none. By the very laws of the trade to which he belongs science is in a manner forbidden to him. Science will teach him to do in an hour what hitherto has occupied him two hours. This science is expressly forbidden him, because it would interfere with the wages of his comrades and take the bread out of their mouths."

Silicates of Methyl.—C. Friedel and J. M. Crafts first tried to prepare this body by reacting on methylic alcohol with chloride of silicium; like Ebelmen, they obtained a product impossible to purify, turning brown in the air and possessing a fœtid odour. They noticed that this product always contained chlorine. Wood spirit was purified by treatment with chloride of calcium; the chloride of calcium compound decomposed with water, and the alcohol rectified several times with sodium. The alcohol thus prepared was sealed in a tube with silicate of ethyl, and the mixture heated during 20 hours at 210° C. After several fractional distillations the principal product isolated from the contents of the tube was a liquid boiling at 143° to 147°. This liquid gave on analysis numbers which correspond with the composition of a mixed silicate, diethylic, dimethylic, silicic ether. There being reason to believe that a minute trace of water contained in the methylic alcohol interfered with the success of the processes in which it was employed, this alcohol was distilled twice with sodium, then with a small quantity of anhydrous phosphoric acid. Thus prepared, it boils at 65·5°, has not the disagreeable odour it usually has, smells like common alcohol, and does not turn brown with soda. Methylic alcohol purified in this way, when added to chloride of silicium, reacts like ordinary alcohol. When the theoretical quantity of the alcohol has been added, the product is distilled, and after a small number of fractional distillations two principal products are obtained, one boiling at 120° to 122°, and the other at 201° to 202·5°. The first is the normal silicate of methyl; the second is the hexamethylic disilicic ether. The normal silicate of methyl is a colourless liquid, has rather an agreeable odour, is soluble in a considerable quantity of water. Moisture or aqueous alcohol gives rise to condensed products, ultimately silica. It burns with a white smoke composed of silica.—*Silliman's Journal*, May, 1867.

Nitroglycerine.—The destruction of the *European* at Colon by an explosion of nitroglycerine, has been the cause of an action at law. The plaintiffs—the West India and Pacific Steamship Company—were the owners of the vessel; the defendants—Guion and another—merchants and forwarding agents. The defendants had received from their correspondents in Hamburg, Messrs. Bandemann, 70 cases of oil, described in the letter of advice as "glonoin oil," and one of 20,000 percussion caps to be forwarded to San Francisco. It appeared that the defendants knew very little concerning the oil they were shipping. Professor Abel, F.R.S., Professor Roscoe, F.R.S., Colonel Boxer, F.R.S., Superintendent of the Woolwich laboratory, gave evidence as to the nature of the material. By their evidence it was shown to be a highly explosive substance, the explosion being excessively rapid and unaccompanied by smoke, and it is produced by heat. Professor Abel stated that the greater the quantity of oil the lower would be the degree of temperature necessary to explode it, and having in experiments exploded 12 to 20 drops by keeping them for six hours daily at a temperature of 180°, he had arrived at the conclusion that a temperature of 110° to 130° would explode the quantity contained in one of the cases. It appeared that the commercial article, being far from pure, contained a certain quantity of free acid, which generated a gas and produced decomposition, which increased the heat, and the decomposition again was increased with the temperature, and all this tended towards explosion. Moreover when the compound became saturated with this gas it became increased in bulk, and its pressure against the sides of the case became stronger, rendering it more liable to be exploded by concussion. To the oil and to the operation of these qualities in it they did not hesitate to assign the explosion, and Professor Roscoe stated that one case of this oil exploded would have destroyed the *European*. Colonel Boxer proved that it was practically impossible to explode a great quantity of percussion caps at once, for, although the explosion of one extended to the few immediately surrounding it, it never went further, and he produced a box of caps upon which he had let a hundredweight fall from a height of 6ft., and it appeared crushed and out of shape, but the caps were unexploded. Moreover, he had put two ounces of gunpowder into the centre of 2,000 caps in a cylinder and placed the whole in a packing-case and exploded the powder. One-third of the caps only were exploded, and the others were untouched. The mode of clearing the composition out of caps, when it is necessary to do so, is by shovelling 20,000 at a time into a heated oven; there is no explosion, and a minute at least elapses before the composition is consumed. The result of all this is that caps will not explode in a body and are not practically dangerous, and in consequence, the practice is to issue all caps from Woolwich, packed in the middle of the packages of small ammunition; and no case of accidental explosion has ever occurred. For the defence, it was contended that the explosion might have been caused by torpedoes to be used by Chili against Spain, loaded amongst the cargo without the owners' knowledge. The jury found the oil to be the cause of the explosion.

CONTEMPORARY SCIENTIFIC PRESS.

[Under this heading it is intended to give the titles of all the chemical papers which are published in the principal scientific periodicals of the Continent. Articles which are merely reprints or abstracts of papers already noticed will be omitted. Abstracts of the more important papers here announced will appear in future numbers of the Chemical News.]

Monatsbericht der königlich-Preussischen Akademie der Wissenschaften. December, 1866.

Ehrenberg: "*Contributions to the Knowledge of the Formation of Organic Siliceous Deposits.*"

January, 1867.

Baeyer: "*On the Effect of Age on the Length and Coefficient of Expansion of Iron and Zinc Metrical Standards.*"

Kunst and Gewerbeblatt. February, 1867.

O. Reinsch: "*On Colouring thin Sheets of Metal, and rendering Membranes, Fabrics, and Glass Iridescent.*"—A. Vogel: "*On the Assimilation of Silica by Plants.*"—H. Vohl: "*On the Poisoning of Bread by the Use of Old Building Timber and Railway Sleepers for Heating Bakers' Ovens.*"—A. L. Treen: "*On a Substitute for Urine for Washing Wool.*"

No. 3. March.

E. Ziegler: "*On the Preparation of a Substitute for Anima Charcoal.*"—H. Avet: "*On the Production of Printing Surfaces by Photography.*"—O. H. Schmidt: "*On the Water Supply of Ludwigsburg.*" Paris Exhibition of 1867: Catalogue of Bavarian Products: "*On the Use of Turpentine for Whitening Household Linen.*" "*On the Electrolytic Deposition of a bright Layer of Silver.*"—F. F. Kuckla: "*Apparatus for Testing the Inflammability of Petroleum.*"—J. Fuchs: "*On the Use of Glycerine for impregnating Casks for holding Oil, Petroleum, and Turpentine.*" "*Receipt for Copying Ink.*" "*On Water Glass as an Ingredient for Cement.*"

Bulletin de La Société d'Encouragement. February, 1867.

Chatin: "*Report on Mauban's Thermometer for ascertaining Temperatures at Different Depths.*"—Paris: "*On the Manufacture of Articles of Enamelled Iron.*"—C. Lauth: "*On the Manufacture of Aniline Black.*"—Bon: "*On the Manufacture of artificial Precious Stones.*"—Guesnier: "*A Method of Producing Castings having an External Coating of Hard Iron.*"—Guiot: "*A Thermoscopic Barometer.*"—Havrez: "*An Improved Lixiviating Apparatus.*"—Lacroix: "*On the Manufacture of Colours for Painting on Glass.*" "*On a dangerous Adulteration of Petroleum.*"

Journal fur Praktische Chemie. March 28, 1867.

G. Nadler and V. Merz: "*On Chinoline Blue.*"—F. Holm: "*On Hæmatoidine.*"—G. Stadeler: "*On the colouring Matter of the Yolk of Egg.*"—F. Holm: "*On the Chemical Constituents of the Suprarenal Capsules.*"—W. Dypkowsky: "*On the Identity of Choline with Neurine.*"—L. S. Igelstrom: "*On some new Minerals from Wermland and Oerebro.*"—Bottger: "*Experiments with Mialaret Becknell's Constant Battery.*" Bottger: "*On the Action of Water on Metallic Lead.*"—G. Streit: "*On the Preparation of Carbonate of Thallium.*"

Journal fur Praktische Chemie. April 4.

C. Heintzel: "*On Triamidophenol and Amidodiimidophenol.*"—K. Frish: "*Researches on Creosote*"—A. W. Hofmann: "*On the Transformation of the Aromatic Monamines into Acids containing a larger Proportion of Carbon.*—F. Rochleder: "*On the Constituents of the Root Bark of the Apple Tree.*" "*On the Elementary Analysis of Organic Substances.*"—K. Frisch: "*On a Method of testing Fused Soda.*"—Schrotter: "*On some Nickeliferous Cobalt Ore from Dobschau.*"—Hlasiwetz and Grabowski: "*On Carminic Acid.*"—Hlasiwetz: "*On Caffeic Acid.*"—F. Rochleder: "*On the Action of Nascent Hydrogen on Chinine, Cinchonine, and Caffeine*"

April 15, 1867.

W. Michaelis: "*On Portland Cement.*"—Peterson: "*Analysis of Apatite from Diez, in Nassau.*"—A. Baeyer: "*On the Constitution of Mellitic Acid.*"

Le Technologiste. May, 1867.

W. Naylor: "*On Smoke consuming Furnaces.*"—W. De La Rue and H. Muller: "*On the Extraction of Copper and Silver from Iron Pyrites.*"—C. Lunge: "*On the Manufacture of Nitrate of Potash and of pure Carbonate of Potash.*"—C. Dietrich: "*On the Preparation of Chrome Green.*"

Journal des Fabricants de Papier. March 15, 1867.

E. Bourdilliat: "*On Testing the Chemical Products used in Paper Making.*"—E. A. Cotelles: "*Apparatus for utilising Chlorine Residues.*"—Taquet: "*A Method of Removing Boiler Scale by Means of Potash.*"—J. M. Mellor: "*A Method of softening, separating, and bleaching Vegetable Fibres.*"

April 1.

E. Bourdilliat: "*On testing the Chemical Products used in Paper-making.*"—A. Ott: "*On the use of Petroleum for lubricating Machinery.*"—Orane: "*A new Safety Paper for preventing Bank Note Forgeries.*"

April 15.

E. Bourdilliat: "*On testing the Chemical Products used in Paper-making.*"—H. Berge: "*On Lignite from Moldavia.*"

May 1.

E. Bourdilliat: "*On testing the Chemical Products used in Paper-making.*" "*On the Estimation of Wood Pulp in its dry State.*"

Genie Industriel. May, 1867.

P. Du Rieux and Roettger: "*An Improved Cylindrical Filter Press.*"—Bloch: "*A Method of joining Lead and other Metal Tubes.*"—C. Stammer: "*On the comparative Advantages of Washing Sugar Filters with Hot and Cold Water.*"—T. Gray: "*On the Preparation and Treatment of Flax and Hemp.*"—Marechal and T. Du Motay: "*On Bleaching Animal and Vegetable Fibres*" "*On the Use of Kamptulicon as Armour Plating.*"—Guenier-Lauriac: "*A Method of Casting Rolls, Columns, and Guns with an external Skin of Hard Iron.*"—Leveque: "*Apparatus for Charging and Collecting Gases from Blast Furnaces.*"—Schlotterbek: "*Varnish for protecting the Surface of Iron from Rust.*"

Sitzungsberichte der Wiener Akademie (Mathematical and Physical Section). December, 1866.

W. F. Gintl: "*A new Pinch Cock for stopping India Rubber Tubes.*"—F. Rochleder: "*On the Elementary Analysis of Organic Substances.*"—K. Fritch: "*On the periodical Phenonena of Plants.*" "*On the ripening of Seeds and Fruits in Australia.*"—A. Brio: "*On the Crystalline Form of Fomiate of Baryta.*"

Comptes Rendus. No. 19.

Becquerel: "*Memoir on some newly-discovered Chemical Effects of Capillary Action.*"—Boussingault: "*On the Effect of Mercury Vapours on Flowers.*"—C. Deville: "*On the Periodical Variations of Temperature.*"—Civiale: "*Description of a Collection of Urinary Calculi, classified according to their Structure and Development.*"—C. Briot: "*On Crystalline Reflection and Refraction.*"—Potier: "*Researches on the Diffraction of Polarised Light.*"—H. Marie-Davy: "*On the 'Electric Mass' of Conductors.*"—P. P. Deherain: "*Experimental Researches on the Use of Potash Salts in Agriculture.*"—J. J. Chydenius: "*On Pseudo-Hexyl-Urea.*"—E. Grimaux: "*On the Brominated Derivatives of Gallic Acid.*"

Annalen der Chemie und Pharmacie. April, 1867.

C. D. Braun: "*On the Formation of Certain Cobalt Amine Compounds.*"—C. Engler: "*On the Action of Bromine on some Nitriles.*" "*On the Action of Ammonia on Trichlorhydrine.*"—R. Otto and O. Von Gruber: "*On Toluol-sulphurous Acid.*" R. Otto: "*A simple Method of Preparing Crystallized Sesquioxide of Chromium.*"—C. Weltzien: "*On the Hydrated Suboxide and Oxide of Silver.*"—"*On the Formation of Ozone.*"—A. Siersch: "*On the Preparation of the Fatty Alcohols from their Primary Members.*"

May.

L. Carius: "*On the Synthesis of Organic Acids.*"—E. Schulz and A. Reinecke: "*On the Analysis of Animal Fats, especially those of Mutton, Beef, and Pork.*"—H. Hlasiwetz: "*On some Tannic Acids.*"—H. Hlasiwetz: "*On the Brominated Derivatives of Gallic Acid, and Oxyphenic Acid.*"—Guckelberger: "*On the Extraction of Thallium.*"

Annales de Chimie et de Physique. April, 1867.

C. Marignac: "*On some Fluates of Antimony and Arsenic.*"—Wyrouboff: "*On the Optical Properties of some new Tartrates.*"—F. Rossetti: "*On the Maximum Density and on the Expansion of Distilled Water.*"

Bulletin de la Société d'Encouragement. March, 1867.

Puscher: "*On a new Gold-coloured Lacquer for Brass Articles.*" "*On the Extraction of Copper by Hydrochloric Acid as used at Braubach* (Nassau)."

Annales du Génie Civil. May, 1867.

L. Droux: "*On the Providencia Soap and Candle Works in San Sebastain, in Spain.*"—N. Basset: "*On G. Ville's Improved Artificial Manure*"—Paquot: "*On the Use of Chloride of Barium for preventing the Formation of Boiler Scale.*"—Libert: "*On the Use of Catechu for Preventing the Formation of Boiler Scale.*"—Taskin: "*On the Prevention of Boiler Scale.*"—A. Souheur: "*A new Petroleum Safety Lamp for Mines.*" "*On the Use of naturally-formed Gas for Illuminating Purposes.*"

Dingler's Polytechnische Journal. April, 1867.

C. Schinz: "*On Lundin's Regenerative Gas Furnace.*" "*On Regenerative Gas Furnaces as applied to the Manufacture of Glass.*" "*On Lindner's Theory Respecting the Influence of the Form of Fuel on Combustion.*"—L. Ramdohr: "*On the Production of Gas from some of the Waste Products of the Manufacture of Creosote.*"—E. Sostmann: "*On the Use of Paraffin in the Manufacture of Sugar.*" L. von Liebig: "*On the Diseases of Silkworms.*" Nessler: "*On MacDougall's Disinfecting Powder for Stables.*"

April 13, 1867.

"*On a Method of renovating Files by etching with Sulphuric Acid.*"—A. Patera: "*On the Electrolytic Precipitation of Copper from its Solutions.*"—C. Aubel: "*On a new Method of Extracting Copper from slags by means af dilute Sulphuric Acid.*"—E. Brimmeyr: "*On the different Processes for utilising the Refuse of the Fuchsine Manufacture, and for reobtaining the Arsenic Acid.*"—L. Walkhoff: "*On Dubunfaut's Method of obtaining Sugar from Molasses by Dialysis.*"—J. C. Lermer: "*On the Alkaloid of Beer.*"

Comptes Rendus. May 20, 1867.

Boussingault: "*On the Effects of Mercury Vapours on Plants.*"—Liebig: "*On Artificial Milk for Infants.*"—Guyon: "*On the Effect of the Sting of the Scorpion.*"—F. Teynard: "*On the Calculation of the Numerical Elements of a Simple Achromatic Objective for Photography.*"—Namiass: "*On the Use of Bromide of Patassium as a Remedy for Epilepsy.*"—C. Flammarion: "*On a Change which has taken place in the Crater of Linnæus in the Moon.*"—Chacornac: "*On the same subject.*"—J. B. Baille: "*Researches on the Variations in the Dispersive Power of Liquids under the Influence of Heat.*"—Veltee: "*On the Effects of Silicate of Potash when applied as a Manure on the Laying of Cereals, and on the Strength of the Stems of Cereals.*"—Fritzsche: "*On the Solid Carbides of Hydrogen obtained from Coal.*"—S. de Luca: "*Analysis of Water from a Bronze Vase discovered at Pompeii.*"—M. Perret: "*An Improved Wine Fermenting Vat.*'—A. Bechamp: "*On Pasteur's Memoir on the Corpuscles of the Silk Worm Disease.*" "*Some new Facts on the present Silk Worm Disease, and on the Nature of the Vibrating Corpuscle.*"—Balbiana: "*On the Supposed Reproduction by Fission of the Corpuscles or Psorosperms of the Silk Worm Disease.*"—E. Cyon: "*On the Influence of Carbonic Acid and Oxygen on the Heart.*"

May 27.

T. Graham: "*On the Occlusion of Hydrogen Gas by Meteoric Iron*"—J Fournet: "*On the Path of Storms in the Department of the Rhone.*"—P. Desains: "*Researches on the Absorptive Action of Ether and Formic Ether and their Vapours on the Heat radiated from a Lamp furnished with a Glass Chimney.*"—A. Wurtz: "*On the Synthesis of Methyl-Allyl.*'—C. Mene: "*An Analysis of some Crystallized and Amorphous Graphites.*"—D. de Luca: "*On the Use of Crystallised Sulphate of Soda for restoring the Transparency of the Cornea.*" Hulot: "*On the Use of Aluminium Bronze for making the lower Die of Machines for Perforating Postage Stamps.*"—H. Deville: "*On Hulot's Improved Hard Solder, formed by mixing Zinc-amalgam with ordinary Solder.*" "*On the Rapid Oxidation of Alloys of Lead and Platinum when exposed to the Air.*"—E. Duclaux: "*On a Hydrate of Bisulphide of Carbon.*"

June 3.

E. Becquerel: "*Notice of the Author's Work on 'Light, its Causes and Effects.'*"—L. Pasteur: "*Two Letters to Dumas on the Silk Worm Disease.*"—A. Secchi: "*Resumé of Observations on Sun Spots for the first Six Months of the year* 1866." "*On the reported Disappearance of the Crater of Linnæus in the Moon.*"—J. Chautard: "*Researches on the Magnetism and Diamagnetism of Gases.*"—J. Rosenthal: "*On Phenomena observed in Cases of Poisoning by Strychnine.*"—Le Ricque de Monchy: "*On the Use of Creosote in the Rearing of Silk Worms.*"

PATENTS.

Communicated by Mr. Vaughan, F. C. S., Patent Agent, 54, Chancery Lane, W. C.

GRANTS OF PROVISIONAL PROTECTION FOR SIX MONTHS.

1212. E. Guenin, Henrietta Street, Covent Garden, Middlesex, "Improvements in the preparation and application of mustard for curative purposes." A communication from P. Rigollot, Paris.—Petition recorded April 26, 1867.

1549. C. Sanderson, Worksop, Nottinghamshire, "Improvements in the manufacture or melting of cast steel."—May 24, 1867.

1583. W. Mitchell, Northwold, near Brandon, Norfolk, "An improved food for sheep and other animals.'—May 28, 1867.

1628. A. G. Schaeffer, Gloucester-street, Newcastle-on-tyne, "Improvements in obtaining increased light in the combination of illuminating matters."—June 1, 1867.

1635. W. H. Richardson, Glasgow, N.B., "Certain improvements in the manufacture of iron and steel, and in the means or apparatus for effecting the same."—June 3, 1867.

1646. E. Meldrum, Bathgate, Linlithgow, N.B., "Improvements in the purification of paraffine."—June 4, 1867.

1655. G. White, Queen Street, Cheapside, London, "Improvements in the manufacture of hydrate and carbonates of soda."—A communication from F. Biall, Turin, Italy.—June 5, 1867.

NOTICES TO PROCEED.

325. J. Wright, and T. Cobley, Copthall Court, Throgmorton-street, London, "Improvements in the treatment of ores of lead for the purpose of obtaining salts and colours of the same."

337. J. Graham, Manchester Road, Warrington, Lancashire, "Improvements in the manufacture of spelter from zinc ashes and refuses, obtained when coating iron with zinc."—Petition recorded, February 6, 1867.

344. G. E. Pain, High-street, Camden Town, and C. Corry, Dean-street, Soho, Middlesex, "Improvements in the preparation of oils for luminating, lubricating, and other purposes for which they may be applicable." February 7, 1867.

1412. A. A. Bonneville, Rue du Mont Thabor, Paris, "An improved washing powder." A communication from L. Lacalm, and B. A. Gayot, Aubin, France.—Petition recorded May 13, 1867.

GRANTS OF PROVISIONAL PROTECTION FOR SIX MONTHS.

1412. H. A. Bonneville, Rue du Mont Thabor, Paris, "An improved washing powder." A Communication from L. Lacalm, and B. A. Gayot, Aubin, France.—Petition recorded May 13, 1867.

1617. F. W. Dolman, Jermyn-street, St. James's, Middlesex, "A new or improved method for obtaining an essential oil applicable as a curative agent or medicine, in the treatment of inflammatory joint diseases, rheumatism, sprains, bruises, and similar ailments." A communication from L. Jamieson, Buxar, Bengal, India.—May 31, 1867.

1712. J. Graham, Banford, Gilford, Down, Ireland, "An improved mixture or composition for bleaching vegetable fibres."

1713. H. Fletcher, Old Hall-street, Liverpool, "Improvements in the manufacture of artificial fuel."—June 11, 1867.

1728. A. M. Clark, Chancery Lane, "Improvements in the manufacture and treatment of white lead, and in apparatus connected therewith."—A communication from R. G. Hatfield, New York, U. S. A.—June 12, 1867.

1747. J. Onions, Devon Place, Newport, Monmouth, "Improvements in the manufacture of steel."

1748. G. M'Kenzie, Glasgow, N.B., "Improvements in the manufacture of illuminating gas."—June 15, 1867.

1803. H. K. York, Cardiff, "Improvements in the manufacture of steel."—June 20, 1867.

1819. G. Dickie, Kilwinning, Ayrshire, N.B., "Improvements in the manufacture of illuminating gas."—June 21, 1867.

1836. J. K. Field, Upper Marsh, Lambeth, Surrey, "Improvements in the manufacture of candles."—June 24, 1867.

1845. J. Webster, Birmingham, "A new metallic zinc paint."

1846 J. Crow, West Ham, Essex, "Improvements in the manufacture of illuminating gas from gas tar oil, or from gas tar."

1850. L. Brunetti, Rovigno, "An improved process of embalming and preserving animal substances from decay, for anatomical purposes."—June 25, 1867.

1886. C. O. Heyl, Berlin, "An improved method of, and apparatus for, making sulphuret of carbon."

1888. J. C. Sellars, Birkenhead, Cheshire, "Improvements in the utilisation of a certain waste material obtained in the manufacture of hydrocarbon liquids."—June 28, 1867.

1900. A. M. Fell, West Calder, Midlothian, N.B., "Improvements in purifying or preservative compounds to be applied to the fleeces or skins of sheep and other animals."

1905. W. H. Richardson, Glasgow, N.B., "A certain improved mode of manufacturing iron and steel."—June 29, 1867.

1950. A. E. Herrmann, Mincing Lane, London, "An improved lubricating compound." A communication from H. Waltjen, Bremen.—July 3, 1867.

2006. G. Gabillon, Rue Joquelet, Paris, "A process to prepare and preserve paper and tissues with a solution of per-chloride of iron, intended for stopping the bleeding of wounds."—July 9, 1867.

2034. J. H. Johnson, Lincoln's Inn Fields, Middlesex, "Improvements in the manufacture of refined sugar." A communication from E. P. Eastwick, Baltimore, Md., U. S. A.—July 11, 1867.

2046. J. Hargreaves, Appleton-within-Widnes, Lancashire, "Improvements in the manufacture of steel and soft iron from cast iron."

2055. A. E. Herrmann, Mincing Lane, London, "An improved compound to be used for igniting fires." A communication from H. Waltjen, Bremen.—July 12, 1867.

2100. J. H. Johnson, Lincoln's Inn Fields, Middlesex, "Improvements in the treatment and purification of oils." A communication from F. Asselin, Paris, France.—July 17, 1867.

NOTES AND QUERIES.

Stearic Acid in Paraffin.—Sir,—Is there a test for the presence of stearic acid in paraffin?—X.

Oxidation of Aniline.—Sir,—I should feel obliged to any of your readers who would give me the name and formula for the crystals resulting from the action of bichromate of potash on an acid solution of sulphate of aniline.—$C_{12}H_7N$.

Wires for Micrometers.—Sir,—If any of your ingenious readers can suggest anything which can be used for the purpose of micrometer wires for the microscope, they will confer a favour on the undersigned. At present spider threads, fine platinum wire, and diamond marks on glass are used, but they are all open to objections.—O. Ramsbottom.

Nitrogen.—Sir,—I require absolutely pure nitrogen gas for some experiments. I have instituted a few trials of different methods which appear likely to be successful, but it has occurred to me that, perhaps, a few lines from one of your correspondents may save me the trouble of further investigation. I may state that the purpose for which I require the nitrogen makes me anxious not to prepare it from air. Is there any way of liberating it from a solution or mixture so that the amount and velocity of the supply may be varied at will?—Theta.

Hardening Steel.—Sir,—It has recently been mentioned that filemakers at Sheffield prefer, for hardening their steel goods, water that has been for a long time in use. They say "old water hardens much better than new water;" and amongst the acts occasionally indulged in by workmen against their employers, that of making a hole in the hardening tank and letting the water run away, is considered to inflict injury for some considerable time. Is there any foundation for this opinion?—A Sheffield Blade.

Commercial Testing of Aniline Colours.—Sir,—The ordinary method of determining the value of these colours, viz, dyeing swatches of equal weight and of equal quantities of the various samples in question, is quite satisfactory for reds and blues, but fails completely for purples, violets, and all intermediate shades. The reason is that these colours, instead of being homogeneous, are now frequently made by mixing an ordinary "magenta" with *bleu de Lyons* in the required proportions, or, in other cases, by adding to a violet or purple colour a little magenta or aniline blue, to alter the shade according to wish. Now, a mixed colour of this kind may dye a swatch in the most satisfactory manner, but when used in the large scale the goods first put into the pan and those entered last will have quite different shades, owing to the unequal affinities of the colours for the fibre, and to their different behaviour with the other substances present. The method which I employ to determine whether an aniline violet is homogeneous or a mere mixture, is the successive operation of different solvents. Thus a sample of this nature was treated with hot water and filtered. The filtrate exactly resembled a solution of common magenta in hot water, and gave a worsted the colour which such a solution would produce. The residue, insoluble in water, was then dissolved in alcohol, and was found to be an ordinary aniline blue. If a mixed colour is dissolved in spirit and diluted with water, a drop of the liquid let fall upon white blotting paper will exhibit concentric rings of colours.—W.

Stearic Acid in Paraffin.—Sir,—In reply to your correspondent "X's" query in last week's Chemical News (Amer. Reprint for October, 1867), I can recommend him to try Wagner's test. Dissolve the suspected paraffin in boiling alcohol, and add to it an alcoholic solution of acetrate of lead. If stearic acid be present a white precipitate will fall of stearate of lead, but if the paraffin be pure no precipitation will take place. I can speak from experience as to the value of this process.—B. Hoffman.

Wires for Micrometers.—Sir,—I think perhaps the best thing your correspondent, "O. Ramsbottom," can use for the above purpose is asbestos. This was suggested many years ago by Professor Wallace, who states that fibres of this substance of the 1-3000th of an inch in diameter give a line beautifully even under the microscope and of considerable capacity; the subdivision of the substance can be carried to almost any degree of minuteness.—J. S. S.

Nitrogen, Preparation of.—Sir,—"Theta" will probably find the following plan answer his purpose:—Half fill a tubulated retort with dry nitrate of ammonia. By means of a wire passing stiffly through a cork in the tubulus suspend a piece of zinc so that it can be moved up and down at will. Fuse the nitrate of ammonia by heat, and then push the zinc down into it. Nitrogen and ammonia will be evolved, and the latter can be absorbed by collecting over water or by passing through Woulff's bottles containing water and dilute acid. By adjusting the height of the zinc and the temperature of the salt the disengagement of gas may be varied as desired.—S. Peterson.

Picric Acid.—Sir,—Out of the many works on chemistry that I have, besides the last seven volumes of the Chemical News, I have not in them all a good process for making picric acid. All the processes that one reads are thus:—Picric acid is formed by acting on carbolic acid with nitric acid, assisted by a little heat. Now, what I want to know is, the proportions of carbolic acid and nitric acid, and the strength of the nitric acid, and the degree of heat, and the length of heat, and the length of time that it has to be heated; or, in other words, a good process for making it. If you can give me the above information through the News, I shall feel extremely obliged.—S. R.

Nitrogen, Preparation of.—A correspondent informs us, in answer to the query on this subject, that the best way to prepare nitrogen is to react on dilute ammonia with bromine. The ammonia is to be put into a tubulated retort, or Woulff's bottle, and the bromine poured in through a funnel-tube reaching to the bottom. Nitrogen comes off freely, and may be collected over water. No bromide of nitrogen is formed. The product is bromide of ammonium.

Oxalate of Cerium—Silicide of Aluminium.—Sir,—Could any of your correspondents inform me what remains on igniting the oxalate of cerium, and how to obtain silicide of aluminium?—C. R. N. P.

A Specific Gravity Problem.—Sir,—Might I, through your valuable column of Notes and Queries, ask assistance in the following case:—In examining some meteoric iron I found its sp. gr. (using 50 grains) to be 6·1, but as this iron was intimately mixed with olivine, I afterwards dissolved out the iron in acid, and found that 20 grains olivine were left behind, which had the specific gravity of 3·2. Now, how can I calculate the true specific gravity of the 30 grains iron actually present? I have no doubt that some of your correspondents can help me in this dilemma.

σιδερος.

Bone Boiling.—Can any of your readers tell me how to destroy the smell arising from bone boiling? or, can they inform me how to utilize size, the result of steaming bones? It is unfit for any stiffening process. If used in artificial manures how can it best be appropriated?—Oss.

Naphtha, ναφθα (No. 399).—This word, in the original Chaldee, signifies *stillare*, to ooze or drop; the true naphtha has been found from the most remote ages exuding out of the earth in several places in Chaldæa. One of the tribes of the children of Israël were the Naphthali (Nephthalim, Rev. vii. 6), inhabiting the shores of the Caspian Sea. No doubt the name of the people was derived from their country producing naphtha.—Septimus Piesse, Ph. D.

Specific Gravity Problem.—Sir,—Your correspondent will find the specific gravity problem solved in the following lines:—Weight of meteoric stone in water:

$$= 50-(50\div6{\cdot}1)=41{\cdot}8$$

Weight of Olivine in Water

$$= 20-(20\div3{\cdot}2)=13{\cdot}75.$$

Subtracting we obtain the weight in water corresponding to the 30 grains of dissolved matter 28·05.

Sp. gr. of matter dissolved out

$$=30\div(30-28{\cdot}05)=15{\cdot}384.$$

F. J. R. C.

Specific Gravity Problem.—Sir,—The following formula will, perhaps, answer the purpose of *Sideros*:—Let A., B., C.; a., b., c. represent the weights (in any given unit) and specific gravities of the compound and its two components respectively. Then assuming the volume of unit of weight of water as the unit of cubic contents, we have:

The cubic content of 1 unit of weight of compound body $\frac{1}{a}$

" " " " A $=\frac{H}{a}$;

" " " " B $=\frac{B}{b}$;

" " " " C $=\frac{C}{c}$;

$$\frac{C}{c}=\frac{A}{a}-\frac{B}{b}=\frac{A\,b-a\,B}{a\,b}$$

$$\therefore c=\frac{a.\,b.\,C}{A.\,b-a.\,B}$$

In the present case the sp. gr. would be

$$\frac{3{\cdot}2\times6{\cdot}1\times30}{(3{\cdot}2\times50)-(6{\cdot}1\times20)}=\frac{575{\cdot}6}{38}=15{\cdot}1458.$$

Lloyd.

Specific Gravity Problem.—Sir,—Your correspondent *Sideros* gives a most singular problem. Either it must be only a hoax or else there is some radical error in his observations. If instead of giving respectively the sp. gr. of the original substance and the remaining olivine, he had simply quoted their weight in water, it would have saved great trouble. But, notwithstanding the difficulty thus put into the question, I answer it according to his proposition, and the results induce me to believe in my first assertion, because iron of such a sp. gr. is preposterous. But here is my calculation, minus the method of finding the statements, which he omitted:—

Weight of the iron and olivine in air (original subst.)......	50 grs.
" " " " " " water....	8·1968
Weight of water equal in bulk to iron and olivine........	41·8032
Weight of olivine in air..................................	20 grs.
" " " water..................................	6·24969
Weight of water equal in bulk to olivine..................	13·75031
Weight of water equal in bulk to iron and olivine..........	41·8032
" " " " " " " olivine..................	13·75031
Weight of water equal in bulk to iron....................	28·05289

$$\frac{\text{Weight of iron in air}\ldots\ldots30}{\text{Weight of water equal in bulk to iron}\ \ 28{\cdot}0528}=1{\cdot}10506=\text{sp. gr. of the}$$

iron according to *Sideros* proposition.—C. H. P.

Manufacture of Size.—Can any of your readers inform me how to manufacture size used for sizing woollen warps, or if there is a treatise on it in any chemical work?—J. Morgan.

Picric Acid.—Sir,—I can give your correspondent "S. R." all the particulars he wants concerning the manufacture of picric acid. You can give him my name and address, if, at the same time, I have his.—B. Sc.

Platinum Metal.—Sir,—On dissolving some waste shreds of platinum I found that a small quantity was insoluble. It was in fine powder with perfect metallic lustre. Can any of your correspondents kindly inform me what it is likely to be?—C. R. N. B.

Preservation of Crystals.—Sir,— If any brother reader of the Chemical News will inform me of a convenient way to preserve fine crystals, or efflorescent substances, and salts which change on exposure to the air, he will confer a favour on —Cassio.

Tannate of Alumina.—Sir,—Is tannate of alumina formed by adding gall liquor to acetate of alumina, settling and filtering? If your correspondent "Effra" wants it for the aniline colours, he will find it to give a good lake by adding the colouring matter to the acet. alumina and precipitating with galls.—C. A.

Dyeing Black.—Sir,—Will you kindly solve me a problem that has troubled me long? Among other things I manufacture inks, and I always find black ink settle a good deal after standing 3 or 4 weeks, in fact, till it is almost clear. One purpose for which I use it is for inking cloth, and I require it to have a thickish body, but not to appear glossy, as when gum is put on, and also to dye cotton in the cloth and appear of a good blue colour. This is my formula:—Bruised galls, 20lbs.; chip logwood, 10lbs.; aqua, 33 galls.; boil 7 hours, strain and add copperas, 4 ozs. to the gallon. Do I put too much iron, or too little, or what is it that causes it to settle? Could you either give me a better formula, or tell me how to improve mine so as to be of a middling thickness and a bluish colour? If you will, I shall ever be thankful to you for so doing.—Bichrome.

Prevention of Dry Rot.—Sir,—As an architect I have felt very much interested in the statement made by Mr. G. Lunge in your paper of the 21st June (Amer. Reprint for August, 1867, p. 97), as to the use of tank waste for the prevention of dry rot, and should much like to give it a trial. But I do not quite understand from Mr. Lunge's letter as to whether the waste is to be in direct contact with the timber, or whether it is to be spread on the ground *under* it. I should be glad, also, to learn how the waste is to be obtained. He would, perhaps, kindly point out one or more alkali works at the east end or other parts of London. —T. H. L.

Specific Gravity Problem.—Sir,—Your correspondent "C. H. P." before writing of the "radical errors" in the observations of others, should be careful to be accurate in his own communications. He has evidently very confused ideas as to the relations between the weight of water equal in bulk to the weight in water of, and specific gravity of, a substance. I think the subjoined is a somewhat simpler solution of the problem in question than those of "F. J. R. C." and "Lloyd," the discrepancy between the three results being due to the difference in the number of decimal places used, and to error in "Lloyd's" calculation.

Weight of water equal in bulk to compound = 50 ÷ 6·1 = 8·1968
Weight of water equal in bulk to olivine = 20 ÷ 3·2 = 6·2496
Subtracting we obtain—
Weight of water equal in bulk to iron = 1·9472
Specific gravity of iron = 30 ÷ 1·9472 = 15·406

As this is about double the true sp. gr. of iron, I assume that *Sideros* has, for the sake of simplicity, given a supposititious example.

Henry Matthews, F.C.S.

Specific Gravity Problem.—Sir,—The different solutions of the above problem must have puzzled *Sideros* almost as much as the problem itself. In that furnished by "C. H. P." "there is some radical error," he having mistaken the weight of an equal bulk of water for that of the body in water, as will appear on comparing his calculation with that of "F. J. R. C." The discrepancy between "F. J. R. C.'s" result and my own is caused by his disregard of decimals beyond the 1st or 2nd place, the remarks with which "C. H. P." was pleased to preface his solution are, as it happens, quite *apropos*, for 15·1458 is an even more preposterous specific gravity than the one which he gives. I should imagine that *Sideros*' specimen of meteoric iron was composed of about 10 grains of olivine and 40 of pure iron, as those numbers will give a more nearly correct specific gravity (7·87) for the iron.—Lloyd.

Specific Gravity Problem.—Sir,—Only this moment returned from the Continent, I have not seen either of your two last numbers before to-day, and hasten to correct a mistake, doubtless of my own. The sp. gr. of the meteoric iron should have been stated at 4·9, not 6·1. For this I must apologise.—Sideros.

Naphtha.—Sir,—The emphatic "no doubt" of your correspondent who replies to the query put in No. 399, is somewhat amusing. The same idea had lurked in my mind for a long time, but I found it wouldn't fit. I can't quote Chaldaic, but we are most distinctly told that naphtali (*not naphthali*) means "my wrestling" (Gen. xxx. 8). This son of Jacob was born at least 350 miles from the Caspian Sea, and it is certainly something new in Scripture history to be told that the tribe inhabited its shores, when it is quite well known their inheritance was much further distant than even this, viz., north and west of the Sea of Galilee! (See Joshua xix. 32-39). I venture to suggest that between the words naphtha and napthali there is no connection whatever.—W. Briggs, Naphtha Distiller.

ANSWERS TO CORRESPONDENTS.

Pharmaceutist.—The substance is incorrectly described as *crystallized*. It is a scaled preparation, to which that term is inapplicable.

M. Meller.—The mineral kindly forwarded by our correspondent unfortunately contains no trace of thallium.

J. W.—Add shellac to the solution of india rubber in naphtha—that will confer on it the desired property.

A Constant Reader.—From what we can gather from your long and involved statement, you have not been careful enough to remove free mineral acids from your solution. This has prevented the complete precipitation of the phosphate, and some having consequently passed through, has complicated the subsequent reactions. You can easily test the correctness of your supposition by trying whether the precipitate which ought to be yttria contains phosphoric acid.

Beta.—The alkaline stearates are very slightly soluble in water.

J. Maxwell.—Use a bath of tin, to which bismuth has been added, until its melting point is reduced to about 439F, cover the bath with paraffin to prevent oxidation (powdered charcoal is of no use), and watch the indications of a thermometer immersed in the melted metal.

Laputa.—A mixture of tallow and black-lead is very effectual as a lubricant for gas taps and metallic rubbing surfaces in general.

J. Jellick.—Leaf-gold is generally about 1-250,000th of an inch in thickness.

C. Charles.—Water your gravel walk with a solution of sulphate of iron, this will in all probability improve its colour.

Effra.—Tannate of alumina may be prepared by grinding together in a mortar equal equivalents of freshly precipitated alumina and tannic acid. Wash and dry spontaneously.

B. Y. G.—The best thing to remove grease stains from silk is ether. If you use methylated the expense will be trifling.

E. J.—The patent was taken out on March 2, 1862. See the Chemical News for July 26, 1862, p. 55.

A Bee.—Permanganic acid is volatile, and may be distilled with caution, but the operation is not unattended with danger, as it sometimes explodes.

A Reader from the first.—The crystals have been examined and prove to be acetate of morphia. The quantity you name is a highly dangerous dose.

C. Porter.—The best enamel to use is boro-silicate of soda. This is not attacked by vinegar, salt, or other ingredients used in cooking. The silicate of lead, which is sometimes used as an enamel, gives up its lead to many liquids. It should therefore be carefully avoided in manufacturing utensils for the kitchen.

Artist.—Constant white is the trade name for sulphate of baryta.

Engineer suggests that some boiler explosions may be caused by the annular discharge of steam, sometimes attracting the safety valves to their seats, and thus obstructing the escape; in the same way that a strong blast of air discharged from a pipe within a short distance of a flat surface, will not repel, but will attract any object placed in the intervening space.

F. H.—The vapour of bisulphide of carbon, constantly inhaled, is said to produce temporary insanity amongst workmen

B.R., U.S.A.—The metal erbium certainly seems to be an elementary body, and not a mixture of yttrium and didymium. Few chemists have worked on the subject, owing to the rarity of the minerals containing these earthy bases.

Quæsitor asks where stoneware vessels capable of holding 1,000 gallons can be had of the best quality, capable of resisting the action of strong commercial hydrochloric acid at 200 to 300F. He cannot do better than apply to Messrs. Cliff and Co.

Books Received, "Report on the Sanitary Condition of the City of London," by H. Letheby, M.B.

"On the Laws of Connection between the Conditions of a Chemical Change and its Amount," by A. Vernon Harcourt, M.A., and W. Esson, M.A.

"Abridgments of Specifications relating to Plating or Coating Metals with Metals."

Ditto "Relative to Photography."

"On some Points of Chemical Nomenclature," by A. Vernon Harcourt, M A.

"On the Practice of Employing certain Substitutes for the Genuine Ingredients in some Articles of Daily Food," by a Lady.

Communications have been received from J. Carter Bell (with enclosure); William Skey, ditto; H. S. Bethell; F. Maxwell Lyte; the Abbé Moigno; Professor Heaton; F. Tomlinson; J. Heath (with enclosure); W. Edmonds; Rev. B. W. Gibsone (with enclosure); H. Watts, F.R.S; A. P. Hurlstone; F. Foord (with enclosure); G. Marrison; Tasmania (with enclosure); F. Day; C. Tomlinson, F.R.S.; F. Avelino Aramayo; C. R. A. Wright; J. Sutherland (with enclosure); James Carruthers; James Owen; May and Baker; Professor G. C. Foster; J. Spiller; J. W. Slatter (with enclosure); Messrs. Townsend and Adams, New York (with enclosure); W. W. Renny; J. Henderson, jur.; P. Anderson; J. A. Lake Gloag; E. C. C. Stanford; John Dods (with enclosure); R. F. Bright; E. Brembridge; Benjamin Wheeler; G. W. Wigner; John Lundy (with enclosure); Townsend and Adams, New York; Dr. E. Rohrig (with enclosure); C. R. A. Wright, B. Sc.; Radcliffe and Layton (with enclosure); F. Maxwell Lyte; A. Bird; Dr. Lunge; J. C. Bell; Dr. E. A. Cook; F. Vikars; J. Dodd (with enclosure); M. Farquarson; S. E. Wood (with enclosure); L. Hughes; Dr. Anderson (with enclosure); J. Taylor; G. A. Keyworth; E. Scott; W. Bailey; S. Robertson (with enclosure); Professor Church, M. A.; C. Tomlinson, F.R.S.; Harold Thompson; A. Jones; E. Maxwell, United States; Professor T. Hayter Lewis; H. Letheby, M.B.; A. Vernon Harcourt, M.A. (with enclosure); W. Esson, M.A.; W. H. Walenn (with enclosures); F. J. R. Carulla; Dr. Roscoe, F.R.S.; J. Baxendell; J. Barclay; F. O. Ward; J. A. R. Newlands; May and Baker (with enclosures); the Abbé Moigno; A. Herrmann and Co.; J. Foord (with enclosure); J. Landauer (with enclosure); A. T. Coimbro; W. A. Townsend and Adams; T. A. Readwin (with enclosure); E. F. Russel; J. Smythe (with enclosure); J. Clark; Olinthus Barry; G. Farrer Rodwell: J. Davies (with enclosure); C. Stride; R. C. C. Lippincott; J. Sutherland; P. Clavel (with enclosure); Mottershead and Co. (with enclosure); Dr. S. Macadam (with enclosure); R. Graesser (with enclosure); J. Collins (with enclosure): E. Beanes; F. Swan; M. A. Baines (with enclosure); H. W. Kearns, B Sc.; G. Davies; R. H. Clark (with enclosure); H. Hudleson; W. A. Wood; G. Croome; A. Bird (with enclosure); L. Dourrieu; E. Elliot; J. Ayling; A. Hofmann; E. O. Jones; John M. Swinstead (with enclosure); J. Schad (with enclosure); C. Littleton; S. Highley; C. R. C. Tichborne (with enclosure); F. J. R. Carulla (with enclosure); S. W. Moore (with enclosure); E. James; P. J. Butler; D. Forbes, F.R.S. (with enclosure); J. Landauer; Arthur C. Bowdler; H. R. Marsden (with enclosure); L. Power (with enclosure); C. Crump; R. C. C. Lippincott; J. Sleesor (with enclosure); Dr. C. P. Bahin; W. Briggs; James Smith; John R. Irvine; Peter Squire.

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ANALYSIS OF BLISTER STEEL.

BY DAVID FORBES, F.R.S., ETC.

Very few analyses of blister steel are to be met with in any of the treatises on metallurgy, and even in Dr. Percy's recent work on the metallurgy of iron and steel, no analysis is to be found of this truly national product. Under these circumstances therefore, the following analysis of blister steel converted in Sheffield from bar-iron of Swedish manufacture, may be considered as worthy of being recorded.

In making this analysis the portion selected for examination was obtained in a sufficiently divided state by chipping off the bar with a cold chisel, since it was found that no reliance could be placed in filings, which even if produced by the best files were always largely contaminated by the *debris* of the file itself; the determination of the constituents was made as follows:—

Determination of the total amount of Carbon.—77.91 grains of the steel in the form of such chippings, were allowed to remain (about ten days) in a cold solution of 200 grains pure chloride of copper, until no undissolved steel remained behind; the residue was then well washed by decantation, dried, mixed with 100 grains of pure oxide of copper, and burnt in a current of purified dry oxygen gas, at a heat sufficient to soften the Bohemian glass tube. The carbonic acid collected as usual in a potash apparatus amounted to 2·08 grains, or equivalent to 0·729 per cent. carbon in the steel.

Determination of the Sulphur.—107·58 grains were placed in a flask provided with a safety-funnel, and digested (for twenty-four hours) in the cold with strong hydrochloric acid; the gas evolved was passed through a solution of pure chloride of zinc supersaturated with ammonia; the iron being all dissolved, the zinc solution was boiled with nitric acid in some excess, nearly neutralized by ammonia to prevent any solvent action from excess of acid, and precipitated by a solution of pure chloride of barium,—0·04 grains sulphate of barytes were obtained, equivalent to 0·005 per cent. sulphur in the steel.

Determination of the Silicon and uncombined Carbon.—The solution from above was evaporated in a water-bath to dryness, re-dissolved in water with some hydrochloric acid, and filtered from the insoluble silica and graphite; these latter were washed off the filter into a silver basin, in which they were boiled with potash, which dissolved out the silica, leaving the graphite, which was collected on a filter, washed, dried, carefully scraped off filter, and after drying at 250° F., weighed 0·11 grains, equivalent to 0·102 per cent. uncombined or graphitic carbon. The potash solution of silica was supersaturated with hydrochloric acid, evaporated to dryness, and the residue treated with water rendered acid by hydrochloric acid. The silica was then filtered off and determined as usual, being 0·06 grains, or equivalent to 0·0304 per cent. silicon in the steel.

Determination of the Manganese.—The acid filtrate, after separating the graphite and silica by filtration, was now nearly neutralized by ammonia, and then treated with carbonate of barytes in excess, filtered, and the filtrate precipitated by sulphide of ammonium, the sulphide of manganese mixed with some sulphate of barytes was then treated with weak sulphuric acid, filtered, and the manganese precipitated by carbonate of soda as usual, affording 0·18 grains manganosomanganic oxide, or equivalent to 0·12 per cent. manganese in the steel.

Search for Phosphorus.—52·75 grains of the steel treated precisely according to Abel's directions (Chemical News, vol. vi. p. 133, *Eng. Ed.*), afforded no trace of ammonic phosphate of magnesia. 73·28 grains examined by Spiller's process (Chemical News, vol. xiii. p. 170, *Eng. Ed.*), gave the same negative result; and, lastly, 48·07 grains tested by Eggertz's method by molybdate of ammonia, did not afford any trace of phosphorus.

Determination of Iron.—The amount of iron present was estimated as loss. The percentage results will be as follows:—

Carbon combined	0·627
——— graphitic	0·102
Silicon	0·030
Phosphorus	0·000
Sulphur	0·005
Manganese	0·120
Iron	99·116
	100·000

ON THE USE OF POTASSIC CHLORATE IN QUALITATIVE BLOWPIPE EXPERIMENTS.

BY JOHN LANDAUER.

I communicated in No. 399 of the Chemical News (*Amer. Reprint, Sept.* 1867, *p.* 159), as the result of many experiments, a method of detecting manganese by means of potassic chlorate and the blowpipe.

I have continued these experiments, and convinced myself that this salt may be used with advantage for the detection of many oxides by means of the blowpipe, inasmuch as it leaves nothing to be desired as regards readiness and delicacy of execution. The delicacy of the test especially is greatly augmented by the fact, that the originally white salt assumes the respective colours.

The action of the potassic chlorate is, of course, that of energetic oxidation, caused by the evolution of oxygen at a high temperature. I find it most convenient to employ glass tubes, not too thick, in dimensions of about 15 centimetres long by 5 millimetres in width, and closed at one end; in these is introduced a small quantity of the chlorate, together with the substance to be examined; heat is applied gradually, at last with the help of the blowpipe, until no more oxygen is given off. The reaction is then completed, and the colour of the flux is examined.

I give in the following table some of the more delicate reactions, reserving a more complete investigation, the results of which will shortly be published in this journal:—

Iron	Flesh colour
Lead	Yellowish brown
Copper	Black or greyish black
Cobalt	Blue (in certain cases black)
Manganese	Purple
Nickel	Black (Ni_2O_3.)

ON THE

UTILISATION OF THE WASTE PRODUCTS OF THE MANUFACTURE OF COAL GAS.

BY DR. LETHEBY.

(Continued from page 170, Amer. Reprint Chem. News.)

Carbolic Acid Colours.

Four or five dyes have already been produced from this compound, namely, *rosolic acid* or *aurine*, *peonine* or *coralline*, *azuline*, and *picric acid.*

Rosolic acid is contained in coal-tar, as was first demonstrated by Runge in 1834, who extracted it from the dark red-brown residual product of carbolic acid by means of spirit; and on treating the solution with caustic lime, he separated a brown compound (brunolate of lime), and obtained a red solution (rosolate of lime), from which he precipitated the rosolic acid as a dark red powder by the aid of acetic acid. Other observers, as M. Tschelnitz in 1857, and Dr. Hugo Müller still later, noticed that the common carbolate of lime of commerce became red on exposure to the air, and that this was due to the formation of rosolate and brunolate of lime; but we are indebted to Dr. Angus Smith, and more recently to M. Jourdan, for an explanation of the changes which thus take place in carbolate of lime, and for suggestions for a process for making the dye on a commercial scale. They found that when the vapour of carbolic acid is passed over a hot mixture of soda and peroxide of manganese, or peroxide of mercury, oxygen is absorbed and rosolic acid produced, thus:

$$\underbrace{2C_{12}H_6O_2}_{\text{Carbolic acid.}}+O_2=\underbrace{C_{24}H_{12}O_6}_{\text{Rosolic acid.}}$$

The residue yields to water a rich solution of rosolate of soda, from which the rosolic acid can be obtained by precipitating by means of acetic acid.

The production of acid commercially has been accomplished and patented by Messrs. Guinon, Marnas, and Bonnet. They mix together about 23 parts, by weight, of carbolic acid, 10 to 20 of oxalic acid, and from 7 to 14 of commercial sulphuric acid, and heat them for three hours or until the desired colour is obtained. The product is well washed with water to remove the excess of acid, and the residue, which is impure rosolic acid (*aurine*), is a soft pitchy material with a green shade of cantharides; but as the acid is insoluble in water and cannot well be fixed upon fabrics, the patentees have converted it into a new compound, named *peonine*, by incorporating nitrogen with it.

Peonine or *coralline* is produced by heating 1 part of the rosolic acid with 2 parts of ammonia of commerce, for three hours, in a closed metallic vessel at a temperature of 270° Fahr. The product is a thick liquid of considerable tinctorial power, and which gives with acids a deep red insoluble or fast colour, which may be so applied to silk, wool, and other textile fabrics.

Azuline, as I have already stated, is a blue colour, produced by heating 5 parts of peonine with 6 or 8 of aniline, and keeping them at nearly the boiling-point for several hours.

Picric acid, or *carbazotic acid*, or *trinitrophenic acid*, is obtained by oxidizing carbolic acid with nitric acid. It was formerly procured by a like treatment of indigo and the yellow resin (*Xanthorrhea hastilis*) of Australia, and also by the action of nitric acid upon the coal naphtha, which distils between 300° and 400° Fahr.

When carbolic acid is cautiously dropped into strong nitric acid it is attacked with great violence and with a hissing noise, as you may observe; and, according to the strength of the acid, there are produced one or more of the following substitution compounds:—

Carbolic acid	$C_{12}H_6O_2$
Mononitrophenic acid	$C_{12}H_5NO_4O_2$
Binitrophenic acid	$C_{12}H_4(NO_4)_2O_2$
Trinitrophenic acid	$C_{12}H_3(NO_4)_3O_2$

If the acid be strong enough, the last compound is alone produced, and when the mixture cools it deposits crystals of picric acid. These are purified by dissolving them in water, neutralising the solution with carbonate of soda, evaporating, and crystallizing. The crystals of the soda salt yield, when they are decomposed with dilute sulphuric acid, fine yellow, pearly looking crystals, or plates of picric acid. They are soluble in from 80 to 90 parts of cold water, and they possess considerable tinctorial power—a grain of acid in 300,000 grains of water will give a moderate shade of yellow to 1000 grains of silk. The colour is best applied with a mordant of alum and cream of tartar; cotton fabrics do not retain the colour, and hence it becomes a test for such tissues when mixed with wool or silk. The solution is very bitter, and, as it is not a poisonous compound, it has been thought that it might be used instead of hops for beer. It forms yellow salts with the alkalies, and with metallic oxides, and most of them are highly fulminating or explosive when heated.

If picric acid is submitted to the action of reducing agents it produces red colours of great beauty; thus *picramic acid* is formed when the acid is reduced by means of a hot solution of protosulphate of iron (Wöhler), or by the aid of sulphuretted hydrogen or sulphide of ammonium (Girard).

$$\underbrace{C_{12}H_3(NO_4)_3O_2}_{\text{Picric acid.}}+6HS=\underbrace{C_{12}H_5(NO_4)_2NO_2}_{\text{Picramic acid.}}+4HO+S_6$$

The acid thus obtained is in the form of brilliant ruby-red crystals, which are soluble in alcohol and ether, and slightly soluble in water.

Isopurpuric acid is another red product of picric acid. It is procured from it by the process of M. Hlasiwetz, which consists in dissolving 2 parts of cyanide of potassium in 4 of water, and when it is heated to a temperature of 140° Fahr., adding little by little a solution of 1 part of picric acid in 9 of water. The liquid evolves ammonia and prussic acid, and, on cooling, deposits an abundant crop of crystals. These are washed with a little cold water, and then dissolved in boiling water to which a little carbonate of soda has been added; as the solution cools, it yields tolerably pure crystals of isopurpurate of potash. They have a red-brown colour by transmitted light, and a green metallic by reflected. By substituting ammonia for potash, as by dissolving the crystals in boiling water and adding sal-ammoniac, there are formed, as the solution cools, beautiful red crystals of isopurpurate of ammonia, which is isomeric with the brilliant red dye called *murexide*, and which, but for the cheaper forms of aniline colours, would have been an important dye; for it gives to silk and wool, when mordanted with corrosive sublimate, a magnificent purple rivalling the purple of Tyre; and with a mordant of zinc it produces a brilliant yellow. The colours are very fast, but they will not resist the action of the sulphurous acid so constantly found in the atmosphere of towns.

We know but little of the homologues of carbolic acid—namely, cresylic acid ($C_{14}H_8O_2$)—and the higher members of the series, which may, perhaps, be capable of yielding corresponding coloured compounds.

Naphthaline Colours

have not yet been successfully produced, although many attempts have been made to utilize it in this way; indeed, as far back as 1858, Strecker drew attention to the similitude of chloroxynaphthalic acid and the red colouring constituent of madder (alizarine), there being required only the substitution of hydrogen for the chlorine to change it into madder red; and in 1861, M. Z. Roussin announced that he had actually converted naphthalin into alizarine. His process was first to act on naphthaline with nitric acid, and so change it into binitronaphthaline, thus:—

$$\underbrace{C_{20}H_8}_{\text{Naphthaline.}}+2HNO_6=\underbrace{C_{20}H_6(NO_4)_2}_{\text{Binitronaphthaline.}}+4HO$$

This is a crystalline body, which he next dissolved, little by little, in concentrated sulphuric acid. The mixture was then heated to a temperature of 392° Fahr., and small portions of granulated zinc were cautiously added to it. After a time sulphurous acid began to be evolved, and the nitronaphthaline was slowly converted into a red colouring matter, which he thought was alizarine. The change appeared to be as follows:—

$$\underbrace{C_{20}H_6(NO_4)_2}_{\text{Binitronaphthaline.}}+H_8=\underbrace{C_{20}H_6O_6}_{\text{Alizarine.}}+2NH_3+2HO$$

By diluting the mixture with 8 or 10 times its bulk of boiling water, and quickly filtering, the solution yielded as it cooled, brilliant red crystals. But they differ from alizarine in many essential particulars, especially in not giving the purple and chocolate tints, as alizarine does, with iron and alumina mordants.

Mr. Perkin has also devoted attention to this subject, but his labours have not been very successful.

Naphthalamine is a compound which bears the same relation to naphthaline that aniline does to benzole, and it is made by somewhat similar transformations. Messrs. Calvert, of the Tower Chemical Works, have produced it very largely, in the hope that, by oxidation in the same way as aniline and toluidine are oxidised, colours might be obtained. In this manner Mr. Brunner produced in Mr. Calvert's laboratory a very fine purple, by heating it with arsenic acid. M. Du Wildes obtained a like result with the nitrates of mercury; and M. Roussin has shown how fabrics may be dyed of a red colour by acting on muriate of naphthalamine with nitrate of potash, and how a violet-red tint may be obtained by heating a mixture of naphthalamine and dry bichloride of mercury in a sealed tube, at a temperature of 356°, for many hours; and by heating a mixture of muriate of naphthalamine and protochloride of tin to a temperature of 472° Fahr. The purple-red colour is in both cases insoluble in water, but soluble in alcohol, and may be thus used as a dye. Messrs. Guinon, Marnas, and Bonnet have also proposed to use it in the place of aniline for the production of a blue colour; but I am not aware that any of these processes have been put into actual practice.

And now in conclusion, as I have been compelled, for want of time, to deal very briefly and generally with this subject, I will merely state that those who are anxious to pursue the matter further will find many memoirs on the subject, to which they may refer with advantage. In this country there have been published the valuable report of Dr. Hofmann, at page 119 of the chemical section of the "Reports of Juries" on the International Exhibition of 1862, and the "Lectures by Dr. Calvert on Coal-Tar Colours, in Relation to Dyeing and Calico Printing;" and on the Continent the following have been published:—

1. "Examen des Matières Colorantes Artificielles dérivées du Goudron de Houille." Par E. Kopp. 1861.
2. "Matières Colorantes dérivées du Goudron de Houille." Par Ad. Wurtz. 1862.
3. "Manufacture and Properties of Aniline Colours, and the Bodies used in their Preparation." By MM. Depouilly Brothers. 1866. Chemical News, vol. xiv., pp. 77, 89, 157, *Eng. Ed.*
4. "Technologie des Anilins." "Handbuch der Fabrikation des Anilins, und der von ihm derivirten Farben." M. Reimann. 1866.

In addition to which there are numerous papers on the subject in the scientific journals of the last six years, several of which have appeared, either in full or in abstract, in the *Journal of Gas Lighting.*

ON THE ABSORPTION OF GASES BY CHARCOAL.

The following letter from Dr. R. Angus Smith, F.R.S., to Dr. J. P. Joule, F.R.S., has been forwarded to us:—

"My dear Joule:—You asked me about my experiments on the absorption of gases by charcoal. I certainly seem to delay them, but I have little spare time. In 1848 I illustrated the oxidising power of porous bodies, referring chiefly to sand. In 1862, when speaking of the absorption of gases by charcoal, I ventured to say that the physical and chemical action could not be separated. I have been anxious to obtain more direct evidence.

"I had worked a good deal with the mixed gases, but lately thought it better to return to the simple, although unwilling to question results got by others. Five of these gases have been tried, and they are found to be absorbed by charcoal in whole volumes, and not in fractions of a volume, hydrogen being taken as one. In three cases hydrogen, oxygen, and carbonic acid, the numbers are 1, 7·99 and 22·05, extremely exact volumes, with a relation the same as our ordinary atomic weights. Saussure's numbers treated thus give 5·3 and 20, that for nitrogen being 4·2.

"It is only by taking the average of many experiments that these results have been obtained, but, in doing so, every one has been added without selection. The numbers from which the averages are obtained diverge so much that I suppose others have not thought of obtaining anything definite. This is caused by the difficulty of finding perfectly uniform charcoal.

"I have not found the other numbers to be the same as the equivalents, although still whole. Equivalents promise here to enlarge their bounds. I cannot believe that these numbers can be the results of any accident. They must be distinguished from chemical equivalents by weight.—I am, yours, &c.

"R. Angus Smith.

"Manchester, June 17, 1867."

ON THE PRACTICAL LOSSES OF SULPHUR, ETC., IN THE VITRIOL MANUFACTURE.

BY CHARLES R. A. WRIGHT, B.SC.

The following table, calculated by interpolation from Bineau's results (*Ann. Chem. et Physique*, iii. xxiv. 341), may be useful to manufacturers and others in calculating the value of sulphuric acid of a given density according to Twaddell's hydrometer, at a given temperature.

At 15 deg. Centigr.		Diminution in density for 1 deg. C. above 15 deg. C.	Percentage of SO_4H_2.	Difference for 1 deg. T.	Percentage of SO_3.	Difference for 1 deg. T.
Deg. T.	Sp. Gr.					
68·52	1·8426	0·192	100·00	..	81·63	..
168	1·840	0·191	97·00	..	79·18	..
				3·38		2·75
167	1·835	0·190	93·62	..	76·43	
				1·87		1·53
166	1·830	0·189	91·75		74·90	
				1·15		0·94
165	1·825	0·188	90·60		73·96	
				0·94		0·77
164	1·820	0·187	89·66		73·19	
				0·76		0·62
162	1·810	0·186	88·14		71·94	
				0·70		0.57
160	1.800	0·185	86·75		70·81	
				0·63		0·51
157	1·785	0·184	84·89		69·29	
				0·53		0·43
154	1·770	0·183	83·30		67·99	
				0·46		0·37
150	1·750	0·182	81·45		66·49	
				0·44		0·36
145	1·725	0·181	79·25		64·69	
				0·42		0·34
140	1·700	0·180	77·16		62·98	
				0·43		0·35
135	1·675	0·179	75·00		61·22	
				0·42		0·34
130	1·650	0·178	72·92		59·52	
				0·42		0·34
125	1·625	0·176	70·83		57·82	
				0·43		0·35
120	1·600	0·174	68·66		56·06	
				0·42		0·34
115	1·575	0·172	66·58		54·35	
				0·43		0·35
110	1·550	0·170	64·42		52·59	
				0·44		0·37
105	1·525	0·167	62·18		50·75	
				0·46		0·38
100	1·500	0·164	59·89		48·89	
				0·47		0·39
90	1·450	0·160	55·19		45·05	
				0·50		0·41
80	1·400	0·155	50·20		40·98	
				0·53		0·43
70	1·350	0·148	44·89		36·65	
				0·56		0·46
60	1·300	0·140	39·29		32·07	
				0·60		0·49
50	1·250	0·131	33·29		27·17	
				0·61		0·50
40	1·200	0·120	27·23		22·23	
				0·64		0·52
30	1·150	0·100	20·79		16·97	
				0·65		0·53
20	1·100	0·080	14·25		11·63	
				0·67		0·55
10	1·050	0·470	7·50		6·12	

The first column indicates the strength as given by Twaddell's hydrometer at a temperature of 15°C., and the second the corresponding sp. gr.

The third shows the fraction of a degree Twaddell to be added to the observed strength for each degree centigrade that the acid is above 15°C.; or to be subtracted for each degree below 15°C.

The fourth indicates the percentage of SO_4H_2, corresponding to the density at 15° given in the first and second columns; and the fifth, the differences between the numbers in the fourth, used for calculating the amount to be added on for fractions of a degree Twaddell.

The sixth and seventh columns indicate the quantities of SO_3 corresponding to those of SO_4H_2 in the fourth and fifth.

Thus, to find the percentage of SO_4H_2 present in acid in which a hydrometer marks 154°·5 T. at a temperature of 25°C.

154°·5 T. at 25°C. correspond to 154·5 + 10 × 0·183,
or 156°·3 T. at 15°C.
Acid at 154°T. at 15°C. contains 83·30 per cent. of SO_4H_2
Add on for 2°·3 T.: 2·3 × 0·53 = 1·22 " "

Percentage required .. 84·52

To find the percentage of SO_3, in acid of 121°·1 T. at 9° C.

121·1° T. at 9° C. correspond to 121·1 − 6 × 0·174, or 120·06 at 15° C.

Acid of 120° T. at 15° C. contains 56·06 per cent. of SO_3.

Add on or 0°06 : 0·06 × 0·35 = 0·02

Percentage required..........56·08

An acid of high specific gravity is more likely to contain lead sulphate, as the effect of temperature in altering the density is greater the stronger the acid, and as there is but little difference in density for a considerable difference in percentage with strong acid, it is evident that the amount of SO_3 or SO_4H_2, can only be approximately determined in strong acid by the aid of the hydrometer; taking also into consideration the fact that glass hydrometers, as usually sold, are rarely correct to within 0·5° T., and are frequently more erroneous, it may be pretty safely stated that the value of any acid of above 160° T cannot be estimated at all accurately by the hydrometer.

The amount of acid theoretically obtainable from any given ore is readily calculable by the following simple formulæ:—

From one part of sulphur ore containing x per cent of sulphur there is theoretically obtainable—

(1) of acid containing m per cent. of SO_4H_2

$$2{\cdot}5 \times \frac{x}{m} \text{ parts.}$$

(2) " " n " SO_3

$$3{\cdot}0625 \frac{x}{n} \text{ "}$$

Thus from a kilogramme of ore at 32 per cent. of sulphur there is theoretically obtainable of acid at 135° T. (containing therefore 75·00 per cent. of SO_4H_2)

$$2{\cdot}5 \times \frac{32}{75} \text{ kilogrs.} = 1{\cdot}067 \text{ kilogrs.}$$

and from a kilogramme of pure sulphur there is obtainable of acid at 154° T. (containing 68 per cent. of SO_3)

$$3{\cdot}0625 \times \frac{100}{68} \text{ kilogrs.} = 4{\cdot}504 \text{ kilogrs.}$$

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PHILOSOPHICAL CONCEPTIONS OF CHEMICAL PHENOMENA.

The "fundamental definition" of a chemical phenomenon lately advanced by Sir Benjamin Brodie, to the effect that "A chemical phenomenon is an operation on the unit of space, the result of which is a weight," has strongly directed the attention of chemists to the primary conceptions on which the philosophy of their science is based.

At such a juncture it cannot but be interesting to set in contrast with the above startling philosophical innovation, one of the most clear and powerful expositions extant of the received mode of viewing this abstruse question.

Such an exposition we find in the ninth chapter of the admirable "Introduction to Modern Chemistry" lately put forth by Professor Hofmann in collaboration with Mr. F. O Ward,—"whose well-known powers of lucid composition, and habits of philosophical thought, are traceable," as his illustrious colleague justly observed in his preface, "in every chapter of the work."

We shall the more readily lay these extracts before our readers, because, while specially apposite to the present tenour of chemical meditation, their appearance will redeem the promise made by us, in our first review of this book, to give it a second notice in our columns.

We therefore, without further preface or apology, proceed to lay the following extracts before our readers, merely remarking that we have here and there exercised our editorial privilege of excision and abridgment.

"Thus far," say the collaborating authors, "we have not quitted the domain of experience, of observation; to the *causes* of the remarkable phenomenon we have contemplated, our attention has not yet been turned. Yet the inquiry into the causes of observed phenomena is urged on us by one of the strongest instincts of our intellectual nature. That instinctive curiosity cannot, indeed, be fully satisfied. The first causes of phenomena lie beyond the limited scope of our perceptive and reasoning faculties. The conditions of their existence or production, and their relations of succession and similitude are, indeed, open to investigation; but their intimate nature and prime origin are for us inscrutable mysteries. We may, however, by the aid of imagination, form *hypotheses*, to connect the results of our experiments, and to guide the course of our inquiries. And, though merely speculative hypotheses, dissevered from experimental investigation, are to be deprecated as vain and sterile exercises of ingenuity, hypotheses based upon facts, assisting in their conception, and deriving probability from the number thereof which they connect and explain, besides (and above all) tending to suggest new experiments, deserve to rank among the most valuable aids to scientific research.

"Hypotheses are, of course, to be held provisionally, subject to modification and abandonment, in so far as they may from time to time prove inconsistent with the results of further experimental research. On the other hand, when hypotheses embrace and explain extensive ranges of phenomena, when experiment confirms the results they foreshadow, when successive discoveries raise them higher and higher in the scale of probability, they lose more and more their provisional character, and gradually assume the name and rank of *theories*, till at last they come to be embodied among the recognised doctrines of philosophy.

"The observed phenomena of combination in definite proportions by weight and volume, are susceptible of explanation by a theory in the highest degree probable and suggestive, which comes next in order for our consideration.

"In order to arrive at this theoretical conception, we must ask ourselves, what *is* matter? Of what parts is it composed? How are these constructed and held together? How comes the very same matter, water for example, to present itself sometimes in the solid form, as ice; sometimes in the liquid form, as the same ice when melted; sometimes in the gaseous form, as the same melted ice changed to dry steam by further heating? And, lastly, what happens to matter, what changes does it undergo, when its various elementary forms combine, as we have seen them, to produce bodies having properties wholly different from those of their constituents?

"Setting aside the more transcendental speculations of philosophers upon the nature of matter, let us here select for consideration those hypothetical conceptions of its structure which seem best adapted to connect and explain the results of modern research; and which, by enabling us to comprehend the phenomena we have already witnessed, may also assist us in shaping the course of our further experimental researches.

"Let us, for this purpose, consider the familiar body, *water*, into the nature of which our experiments have already given us some insight; and let us consider it in its three conditions, as ice, as fluid water, and as water-gas or dry steam. What is the first thing that strikes us in looking at them?

"The first thing that strikes us is, that ice, water, and steam manifest two sorts of activity;—one exerted by masses of sensible magnitude, acting through measurable distances of space; the other operating between particles, and through intervals of space, so minute as to be incommensurable.

"The attraction of mass for mass of matter, as manifested in the courses of the celestial bodies, in the movement of falling bodies, and in the pressure of bodies at rest upon the ground, exemplifies the first kind of activity. This is equally observable in the ice, in the water, and in the water gas; for these all possess *weight;* a sensible mass of either reciprocates attraction with the earth, through measurable distances of space.

"The Latin for mass is *moles;* and its modern diminutive, *molecula*, is employed to designate 'a little mass,' that is to say, a material particle of incommensurable minuteness; hence the reciprocal actions of minute particles through insensible intervals of space are distinguished as *molecular*. We may fairly therefore contradistinguish, by the epithet *molar*, the reciprocal actions of measurable masses through measurable intervals of space.

"The means of mechanical comminution at our disposal, our grinding-mills, mortars, and the like, do not carry us beyond the *molar* subdivision of matter. However finely we might grind up ice, for example, if we took care to keep the temperature below freezing point, we should still have masses each consisting of several molecules. For, our finest ice-powder would still consist of very small fragments of solid ice; and if, of this ice-dust, we took the smallest grain, we could, by applying heat, turn it into water, thus proving it to have *parts*, capable of separation, so as to be rendered moveable amongst each other. There is no instance of liquefaction resulting from the mechanical comminution of a solid body. Hence we take it as certain that the most impalpable product of mechanical pulverisation is still a cluster of molecules.

"We are thus enabled to distinguish in matter two kinds of divisibility, *molar* and *molecular;* the former being accomplished by *mechanical* means, and only resulting, even when pushed to its utmost attainable limits, in the production of a molecule-cluster or mass of sensible dimensions, which may be termed a *mole;* while the latter is accomplished by *physical* means (that is to say, by the aid of physical forces, such as heat), resulting in the disruption of the masses or *moles* into their incommensurably minute constituents *molecules.*

"The study of the reciprocal action of material *masses,* or *moles,* constitutes the science of *mechanics;* a science of the deepest interest, abounding in simple and admirable laws, with which, however, we are not at present concerned.

"Turning to the consideration of *molecular* activities, of those which are distinguished by the incommensurable minuteness of the particles of matter, and of the intervals of space, between and through which they take place; and looking once more at the samples of matter before us—at our ice, our water, and our water-gas or steam; we are again, as before, struck with a contrast between two diametrically opposite kinds of activity, one conspicuously manifested in the solid ice, and called *molecular cohesion,* the other especially manifested in the water-gas, and termed *molecular repulsion.* The former force gives to solid bodies their tenacity; to the latter, gaseous bodies owe their extreme tenuity, and the free mobility of their molecules amongst each other."

"In fluid bodies, here represented by our water, we observe these two forms of molecular activity balanced at an intermediate point. The molecules of fluids cohere with considerable force; as we perceive, when a rod is dipped into water, and a bunch of them taken out, sticking to each other, and also to the rod, in the form of a pendent water-drop; but this cohesion is exceedingly feeble as compared with that of the same molecules agglomerated in the solid form in a block of ice. Again, the molecules of fluids are moveable amongst each other; as we notice when water is shaken in a vessel, agitated with a rod, or poured into another glass; but their mobility is far inferior to that of the molecules of gas. In vain should we dip our rod into the gas to take up a drop of it; we should obtain no coherent bunch of gas-molecules, like the pendulous water-drop. And it is precisely to their superior molecular cohesion that fluids owe their inferior molecular mobility as compared with gases.

"This difference of comportment is not surprising when we reflect how much greater are the intervals which separate the molecules of a gas—of our water-gas, for example—than those which intervene between the molecules of the same body in the form of ice or of water."

"What is the nature of the intervals between the molecules of a gas?—are they empty space, or are they filled? and, if so, how, or with what are they filled? That they are not empty spaces we have very good reason to believe, on account of the powerful resilient property manifested by gases when forcibly compressed.

"But what is the nature of this elasticity or resilience—to what power or force is it due?

"Several phenomena point to *heat* as its cause. Heat is the agent by which ice is made to pass, through the fluid, into the gaseous form; and, with every increment of heat, the elastic power of the ice-derived gas augments.

"To the questions, therefore, what is a gas? and with what are the intervals between its molecules filled? succeeds the question, what is heat? This brings us face to face with one of the most ardently-mooted and deeply-interesting philosophical questions of the day. For some, heat is a species of thin ether, vibrating in the manner of light; for others, it is a pure force, having neither parts nor weight; for a third class of thinkers, of late years the majority, heat has no separate existence, but is merely a mode of motion, the result of the vibration of material molecules.

"It is no part of our present task to attempt the solution of this deep and difficult problem. We may content ourselves here with the conception that heat, whatever may be its intimate nature, so operates, when it becomes latent in a gas, as to surround each molecule with a sort of repellent atmosphere which tends to keep it apart from its fellows; and that these molecular force-spheres—or, to employ the Greek equivalent, *dynami-spheres,* more shortly, *dyna-spheres,* when mechanically compressed, counteract the pressure with exactly equal energy, and on the removal of the pressure, restore the gas (other things being equal) to the exact volume it previously possessed.

"It thus stands clearly demonstrated that, if equal volumes of the elementary gas, hydrogen, and of the compound gas, hydrochloric acid, be taken under any given pressure, and the pressure be doubled for each, each becomes reduced to half its former volume, and at the same time acquires double its former resilient force, or elasticity; which it exerts in counterbalancing the pressure from without.

"It stands equally proved that, if equal volumes of hydrogen, and of hydrochloric acid gas, taken at equal degrees of pressure and temperature, be exposed to equal increments or decrements of heat, they undergo equal degrees of expansion and contraction.

"It has been experimentally established as a law, that all true gases, simple as well as compound, comport themselves in sensibly the same manner under like variations of temperature and pressure; whence the inference fairly follows that their molecular structure is the same. Assuming, then, each gaseous molecule to be clothed or enveloped by a resilient dynasphere (as we have termed it), due, in some unknown way, to the influence of latent heat, experiment justifies us in inferring, from the identical comportment of all gases, when exposed to like variations of temperature and pressure, that they all contain, in equal volumes, an equal number of molecules so clothed; and that, as an obvious corollary, the diameter of these gas-molecules (including in that term as well the dynaspheres as their material nuclei) is, under like physical conditions, precisely the same for all gases. To express it more shortly, our unit-volume, or litre, whether of hydrogen, of hydrochloric acid, or of any other gas, simple or compound, is composed of mutually repellent dynaspheric molecules, equal (*omnibus paribus*) as to their *number,* and (consequently) as to their *size.*

"At this point of the inquiry we may advantageously resume the consideration of material divisibility, of which we have already studied two forms or grades, the *molar* and the *molecular;* the former consisting in the mechanical disruption of large masses into small ones, the smallest still possessing sensible magnitude; while the latter is the further disruption, by physical agents, such as heat, of moles or masses, whether large or small, into their constituent molecules; that is to say, into parts contradistinguished from the minutest moles by the fact that they (the said parts) possess no commensurable magnitude at all. In the particular sample of matter which we have selected for study, as being the most familiar of all compounds, we see molecular succeeding to mere molar division, when heat melts comminuted ice into water, and then raises water into invisible steam or gas, by clothing its molecules with the mutually repellent dynaspheres, each dynasphere 1689 times larger than its material nucleus.

"Infinitesimal as this subdivision of matter appears, —inexpressibly minute as we cannot but conceive the material particles to be that form the central nuclei of the dynaspheres of bodies so attenuated and rare as the invisible gases,—we yet know, by experimental proof, that a further comminution of matter is possible; and that, as the smallest mass or mole of any compound may be broken up into its constituent molecules, immeasurably smaller still, so the ultimate molecule itself, however small we may choose to conceive it, is nevertheless still a *compound*, consisting of at least two parts, which, by chemical agency, may be detached from each other, so as to resolve the compound into its elements.

"Here the divisibility of matter, so far as our experimental knowledge extends, reaches its final term. The *elementary* bodies are, as we remember, so called precisely because they resist every agency, mechanical, physical, and chemical, which we can bring to bear in the hope of dividing or decomposing them. We may imagine the two elementary particles which form the compound molecule of hydrochloric acid, for example, to be as small as we please. In this respect we may give the imagination free rein; we may conceive the particle of hydrogen, or of chlorine, to be divided and subdivided as many millions of times as we like,—or rather, until the imaginative power is baffled by sheer exhaustion in the endeavour to push this conception further. No experiment yet made tends to restrict the freest range of our mental faculties in this direction; their only limitation lies in their own finite scope, doubtless more or less extensive in different minds. But, when we have, each of us, thus reached the idea of the smallest elementary particle which it is within the power of the mind to picture, all experience stands opposed to our going still further, and presuming to declare the elementary particles capable of division *ad infinitum*. Not one experimental result can be adduced in support of such an assertion. At this point, therefore, the experimental philosopher arrests his inquiry. Beyond this limit he sees only the dream-land of metaphysical speculation—a region essentially sterile because shut out from cultivation by means of experiment, from which alone can spring the harvest of Truth in the proper sense of the word; having for its foundation natural facts; for its object the study of their relations; for its result the determination of their laws.

"To the metaphysical speculators, therefore, let us cheerfully resign the utter futile and fruitless discussion whether even elementary matter may not be infinitely divisible. It is enough for us to know that, at all events, *we* cannot infinitely divide it, but that, relatively to our powers and purposes, to the limits of our imagination as well as of our experience, the assertion of the infinite divisibility of the elements is one we are not justified in making.

"We thus arrive at the conception of indivisible particles as the ultimate constituents of elementary bodies, and these particles have received the appropriate name of *atoms* (from the Greek word τέμνω, *I cut*, *I divide*, with the privative *a* prefixed in token of negation).

"The addition of this final term completes and enables us to epitomise our view of the threefold divisibility of matter, molar, molecular, and atomic; the first (molar) being performed by *mechanical* means, and resulting, when pushed to its utmost limits, in masses or moles (clusters of molecules) characterised by their possession of sensible magnitude; the second (molecular) accomplished by the agency of the physical forces (heat, electricity, etc.), employed under special conditions for the purpose, and resulting in the production of the dynaspheric *molecules* of which we reasonably conceive compound bodies to consist; the third (atomic) being capable of accomplishment only by agencies, such, and so applied, as to produce *chemical* decomposition, breaking up the incommensurable molecule itself into its elementary particles, which (as just explained) are called *atoms*, because incapable of further disruption or comminution by any means at our disposal.

"This conception of the threefold divisibility of matter, molar, molecular, and atomic, being once clearly understood, and firmly grasped by the mind, we may usefully proceed, in the light which this theory supplies, to compare as to their structure compound with elementary gases. At first view we should be disposed, perhaps, to anticipate as probable, that, while the compound gases would be formed of divisible molecules or atom-clusters, the elementary gases would present no such complexity of structure, but consist merely of separate and indivisible elementary particles. But a little consideration will show us that this view is incompatible with the results of our preceding inquiry.

"Let us, to simplify our calculations, assign to the unknown number *n* of HCl molecules, existing in our bilitral volume of hydrochloric acid gas, some definite numerical value, say 1000.

"This being assumed as the number of molecules in 2 litres, the number in 1 litre is of course just half, or 500; and, as we recognise that equal volumes of all gases contain equal numbers of molecules, the litre of hydrogen and the litre of chlorine, which go to the formation of our 2 litres of hydrochloric acid gas, must likewise contain 500 molecules each.

"Now, as each molecule of hydrochloric acid contains 1 atom of hydrogen joined to 1 atom of chlorine, the 1000 molecules of hydrochloric acid must, of necessity, contain 1000 atoms of hydrogen joined to 1000 atoms of chlorine—the whole number of atoms present being therefore 2000.

"But we have just seen that one litre of hydrogen and one litre of chlorine contain, not 1000 molecules each of the respective bodies, but only 500.

"It follows clearly that 500 *molecules* of hydrogen and 500 *molecules* of chlorine have supplied respectively twice as many *atoms* of those constituent bodies; each contributing its 1000 atoms to the aggregate number of 2000 atoms, existing in the 1000 HCl molecules, contained in our 2 litres of hydrochloric acid gas.

"If 500 molecules of an elementary gas supply 1000 atoms it is plain that each molecule supplies 2 atoms; and thus we clearly perceive that the molecule of the compound gas under review, and the molecules of each of its elementary constituents, are all formed on the same type—that type being the first of our quadruple series, viz., the hydrochloric acid or *diatomic* type.

"This is a remarkable and striking, yet strictly logical deduction. It completes a chain of reasonings which, if correct, justify the conception that simple as well as compound gases are complex as to their molecular structure; and that this structure, for all the permanent elementary gases, is of the diatomic type."

THE CHEMISTRY OF METEORITES.

BY W. WARINGTON SMYTH, M.A., F.R.S.

M. Daubrée, already so distinguished for his researches on metamorphism, has recently published the results of his Synthetical Experiments on Meteorites, and has thus brought before us, from an entirely different point of view, an inquiry into the nature and origin of the silicated magnesian rocks and minerals.

M. Daubrée first describes his experiments on *the imitation of the meteoric irons.* The most characteristic feature of these masses is the crystalline pattern (Widmanstätten's figures) which is brought to view on a polished surface by the action of an acid. Simple fusion of the meteorite of Caille (Var) in a *brasque* of alumina (to avoid the contact of carbon, which would have combined with the iron), was insufficient to reproduce the appearance, although the resulting substance was certainly crystalline. Further experiments, in which soft iron was associated with some of the other substances that commonly accompany meteoric iron, such as nickel and protosulphide of iron and silicon, yielded a highly crystalline result, but not yet of the true character. If, however, to the soft iron was added phosphide of iron, in the proportion of from two to five or ten per cent., and, still better, if there was introduced at the same time nickel, and if a mass of as much as two kilogrammes in weight was operated on, there appeared, when the cooled lump was polished and etched, in the midst of dendritic patterns of great regularity, lines of a brilliant material dispersed in a reticulated form.

A third mode of attempting the imitation was that of melting down certain terrestrial rock-substances, as peridote, lherzolite[*], hypersthene, basalts, and melaphyres. By this means specimens of iron were obtained which, both in composition and structure, bore strong resemblance to many of the siderolites. Especially was this notable in the metal obtained from the lherzolite of Prades (Eastern Pyrenees). These artificial irons were then found, like the natural meteoric ones, to contain nickel, chromium, and phosphide of iron, the latter in long needles recalling the appearance of the natural patterns.

Imitation of the Meteoric Stones.—Contrary to what might have been expected from the appearance of the black vitrified crust on the surface, the substance produced by the melting down of meteorites obtained from above thirty different falls, was in every case highly crystalline. Those of the common type present a group of metallic granules, disseminated in a stony mixture of peridote ($\dot{M}g_2\ddot{S}i$) and enstatite ($\dot{M}g\ddot{S}i$), the former generally on the surface as a thin crystalline pellicle, the latter in the interior as long acicular crystals. A notable contrast was yielded by the aluminous meteorites, such as those of Juvinas, Jonzac, and Stannern, which produced, instead of crystalline, a vitreous mass.

But perhaps the more remarkable results were those obtained synthetically by melting down pieces of rock characterised by the minerals peridote and enstatite. For this purpose peridote (olivine), from the basalt of Langeac (Haute Loire), and lherzolite, from Vicdessos and Prades, were fused in earthern crucibles. They melted easily and yielded crystalline substances, the latter especially closely resembling the original rock. The proportion of enstatite (the bisilicate of magnesia) was found to be increased by the addition of silica.

When similar mineral substances were melted in presence of a reducing agent, the iron (which in the other case remained combined in the silicate) segregated itself in grains of various sizes, separable by the magnet. Thus a perfect analogy was established between the above rocks and the meteorites, as well in their stony minerals as in the iron, which always contained nickel.

Furthermore, some remarkable characters in the structure of the stony meteorites were found to have been imitated, especially the delicate parallel lines attributable to cleavage, which are visible when a thin slice is examined under the microscope, and the globular structure where the little spherules are sometimes smooth at the surface, at others drusy, or roughened with the points of minute projecting crystals, like the meteorite of Sigena, November 17, 1773.

When hydrogen was employed as the reducing agent, the results were very similar, and the reaction would take place at a temperature not exceeding red heat.

Again, another method of imitation, the reverse of the foregoing, was by oxidation. From silicide of iron, heated in a *brasque* of magnesia by the gas blowpipe, a substance was obtained extremely similar to the common type of meteorite. The iron was separated partly as native iron, partly as a silicate, forming peridote, some of it in the crystallised state. Further details of resemblance were attained by heating a mixture of silica, magnesia, and nickeliferous iron, phosphide and sulphide of iron. The stony gangue of the melted product was found to be free from the latter three substances; and instead of the simple phosphide introduced in the experiment, there was observable the triple phosphide of iron, nickel, and magnesium, first noticed by Berzelius in meteoric irons.

The preceding experiments suggest some important deductions on the condition of the planetary matter from which the meteorites have been diverted to our own globe. M. Daubrée makes no attempt to enter the lists with Von Haidinger[*], Baron Reichenbach, Prof. Lawrence Smith, and others, on the questions attending the entry of these bodies into our atmosphere, and the circumstances of their fall; but, considering that their surface alone is modified by these conditions, he infers that their interior mass remains the same as when it was wandering in space, and may to a great extent be taken as a sample of the material of the planetary bodies of which they are the fragments.

Seeing how nearly the composition and structure of the meteorites are reproduced by the two methods of

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[*] Lherzolite (so called from Lherz, in the Pyrenees) is a rock composed of peridote, enstatite, and pyroxene (augite).

[*] See Haidinger, *Phil. Mag.* November and December, 1861.

experiment, M. Daubrée refers by their aid to the original mode of formation of the bodies from which these meteorites come.

If they were produced from silicated minerals by reduction, in which carbon was the reducing agent, it may be objected that the iron could scarcely have remained in the metallic state; and if hydrogen be supposed to have been the reducing agent, water ought to have been formed at the surface, whence it appears more simple and reasonable to recur to the idea of an oxidising process. Allow that silicon and the metals existed at one time in the meteorites, not combined with oxygen as they now mostly are, and this by reason either of too high a temperature to allow them to remain in combination, or of too great a separation of their particles, then, as soon as, by their cooling down or by their condensation, the oxygen was able to act upon the other elements, it would at once combine freely with those for which it had most affinity, and if not sufficient in quantity to oxidise the whole, or not enabled to act long enough, would leave a metallic residue. In fact there would be produced the silicate of magnesia and iron, peridote or olivine, and granular portions of nickeliferous iron and of sulphides and phosphides of iron. These views, whilst applicable to a large proportion of the meteoric bodies, would require modifications for those rarer varieties which consist essentially of pyroxene and anorthite. Whilst the magnesian silicates crystallise so readily after simple fusion, these latter substances would only melt to vitreous and amorphous masses, and in order to become crystalline would have needed the presence of water.—*Address to the Geological Society. Anniversary meeting*, 1867.

ON THE PRACTICAL LOSSES IN THE BLEACHING-POWDER MANUFACTURE.

BY C. R. A. WRIGHT, B.SC., F.C.S.

The difference between the amounts of bleaching powder of a given strength obtainable theoretically and practically from a given quantity of manganese ore, depend mainly on three circumstances, viz:—

1. Incomplete decomposition of all MnO_2 used.
2. Loss of chlorine by leakage from the generators, conducting pipes, and powder-chambers, and non-absorption by the slacked lime of all the chlorine supplied to it.
3. Deterioration of the powder made, either by loss of hypochlorous acid from the action of the atmospheric CO_2, or by conversion of hypochlorite into chlorate and chloride, or other compounds deficient in bleaching-power.

In practical working, it is very difficult to obtain a good estimate of the several amounts of loss experienced from these three causes; the total loss, however, is readily calculable when the weights of manganese ore and bleaching-powder used and made, and the average percentage of MnO_2, and available Cl contained therein, are respectively known. Thus, taking 55 and 35·5 as the respective equivalents of manganese and chlorine, 100 parts of manganese ore containing M per cent. of "available binoxide" should yield $\frac{71}{87} \times$ M parts of chlorine, and consequently should theoretically give $81{\cdot}61 \times \frac{m}{n}$ parts of bleaching-powder containing n per cent. of "available chlorine."

The following results were obtained as the average losses, in different periods extending over several months each, in a works manufacturing upwards of 70 tons of bleaching-powder weekly.

Average per cent. of available chlorine in the powder packed in casks ready for sale.	Average per cent. of available MnO_2 in the manganese ore used.	Practical yield: the theoretical being taken as 100.
35·00	64·00	76·8
35·35	66·50	75·7
35·19	66·70	72·8

On the whole, therefore, the total average loss is just 25 per cent. of the theoretical yield.

These results were obtained by the "Lancashire" mode of working; *i. e.* where the chlorine generators, or stills, are formed of flags from 4 to 7 inches thick, jointed together and made tight by a composition of fire-clay and tar, known technically as "Barytes." The average amount of powder manufactured per square foot of surface on the floor of the chambers was 13·5 lbs. weekly; while the ratio of the cubic contents of the stills to that of the chambers was about 8·1 to 100. Out of a 100 parts of chlorine contained in the salt decomposed, upwards of 80 were obtained as yellow muratic acid of 25° Twaddell (25 per cent. of HCl), and 15 in the shape of bleaching-powder.

It was found that the average percentage of chlorine found in samples taken from the floor of the chambers immediately they were opened was about 1 per cent. more than the average in the same powder when packed in casks ready for sale, and, contrary to expectation, that this difference was almost exactly the same in hot weather as in cold; the numbers obtained being:—

	Summer months.	Winter months.
Average of samples from chambers........	35·09	36·71
" " casks	34·98	35·71
Difference	1·11	1·00

This difference therefore amounts to $\frac{1{\cdot}05}{36{\cdot}4}$ or three parts in 100 on the average for a whole year, and probably represents the atmospheric action on the powder during the processes of packing in casks, etc. Bleaching-powder manufactured in hot weather was frequently found to contain perceptible quantities of chlorate; samples kept in a warm place in sealed bottles were found at the end of some weeks to contain several per cents of chlorate: it was, however, found impracticable to determine the average amount of chlorate found in the process of manufacture.

The physical character of the sifted and slacked lime employed was found to have a great influence on the rapidity of absorption of the chlorine, and on the quality of the powder produced. It was noticed generally that those quicklimes technically called "Fat," (*i. e.*, which slack, rapidly falling to a fine flowery powder,) gave always the most satisfactory results; whilst poorer limes which did not slack so quickly, and yielded a gritty powder after slacking, absorbed chlorine much less rapidly, and gave a bleaching powder deteriorating much more rapidly on keeping. Samples of these two kinds of bleaching powder kept in a warm place under the same conditions acted thus: less chlorate was found in the first kind (Fat lime), and no gas was evolved; more chlorate was found in the second kind, and frequently gases were generated, rupturing the sealed vessel containing the same. On making careful analyses of the limes from which these powders were made, scarcely the slightest chemical difference was detectable, all containing but a few tenths per cent. of combined silica, and scarcely any other impurity; physically, however, the former kind attracted moisture from the air very much more rapidly than the other.

By way of comparison with the foregoing numbers, the following calculations are given, based on data given in "Richardson and Watts's Technological Dictionary," vol. i. part. iii. p. 379:—

(1.) 26 cwt. of 64 per cent. manganese yield 24—26 cwt. of powder of 35—38 per cent. of chlorine; averaging, therefore, 25 cwt. at 36·5 per cent. Hence the practical average yield is 61·2 per cent. of the theoretical amount.

(2.) 2240 parts of salt give 2753 of acid of 28 per cent. HCl, and with 448 parts of manganese ore at 60 per cent., yield 416 of bleaching powder at 39 per cent. Assuming the salt to contain, as is probable, 93 per cent. of NaCl, out of 100 parts of chlorine contained in the salt, 59·3 are obtained as strong acid, and 12·8 as bleaching powder; the practical yield of powder from manganese being 74·0 per cent. of the theoretical quantity.

Chemical Laboratory, St. Thomas's Hospital.

ON SOME USEFUL APPLICATIONS OF CHLORIDE OF CALCIUM.

BY J. HARGREAVES.

THE utilisation of waste products has within the present century become an object of great importance, and the results such, for instance, as obtaining ammonia, benzol, aniline and its derivatives, etc., from coal-tar; garancin from madder waste, acetic and oxalic acids from sawdust and other ligneous materials, ammonia, animal charcoal, and manure from bones, have been sufficiently profitable to encourage further attempts in the same direction. The writer hopes that the following may result in directing attention to some of the means of utilising another waste product.

There is produced in the manufacture of soda by Leblanc's process a larger quantity of hydrochloric acid than can be utilised. The principal use to which it is applied is for the production of chlorine in the manufacture of bleaching powder, but this uses up only a comparatively small proportion of the total production; and many alkali manufacturers throw away nearly all the hydrochloric acid made by them, causing a great amount of mischief in the streams into which the acid is run, in many instances completely ruining them as fishing streams, while the conduct of the luckless manufacturer who can find no better use for his acid, is commented upon by anglers and the lovers of fish diet, in language more remarkable for force than refinement: the former find their sport ruined, and the latter their favourite article of food destroyed.

This acid instead of being thrown to waste can be used to produce chloride of calcium by filling the condensing towers with limestone instead of coke, adding more stone as it is dissolved away by the acid, thus supplying a quantity of chloride of calcium almost unlimited. Or the chloride of calcium may be produced by the reaction of hydrochloric acid, on alkali waste in suitable apparatus, and using the sulphuretted hydrogen given off, in the manufacture of sulphuric acid. There are practical difficulties in the way of adopting this mode of making chloride of calcium, but are they impossibilities? The chief difficulties are that unless great care is taken there is an escape of sulphuretted hydrogen which is not only very disagreeable but is liable to explode when mixed with the atmosphere. Great care is also required when burning sulphuretted hydrogen, for if the supply of air is deficient, sulphur is sublimed and passed into the vitriol chambers unburnt, and if the supply of air is for a short time cut off, sulphuretted hydrogen is liable to get into the chamber and cause an explosion on the admission of an excess of air afterwards. In fact, making sulphuric acid by the use of sulphuretted hydrogen has after many trials by various inventors been found to be one of those matters which become dangerous and impracticable in the hands of ignorant workmen (another illustration of how the ignorance of a population restricts the sources of wealth in a nation).

Another cheap source of chloride of calcium is to be looked for in the bye products from several other processes for the recovery of sulphur from alkali waste, such, for instance, as that of M. Mond, a description of which lately appeared in the CHEMICAL NEWS. The chloride of calcium has in this case the advantage of being free from arsenic, the arsenic being precipitated along with the sulphur as tersulphide. Another ready source of chloride of calcium exists in the bicarbonate of soda manufacture; the chloride is obtained when limestone is acted upon by hydrochloric acid for the production of carbonic acid.

The chloride of calcium by whatever means obtained, should, if it has to be carried to a distance, be boiled to dryness in a reverberatory furnace, and the heat urged till the chloride is in a state of igneous fusion, then drawn into suitable moulds, and when cold it is in a fit condition for packing and transport. It should be packed so as to be protected from the air, on account of its deliquescence.

One of the most prominent reasons why the collection of manure by sedimentary deposition from sewage is not practised, is, that the greater portion of the manurial ingredients, and among them a great proportion of that most important one, phosphoric acid, are held in solution, while the least valuable are contained in the sediment. Chloride of calcium added to town sewage causes the precipitation of phosphoric acid from the soluble phosphates. The compounds of nitrogen are not, however, precipitated except in a very small proportion, and are therefore lost, when only the sedimentary portion of the sewage is used, but this is of little importance compared with that of the loss of the phosphates, as the atmosphere will in time supply to plants sufficient hydrogen, carbon, oxygen, and nitrogen, though of course the latter elements will not be so rapidly assimilated by the plants, as would be the case were they supplied with the manure; but the mineral elements if once exhausted can only be re-supplied by artificial means. Chloride of calcium added in excess to sewage also exerts antifermentative and antiseptic properties, retarding the decomposition of the organic portion; and when fermentation does occur, the evolution of the nauseous and poisonous sulphide of ammonium produced by the decomposition of organic compounds of sulphur and nitrogen, is prevented, by production of chloride of ammonium and sulphide of calcium,* $NH_4S + CaCl = NH_4Cl + CaS$. And when

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* This and the succeeding reactions are inverted when the materials are heated.

carbonate of ammonia is produced by the decomposition of compounds of nitrogen, from which sulphur is absent, or fixed by other combinations, the evolution of ammonia is prevented by the formation of the less volatile chloride of ammonium $NH_4OCO_2 + CaCl = NH_4Cl + CaO\,CO_2$. The addition of the chloride is an effectual preventive of the evolution of compounds of ammonia from stable manure and from cesspools. These properties make chloride of calcium an effective agent in preventing the waste of phosphoric, and, in some cases, ammoniacal compounds, and are well worthy the attention of agricultural and sanitary reformers.

When esparto or other vegetable fibres are used in the manufacture of paper, a solution of caustic soda is employed for dissolving out the resinous and gummy portion of the vegetable from the fibre. The waste lye produced by the operation is allowed to run off in the form of a deep brown-coloured liquid, and consists of extractive matter, and resinous and fatty substances, combined with soda in the form of soap, together with carbonate of soda, caustic soda, and notable quantities of phosphate of soda. All these soda compounds are decomposed by chloride of calcium, causing the formation of corresponding lime compounds and chloride of sodium. The lime compounds being insoluble, are precipitated, carrying with them a large proportion of organic extractive matter, leaving the water with comparatively little colour, and by converting the soda present into common salt, depriving it of many of those noxious qualities which cause paper works to be regarded with such disapprobation when situated on the banks of fishing streams. The precipitate contains all the elements of an excellent manure; its principal disadvantage is owing to the great quantity of water adhering to it, which renders it difficult to remove, and dilutes the manure; but a little practical experience will remove this objection—perhaps spreading it in shallow layers to dry and drain might make it sufficiently concentrated. The chloride of calcium has, in practice, to be used in excess of the quantity theoretically necessary; that excess, however, does not perceptibly injure the water for supporting life in aquatic plants and animals.

On the one hand, the streams of one part of the country are polluted with a powerful alkali, and in another with a corrosive acid; in both instances the streams are unfitted for supporting animal life. In many cases rivers once abounding in salmon, trout, and other valuable edible fish, are now deserted in consequence of these pollutions. All that is required to put an end to this state of things is to use the one to neutralise the other, and produce a harmless neutral salt, and, at the same time, prevent our manurial wealth leaving us by being carried to the sea.

The writer has not at hand the means for forming a correct estimate of the quantity of chloride of calcium that might practically be obtained; but it is evident to any one having any acquaintance with the immense manufactories of soda on the Tyne, the Clyde, and the Mersey, that many thousands of tons of chloride of calcium per annum may be obtained, and the precipitation that of phosphate of lime from sewage, and the use of that precipitate as manure, would go far to supplement or supersede the phosphates imported in the form of guano and bones. All the materials used in its manufacture are cheap and abundant, in some instances costing less than nothing, inasmuch as some of them have at present to be removed out of the way at great expense of carriage and space in which to deposit them.

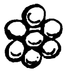
Appleton-Widnes, Aug. 19, 1867.

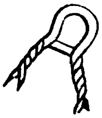
NOTES ON CRYSTALS DEPOSITED FROM THE BRAIN.

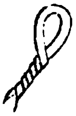
BY S. W. MOORE.

In the month of June this year (1867), Mr. Stuart, curator of the Museum, St. Thomas's Hospital, called my attention to the fact that he had noticed in some of the brain preparations a deposit of crystals which appeared to him to present a very beautiful and unusual appearance; he thought, perhaps, that I might like to examine them chemically, which I have done, thinking the results may lead to facts which will ultimately throw some light on the now very imperfectly understood compounds of the brain.

On inspecting a jar containing the deposit, there was found a very thick layer of crystals at the bottom, which upon further inspection were seen to have the form of rhombic plates; over these, however, there was a layer of what might have been mistaken for mucous or brain matter, but on examination with the microscope they presented a very beautiful appearance, two or three distinct forms being apparent, viz.—*a*, small stars, formed of globular bodies (of which there were seven, six aggregated round one), a little smaller than the male human blood corpuscle. *β*, resembling two pieces of tape, one in a semicircle, the other stretched across its diameter, the ends on both sides being twisted. *γ*. This form was one piece only, its ends being brought round upon one another and twisted.







These strange forms suggested the idea that some albuminous principle might probably have united itself with a crystalline substance, and have caused these structures to become manifest in the attempt to crystallize; they gave under the influence of polarized light a distinct cross, and what seems to confirm the supposition of their being a colloid is that upon testing nitrogen was developed. They are saponified by KHO, and dissolved by hot absolute alcohol, and separate out on cooling in a granular form, and are of course insoluble in water.

On presenting the various tests to the crystals which were so densely crowded at the bottom of the vessel, some very interesting data were collected, agreeing with the tests for no other hitherto mentioned brain compound.

In appearance the crystals were waxy, they were tasteless and insoluble in water; on ignition they burned away with a bright smoky flame, leaving no residue whatever. The tests for N, P, and S, were carefully applied, but with no result; the substance was precipitable from its ethereal solution by alcohol; its melting-point was 103° C., and on combustion it gave the following percentage:—

Carbon	43·79	
Hydrogen	8·09	
Oxygen	48·12	by difference
	100·00	

From this an empirical formula may be calculated, having the following constitution, $C_7H_{16}O_6$, or $C_{12}H_{26}O_{10}$, the latter perhaps giving a calculated result nearer the found one, viz.,—

Carbon	43·64
Hydrogen	7·88
Oxygen	48·48
	100·00

From the results obtained above we may safely conclude that the substance is not cholesterine, its high percentage of oxygen, and its low melting-point, excluding it from that supposition. It is equally impossible that it should be cerebric acid, because it is perfectly neutral and contains no nitrogen, the absence of phosphorus proves it cannot be oleophosphoric acid.

I hope to obtain some more of the substance, when further experiments will be made, from which I shall doubtless obtain something of a more definite nature, and be enabled to give it a rational formula, it appearing to have been up to the present time unnoticed.

On exposing to the air the spirits from which the crystals had been taken, a fresh crop formed; these, however, were only crystalline plates of cholesterine.

AN IMPORTANT ADJUNCT TO THE INDUCTION COIL.

BY HENRY MORTON, PH.D.

The arrangements I am about to describe have proved of great value to me, and will, I presume, be of like use to others who may have need of similar lecture illustrations.

Take eight plates of glass, about 11 inches by 14 inches, and attach to both sides of each plate sheets of tinfoil 7 inches by 10 inches in size, with rounded corners. Set these plates upright in a box (provided with grooves for the purpose) about 1 and a half inches apart; then, rolling up some balls of paper large enough to fit between the plates, and wrapping a strip of tinfoil around each ball, thrust them between the plates, and, lastly, make an outside pole to the terminal sheets of foil by means of wires enclosed in glass tubes passed through the side or top of the box. It is evident that we have here a compact form of Leyden battery arranged for "cascade." With the ordinary electrical machine such an arrangement would be worthless from its want of insulation. With the induction coil, however, which developes an entire charge in an instant, it becomes of great value in a certain class of experiments, because it gives us at once the concentrated charge peculiar to the Leyden battery, combined with a spark length which is otherwise lost. (This property of long spark in the "cascade" arrangement of jars is well known.)

If such an apparatus as we have just described be connected with the secondary poles of an induction coil, and other wires are then led off (with a break in the circuit, however, of ¼ to ¾ inches) to some piece of apparatus for the illustration of electric discharge in vacuo, such as Gassiot's cascade (especially with a canary goblet), the Aurora tube, an electric egg of canary glass, etc. (but not a Geissler tube), the brightness of the illumination and volume of the discharge will be immensely increased. Thus a goblet invisible at 30 feet when the unaided coil is used, becomes *brilliant* at 50 feet with this attachment. I have used two coils with the above apparatus, both made by Mr. E. S. Ritchie, of Boston, one (which is my own property) yielding a spark of 8 inches, the other (also in my hands, as it belongs to the Physical Cabinet of the University of Pennsylvania) which gives, in its present mounting, sparks of 16 inches, but is capable of yielding sparks two feet in length. Such sparks were, in fact, obtained from it by Mr. Ritchie during its manufacture; but, in mounting it, the poles have been secured at a maximum distance of 16 inches to provide against accident, such a length being abundantly sufficient for use. How the above battery would work with smaller coils I cannot say. Geissler tubes, unless of very large area, are not benefited in appearance by this arrangement; because, as I believe, the coil unaided can supply all the electricity they are capable of transmitting, and this excessive charge only tends to develope inductive resistances in the glass tubes themselves, which resistances this *momentary* current is the least fitted to overcome.

Allow me to mention another little practical detail in this connection. It is generally assumed that the induction coil is unfit for the exhibition of those experiments of attraction and repulsion which especially characterise statical electricity. A great number, however, may be very satisfactorily exhibited by charging Leyden jars and using them as the sources of electricity. Thus, connect a chime of bells with the knob of a large jar, connect the outer coating with the earth and with the negative pole of the coil; then bring the positive pole within striking distance of the knob, and charge by a few sparks. The electrical flyer, orrery, sportsman and birds may be successfully operated in this way, even in summer weather.

Probably, however, the coil should not be of less than 6 inches spark length.

University of Penna., Philadelphia.

ON THE SO-CALLED "INACTIVE" CONDITION OF SOLIDS.

BY CHARLES TOMLINSON, F.R.S.

In the Chemical News for the 2nd of August (*Amer. Reprint, Oct.* 1867, page 162) is given a notice of my paper on the above subject, in which I endeavour to prove that the action of solids in disengaging gases from their solutions, or in inducing crystallisation in saline solutions, is simply a question of adhesion depending on the state of purity of the surface of the solid. I have since endeavoured to express my theory in such general terms as to embrace a larger number of phenomena, which indeed seem to increase the more it is examined.

My theory, as it now stands, is as follows:—

Any supersaturated solution of gas, with its upper surface freely exposed to the air, is always giving off that gas, either with effervescence, or silently and imperceptibly. It does so because the excess of gas has only a slight adhesion for the liquid, and the air is virtually a vacuum for it, the only difference being, that it would pass off into a real vacuum suddenly and instantaneously. The remaining surface of the liquid, or that confined by the sides of the vessel, is in exactly the same state, subject however to two conditions—(1) the purity of their surface, and (2) the pressure exerted by them (virtually) on the liquid.

(1.) Suppose the vessel to be chemically clean. No gas will be disengaged, and no bubbles will form on the sides, because the adhesion between the sides and the liquid is perfect. Hence the sides may be considered, *pro râta*, as merely a continuation of the liquid itself, and no bubbles will form there any more than in the central parts of the liquid. But suppose the sides to be dirty, adhesion is diminished *or* annulled; and therefore the surface of the liquid next to such sides is virtually as free as its upper surface. (2.) Hence bubbles will form here, just as they form on the upper surface; but in the latter case they do not appear as bubbles (except in effervescence) because there is no pressure. The sides do exert pressure, and therefore bubbles are formed. Now it does not at all matter whether there be air or not between the sides and the liquid: there may probably be a vacuum or any other gas. The result will be the same. Hence it is futile to talk of the air as disengaging bubbles, as in M. Gernez's theory; it is really want of adhesion. Now to apply this to the case of the so-called "inactive" glass rod. A rod, a coin, a piece of flint, etc., placed in the liquid, does nothing more than form new sides, as it were, to the vessel, and its effect is merely that of the sides. If chemically clean, the rod, etc., will form no bubbles round it, and it is called "inactive" because its adhesion is perfect. If dirty, the surface of liquid in contact with it will be as free, or almost so, as the upper surface.

The same theory applies equally well to the action of *nuclei* in inducing crystallisation. It also applies to the common theory of ebullition, and the action of the vessel in raising or lowering the boiling-point under the same pressure. Writers down to our own day state that water boils at about 105° C. in a glass vessel, and at 100° in a metal vessel; at a lower temperature in vessels whose internal surfaces are rough than in smooth ones; that *bumping* is produced when the liquid has comparatively little adhesion to air; and soon I think it can be proved that these and other phenomena which figure in our text-books as remarkable facts, can be explained with reference to the same law of adhesion, and have nothing to do with the air except indirectly.

King's College, London, Aug. 31, 1867.

REVISION OF THE MINERAL PHOSPHATES.

BY A. H. CHURCH, M.A.,

PROFESSOR OF CHEMISTRY, R.A. COLLEGE, CIRENCESTER.

(Continued from vol. XII., p. 183, Eng. Ed. CHEM. NEWS.)

No. VI. OSTEOLITE.

A STATEMENT appears in some chemical works to the effect that osteolite, a white and compact mineral not unlike fine lithographic stone, is really pure tricalcic diphosphate $Ca''_3 2PO_4$. All the analyses, however, of the substance which have been published, point to a very different conclusion; and, in fact, a pure native tricalcic diphosphate is still unknown. My analyses of specimens of osteolite from various localities serve to confirm the notion that this so-called species is merely an apatite more or less altered by the substitution of calcic carbonate for the chloride or fluoride.

One of my specimens was from Eichen, Wetterhau. It was white, hard, and tough, showed slight signs of being stratified, and had a density of about 2·86. The following are the analytical results:—

35·46 grains osteolite gave........	·83 grain	HO
" " "	30·97 "	$Ca''_3 2PO_4$
" " "	4·26 "	$Ca''CO_3$
55·32 " "	1·39 "	CO_2

From these results we find that more lime was present in the mineral than sufficed to saturate the phosphoric and carbonic acids. Qualitative tests revealed the presence of much fluorine. If the remainder of the calcium be calculated as if in union with fluorine, the following satisfactory percentages are shown:—

$Ca''_3 2\ PO$	87·25
$CaCO_3$...........................	5·70
CaF_2	4·92
H_2O...........................	2·34
	100·21

Osteolite, therefore, cannot rank as a distinct species: it is a more or less altered apatite.

ON THE REFRACTION EQUIVALENTS OF SALTS IN SOLUTION.*

BY J. H. GLADSTONE, F.R.S.

THE British Association has already more than once heard of "refraction-equivalents," but for many chemists the term may still require definition. It is well known that every body has the power of bending a ray of transmitted light, and that this power may be expressed by a number, termed the "refractive index." Now this "refractive index," *minus* unity, divided by the density of the body, is termed its "specific refractive energy,"—a property of great importance, and one that accompanies the body, notwithstanding great physical or chemical changes; for instance, to quote words formerly used, as a rule, when a gas, liquid, or solid dissolves in water, it preserves its specific refractive energy."† For many purposes it is convenient to multiply this number by the atomic weight of the substance, and that is termed its "refraction-equivalent."

Now it is not difficult to arrive at the refraction-equivalent of a salt in solution. Let a weight of it, answering to its chemical equivalent, be dissolved in a certain number of equivalents of water; the refraction-equivalent of the whole solution will consist of the refraction-equivalent of the salt, *plus* so many times the refraction-equivalent of water; and as this number is known, we have only to subtract it from the whole to obtain the refraction-equivalent desired.

It occurred to me that if a series of salts in solution were thus examined, I might arrive at numbers from which many interesting facts might be deduced, and especially that it might afford data for determining the refraction-equivalents of all the metals, and of those substances with which these metals will combine to form soluble salts.

As the determinations are matters of great delicacy, especially when the solutions are weak, I am having a superior apparatus made by Mr. Browning for the purpose; but some preliminary observations have been made with the old apparatus of Baden Powell, and these perfectly confirm my expectations, and induce me to undertake a careful examination of the whole subject.

In the first place I prepared solutions of iodides,

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* Read before the British Association, in Section B.

† *Journ. Chemical Society*, May, 1863.

bromides, and chlorides. The metallic iodides gave refraction-equivalents ranging from 30·5 to 35·3; the metallic bromides from 21·7 to 25·7; and the metallic chlorides from 15·1 to 18·6, the highest number in each instance being the potassium salt, the while ammonium compounds of these halogens gave numbers more than three higher. Again, it was at once evident that the dispersion-equivalent of an iodide was at least double that of a bromide, and three times that of a chloride. On comparing the salts of the same metal this difference between the halogens was still better defined, the number for the iodide almost invariably exceeding that for the bromide by a little more than ten, and that for the chloride by a little more than sixteen. It was evident, therefore, that the halogen was exerting the same influence on the rays of light with whatever metal it was combined; and that any metal, as calcium, was unchanged in its refraction-equivalent, whether it was united to chlorine, bromine, or iodine. This observation was subsequently extended to a totally different class of salt, the sulphates, which give numbers always about one less than those given by the chlorides.

It may be asked—What numbers do you deduce from these results as to the refraction-equivalents of the metals? Unfortunately I am not in a position to reply with certainty. If we knew the refraction-equivalents of chlorine, bromine, or iodine, it would be easy; but the numbers previously deduced for them from organic compounds evidently require some rectification before they can be applied to this purpose. I hoped to arrive at the matter from the refraction-equivalents of the hydracids in solution, as the number for hydrogen is known to be 1·3; but I obtained such high numbers for hydriodic, hydrobromic, and hydrochloric acids, that I am disposed to think hydrogen in these compounds must exert a far greater refractive influence on the rays of light than when alone, or combined with carbon or oxygen.

In any case the refraction-equivalents of the metals examined in solution are very low as compared with the known refraction-equivalents of non-metallic bodies, except those that have very small atomic weights. They present themselves in about the following order, commencing with the lowest:—Magnesium; lithium; sodium; zinc; calcium; manganese; cadmium; copper; strontium; iron (ferricum); barium; potassium; ammonium.

ON THE COMMERCIAL ANALYSIS OF SOME OF THE PRODUCTS AND MATERIALS OF THE ALKALI MANUFACTURE, Etc.

BY C. R. A. WRIGHT, B.S.C., F.C.S.

(I.) **Salt-cake.**—Ordinary salt-cake is valued according to the percentage of "Available sulphate of soda" contained; *i.e.*, the percentage of Na_2SO_4 existing mainly as such, and partly as $NaHSO_4$. The mode of estimation of the available sulphate usually pursued is the following:—

1. The NaCl is determined volumetrically by a standard silver solution.

2. The quantity of a standard alkaline solution required to render a known weight of salt-cake exactly neutral to test papers is determined, and the result, sometimes calculated as SO_3 sometimes, as SO_4H_2, called "free acid."

3. The difference between the sum of the two previous determinations and 100 is assumed to be "available sulphate of soda."

By this mode of proceeding errors of one to three or more per cent. are introduced; ordinary salt-cake containing, in addition to Na_2SO_4, $NaHSO_4$, and NaCl, perceptible quantities of $PbSO_4$, $Fe_2\ (SO_4)_3$, Fe_2O_3, $CaSO_4$, $MgSO_4$, moisture, and particles of sand, brick, etc., derived from the furnace during the manufacturing processes. Where a greater degree of accuracy is desirable, a known weight of salt-cake may be treated with water, ammonia and ammonium oxalate added to the unfiltered solution, and the precipitated Fe_2O_3 and $CaCO_3$, with the insoluble matters, weighed after ignition: by moistening the ignited precipitate with pure SO_4H_2, and igniting again, the $CaCO_3$ is converted into $CaSO_4$, and then the weight of the mixed substances indicates all the "impurities" present in the salt-cake, with the exception of the $MgSO_4$, which rarely amounts to more than traces, and the moisture, which is occasionally a very perceptible quantity, especially in samples that have been made some length of time.

The amount of ferric sulphate present depends on the degree of heat to which the salt-cake has been subjected during manufacture. In highly roasted samples, cold water yields a solution containing no iron whatever, all the iron present in the salt-cake consequently existing as Fe_2O_3; specimens of under-roasted salt-cake, on the other hand, when treated with cold water, leave only fragments of brick, $CaSO_4$, etc., undissolved, all the iron existing as $Fe_2(SO_4)_3$. In ordinary salt-cake, however, there is so little ferric sulphate, that no perceptible error is committed in assuming that all iron present exists as Fe_2O_3, and all the "free acid" as $NaHSO_4$. Accordingly the following methods have been found to give tolerably expeditiously the exact composition of such salt-cake.

(*a.*) A known weight, 5 or 10 grammes, is dried at 110°—120° C., till constant in weight; too great elevation of temperature being avoided to prevent any possible loss of HCl by reaction of the $NaHSO_4$ on the NaCl present.

(*b.*) The NaCl is determined volumetrically by a standard silver solution.

(*c.*) A solution of sodium hydrate free from carbonate, or of caustic ammonia, of known strength, is added to a known weight of salt-cake until test-papers indicate exact neutrality of the liquid; the alkaline solution used accordingly corresponds to the $Fe_2(SO_4)$, and $NaHSO_4$ together, and may therefore be safely calculated as the latter.

(*d.*) A known weight of salt-cake is boiled with an excess of a standard sodium carbonate solution, and filtered; the unneutralised alkali is then determined by a standard acid solution. The amount of alkaline solution neutralised by the salt-cake indicates the $CaSO_4$, $NaHSO_4$, and $Fe_2(SO_4)_3$ together; and hence the difference between (*c*) and (*d*) indicates the $CaSO_4$. Or the $CaSO_4$ may be determined gravimetrically by precipitation with ammonium oxalate after separation of the Fe_2O_3 by ammonia from the solution of a known weight of salt-cake in hydrochloric acid.

(*e.*) The precipitate thrown down in (*c*) may be collected and boiled with hydrochloric acid; the insoluble bricks, etc., may be weighed, and the ferric salt reduced by zinc or other reducing agent, and titrated volumetrically by permanganate or otherwise.

(*f.*) When the $PbSO_4$ is to be determined, it may be done by treating a considerable quantity, say twenty grammes, with water, and boiling the insoluble residue with strong hydrochloric acid, till the $PbSO_4$ is entirely dissolved, and from the solution PbS may be thrown down by sulphuretted hydrogen, and the lead determined in the ordinary way.

(*g*.) If $MgSO_4$ is to be determined, it may be done by dissolving a known weight, say twenty grammes, in hydrochloric acid, adding ammonia and ammonium oxalate, and precipitating the magnesia from the filtrate by a phosphate, and ultimately weighing the magnesium pyrophosphate.

(*h*.) If the preceding determinations have been carefully conducted, the difference between 100 and the sum of them may be safely taken as Na_2SO_4; if this is to be directly determined, however, it may be done either by determining the total SO_4 present by dissolving a known weight of salt-cake in hydrochloric acid, and precipitating by $BaCl_2$, and weighing the $BaSO_4$; subtracting the SO_4 contained as $CaSO_4$, $NaHSO_4$, $MgSO_4$, $PbSO_4$, the remainder being calculated as Na_2SO_4; or by adding ammonia and ammonium oxalate to the aqueous solution of a known weight, and estimation of the residue left on evaporation of the filtrate and ignition with SO_4H_2: on subtraction from this of the amounts due to $MgSO_4$,NaCl, and $NaHSO_4$, the Na_2SO_4 is directly ascertained.

The writer has obtained very concordant results by either of these plans, viz., estimation of Na_2SO_4 by difference, by determination of total SO_4 present, or by determination of total Na present. The total "available sulphate of soda" is known by adding $\frac{71}{120}$ of the $NaHSO_4$ to the amount of Na_2SO_4 found.

II.—**Black-Ash** is rarely sold as such, being generally converted into soda-ash on the spot where it is made. Commercially, the only valuable ingredient is the sodium carbonate, the amount of which is generally determined by lixiviation of a known weight of black-ash, and titration by normal test acid of the liquor obtained. In manufacturing establishments it is frequently the practice to lixiviate the ash with water at some definite temperature, considered to be about the average temperature of the lixiviating vats; the liquor so obtained is examined (*a*) for alkali, determined by test acid; (*b*) for sodium sulphate, generally estimated roughly, but with sufficient nearness for manufacturing purposes, by addition of a standard barium chloride solution to a portion of the acidulated lixiviate, till no further precipitate is thrown down; (*c*) for sulphide, estimated by passing chlorine through the alkaline lixiviate till all sulphide is destroyed; boiling with hydrochloric acid, and volumetric determination of the sulphate as before, the increased amount representing the sulphide. Prizes are frequently given to those workmen who produce black-ash containing but little sulphate, showing a nearly complete decomposition of the salt-cake employed; and occasionally prizes are given when the sulphate after oxidation is low in amount, it being supposed that this indicates that over-roasting of the black-ash has not occurred. A slight misapprehension, however, usually attends this mode of analysis; although an over-roasted black-ash will yield a perceptible quantity of sulphide when treated with nearly absolute alcohol, yet the fact of an aqueous solution containing sulphide by no means proves that the ash was over-roasted, inasmuch as on addition of water to black-ash there is always a mutual reaction between the CaS, and Na_2CO_3 contained therein; the amount of Na_2S formed therein, as the researches of M. Kolb have shown,* depends on the temperature and dilution of the liquid, and the time employed; and accordingly it is often found that the sulphide existing in the black-ash lye from the vats is very different in amount from that calculated from the laboratory analyses of the black-ash worked. The laboratory test for "sulphate after oxidation," therefore, is really useless, as it neither denotes the quality of work done by the furnace-man nor that of the black-ash lye.

The writer has shown in a recent paper (*Chem. Soc. Journ.*, xx., 407) that there is contained in ordinary black-ash a sodium compound insoluble in hot water even on long digestion, but decomposable by long continued boiling. In cases, therefore, where the total "available alkali is to be exactly determined, either this long boiling must be performed, or the total sodium present must be determined gravimetrically, and that contained as chloride and sulphate subtracted; in either case a tedious operation. The same applies in the case of the analysis of the lixiviated black-ash, or vat-waste. Ordinarily the vat-waste is examined by lixiviating or washing on a filter a known weight of waste fresh from the vats, or previously completely dried. In either case a considerable amount of calcium hydric sulphide comes into solution, and hence if the solution so obtained be immediately titrated with test-acid, more soda is indicated as present than really has been dissolved out. By passing CO_2 through the solution till H_2S is completely expelled, boiling to decompose calcium bicarbonate, and filtration from the precipitated calcium carbonate, this error is avoided. The same effect is produced by adding ammonium carbonate to the solution and boiling in a flask till no further evolution of ammoniacal gases takes place; but in either case the sodium contained in the insoluble compound, or as sulphate (found by oxidation of calcium sulphide and subsequent reaction on the sodium carbonate, especially if the waste have been previously dried), remains unestimated. When accuracy is required, therefore, a gravimetric determination of sodium is unavoidable.

In cases where an accurate analysis of the total contents of a sample of black-ash is required, the following method gives reliable results tolerably speedily. Most of the modes of determination are likewise applicable to samples of dry vat-waste:—

(*a*). A known weight is dissolved in hydrochloric acid, the insoluble coke and sand collected on a weighed filter, and the carbon subsequently burnt off.

(*b*). In the filtrate from (*a*) the SO_4 is estimated by precipitation by barium chloride and weighing the $BaSO_4$.

(*c*). A known weight is dissolved in nitric acid, and the Cl determined volumetrically by a standard silver solution.

(*d*). A known weight is treated in Mohr's CO_2 apparatus; the ammonium carbonate found precipitated by boiling with calcium chloride; the precipitate washed till the washings are neutral, dissolved in a slight excess of standard hydrochloric acid, and the excess determined by a standard alkaline solution; thus the CO_2 can be calculated.

(*e*). A known weight is fused with four times its weight of a mixture of three parts dry sodium carbonate and one of potassium nitrate (both free from sulphate). From the total sulphate thus formed, and estimated gravimetrically by barium, that existing as Na_2SO_4 is subtracted, and the remainder calculated as S.

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* *Annales de Chemie et Physique*, June, 1866.—*Vide* Chemical News, Nos. 345—347.—[*Eng. Ed.*]

(*f*). A known weight is treated with hydrochloric acid, the filtrate oxidized by nitric acid, and the mixed $Fe_2O_3Al_2O_3$ and P_2O_5 precipitated by ammonia.

(*g*). The filtrate from (*f*) is treated with ammonium oxalate, the precipitate estimated volumetrically by permanganate, or gravimetrically as $CaCO_3$; hence the Ca known.

(*h*). A known weight is lixiviated with warm water, and in the filtrate from the insoluble matter the SiO_2 estimated by evaporation to dryness with hydrochloric acid; in the filtrate from this the Al_2O_4 combined as aluminate is determined by precipitating the alumina by ammonia.

(*i*). A known weight is cautiously treated with sulphuric acid in a capacious platinum crucible, and heated till gases cease to be evolved; the residue is treated with water, filtered and well washed, ammonia and ammonium oxalate added to the filtrate; and ultimately the total Na contained weighed as Na_2SO_4.

In calculating results from the foregoing data, the Cl found is calculated as NaCl, the SO_4 as Na_2SO_4, the SiO_2 as Na_2SiO_3, and the Al_2O_4 (soluble in water) as $Na_2Al_2O_4$, the remaining sodium is then calculated as Na_2CO_3, and the remaining CO_2 as $CaCO_3$. The sulphur is calculated as CaS, and the remaining calcium as CaO. From the total $Al_2O_3+Fe_2O_3+P_2O_5$ the alumina present as aluminate is subtracted; the coke and sand, etc., are directly determined (*a*). The difference from 100 in a carefully conducted analysis will not amount to more than a few tenths per cent., and represents cyanogen, traces of moisture, etc., and loss.

In an over-roasted ash the alkaline sulphide can only be safely estimated by digestion with nearly absolute alcohol, oxidation to sulphate by chlorine, and precipitation by barium. The Na contained as poly, or mono, sulphide, may be determined volumetrically by test acid in the alcoholic solution, and must be subtracted from that to be calculated as Na_2CO_3 as above: the S existing as poly, or mono, sulphide of sodium must be subtracted from the total sulphur found, the difference being calculated as CaS.

ON THE ECONOMISATION OF SULPHUROUS ACID IN COPPER SMELTING.*

BY PETER SPENCE, F.C.S.

It will be in the recollection of members of this Section that Lord Derby, in 1861, obtained the appointment of a committee of the House of Lords, for obtaining evidence as to the noxious vapours from chemical works, etc. That investigation, carried over many months, resulted in the passing of what is called the "Alkali Works Act," which has been so ably and successfully carried out by my friend Dr. Angus Smith.

At the same time a large amount of evidence was taken as to the emission of sulphurous acid, and arsenious acid, from the copper smelting works at Swansea, and other parts of the country; but no legislation was adopted, because, with the exception of the writer of this paper, all the witnesses testified to their being no practicable means of suppressing the nuisance without destroying the trade.

Copper smelting as now conducted appears at first sight a very crude, but is in reality a very beautiful, chemical process. The ores used are of a heterogeneous character, chiefly iron pyrites more or less impregnated with copper, and containing besides arsenides and sulphides of various other metals, with a large mixture of quartz. The first process of the copper smelter is by calcination to separate a quantity of the sulphur and as much as possible of the arsenic; for this purpose the mixed ores are exposed to a red heat, and these bodies are dissipated into the atmosphere. When calcined the ores must still retain a portion of the sulphur, varying with its richness in copper, this sulphur playing an important part in the next operation. The calcined ores are now melted down by a great heat into a fluid state, when the sulphur not dissipated unites with a portion of the iron and every trace of the copper, for which it has great affinity, and sinks to the bottom of the furnace carrying down with it any of the precious metals which may be present. The large mass of the fluid now floating at the top is silicate of iron, and is skimmed off and thrown away as slag. The *regulus* run from the bottom of the furnace contains from 20 to 35 per cent. of copper, and almost invariably 28 per cent. of sulphur. The *regulus* is again calcined to throw off the sulphur, and the subsequent processes of refining take place.

I effect the saving of sulphur by calcining in long furnaces, the bed being heated from below, air being made to travel from one end of the furnace to the other over the heated ore, the air and SO_2 being passed from the furnace directly to the lead chambers, the ore being at regular intervals made to traverse in an opposite direction, coming out calcined where the air enters. The calcination of *regulus* is exactly similar in both cases, the calcination is only carried to a certain point, 8 to 9 per cent. of sulphur being left in both the ore and regulus.

This process, carried out for several years chiefly in vitriol works, is now being successfully employed by the Goole Alum Smelting Company, on a large scale as a copper smelting process. It has been in operation there for over twelve months, and at present 150 to 200 tons of mixed ores are being smelted weekly. These ores are Cornish, Swedish, Norwegian, and Spanish.

About two months ago I sent down one of my chemical assistants to superintend during a month some large working experiments, analysing the results at every stage, so that reliable data might be got. One of these experiments I append, and as it is typical of the general operations, it may safely be taken as indicating what is being done.

	Sulphur.			
	tons.	cts.	qrs.	lbs.
10½ tons Cornish ores containing 19 per cent. sulphur 13½ " Spanish smalls " 47 " " { mixture 33·3 per cent. } =	8	0	0	0
This was calcined, the SO_2 going to the vitriol chamber. The result was 22 tons of calcined ore, containing 8 per cent. sulphur..............................	1	15	0	0
The ore when smelted gave 2 tons 15 cwt. of regulus containing 28 per cent. sulphur..............................	0	15	1	20
The loss in sulphur dissipated is therefore...	0	19	2	8
This regulus calcined, the SO_2 going to the vitriol chamber became 2 tons, 10 cwt., containing 9 per cent. of sulphur........	0	4	2	20
No more sulphur can be economised, therefore the total loss of sulphur is..........	1	4	1	0

Or as under,

Sulphur economised.................	84·8 per cent.
Sulphur lost.......................	15·4 "
Total sulphur in ore.................	100·0

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* Read before the British Association, in Section B.

ON A NEW TELEGRAPHIC THERMOMETER.*

BY PROFESSOR WHEATSTONE, F.R.S.

The telegraphic thermometer which I constructed in 1843, and which is described in the report of the thirteenth meeting of the British Association, depended on the simultaneous action of two isochronous chronometer or clock movements—one at the remote station regulating the motion of a plunger in the bore of a thermometer, and the other at the near or observing station, marking, by the motion of the needle of a galvanometer, the moment at which the contact of the plunger with the mercury of the distant thermometer completed or broke the circuit. The clock movements required to be periodically wound up, and therefore the affected instrument could not be left to itself for an indefinite time. There are, however, many situations in which it might be desirable to have meteorologic indications when the instruments would not be accessible for very long periods. I have, therefore, devised a new class of telegraphic meteorometers which shall be independent of clock work, and may remain in any situation of difficult access as long as the instrument endures. This principle is applicable to all instruments which indicate by means of a revolving hand, and I have already devised its application to a Breguet's metallic thermometer, an aneroid barometer, and a hygrometer, depending on the absorption of moisture by a thin membrane. It is also applicable to a bar magnet in a fixed position, and to a variety of other indicators. The apparatus consists of two distinct instruments connected only by telegraphic wires. The first I will call the questioner (A), the second the responder (B). The questioner (A) is a rectangular box, presenting externally a circular dial face, round which are engraved the degrees both of the Fahrenheit and Centigrade thermometric scales, the former ranging from 20° below zero F. to 220° above that point, and the latter from 0° to 110° C. It shows, besides, three binding screws for the purpose of connecting the telegraphic wires, and a handle which causes the rotation of the armature of a magneto-motor in the interior. This magneto-motor is similar in its construction to that employed in my alphabetic magnetic telegraph; a soft iron armature rotating before the four poles of the magnet occasions, when the circuit is completed, alternate currents of equal intensity. The box also contains a small electro-magnet, which acts by means of mechanism similar to that employed in the indicator of the aforesaid telegraph, and causes the revolution of the index of the dial. The responder (B) is a cylindrical brass box, which presents on its upper surface a similar dial, with its thermometric scales and index; at its base three binding screws, corresponding to those of the questioner, are fixed for connecting the telegraphic wires; and it is furnished with a brass cover that it may be hermetically sealed when lowered in the sea or buried in the ground. Its interior contains three essentially distinct parts.—1. The metallic thermometer, which consists of a spiral ribbon of two dissimilar metals, with its hand capable of ranging through the extent of the circular thermometric scale of the dial;—2. A small electro-magnet, acting by means of a propelment on a disc, making as many steps in one rotation as there are half-degrees on the scale;—3. An axis to which is fixed a delicate spiral spring, which causes a pin to bear lightly against the hand of the thermometer however it may vary in position. The two instruments are connected by means of two telegraphic wires. The first proceeds from an earth plate at the near station, passes through the coil of the electro-motor in A, joins the coil of the small electro-magnet in B, and then proceeds to another earth plate at the distant station. The second wire is permanently connected with the first between the earth plate and the coil of the magneto-motor, and includes that of the electro-magnet in B, and its opposite end is brought close to the remote end of the first wire. The mechanism is so disposed that when the first wire is disconnected from its earth terminal it is brought into circuit with the second wire. By this arrangement, when the dial of A is brought to 0° and the handle turned, at the first moment the circuit is completed through the first wire, containing the coil of the electro-magnet in B, and the return earth. A disc is thereby caused to revolve in an opposite direction to the graduation of the scale until a pin, originally starting from 0°, comes into contact with the pin, pressing against the thermometer hand, and thereby completes the circuit of the second wire, and breaks the connection with the earth plate. At first only the electro-magnet in B is acted upon, but when the currents are diverted into the new channel both the electro-magnets act simultaneously. In consequence of the action of the electro-magnet in A, the hand of its dial passes over a space corresponding with that between 0° and that indicated by the thermometers and the hand of the dial ultimately accords with that of the distant thermometer. When the hand of the dial on A comes to rest the disc in B arrives at 0°, and a catch permits the spiral spring to unwind itself, and its pin flies to and presses against the thermometer-hand. It must be observed that instruments thus constructed are not capable of marking every possible gradation; but they may be made to indicate divisions of the scale of any required minuteness. It is advisable to limit the extent of the scale when more minute divisions are deemed necessary. The only circumstance that can affect the accuracy of the indications of the instrument is this—the pin pressing against the thermometric index displaces it a little, and causes it to assume a position about a degree in advance, but as this pressure is a constant one, the inconvenience is remedied by a slight corresponding shifting of the scale. In this class of instruments the indications are not spontaneously conveyed to the observer, but they must be asked for, and whenever this is done the indications will be immediately transmitted to him however frequently the question is put. The uses to which this telegraphic thermometer may be applied are, among others, the following:—The responder may be placed at the top of a high mountain and left there for any length of time, while its indications may be read at any station below. Thus, if there should be no insuperable difficulties in placing the wires, the indications of a thermometer placed at the summit of Mont Blanc may be read as often as required at Chamouni. A year's hourly observations under such circumstances would no doubt be of great value. If it be required to ascertain during a long-continued period the temperature of the earth at different depths below its surface, several responders may be permanently buried at the required depths. It will not be requisite to have separate questioners for each, as the same may be applied successively to all the different wires. The responder, made perfectly water-tight, in which there would be no difficulty, might be lowered to the bottom of the

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* Read before the British Association, in Section A.

sea, and its indications read at any intervals during its descent. In the present mode of making marine thermometric observations, it is necessary that the thermometer should be raised whenever a fresh observation is required to be made.

ON THE ELECTRICAL RESISTANCES OF THE FIXED AND VOLATILE OILS.*

BY T. T. P. BRUCE WARREN.

The want of an acknowledged and reliable means of recognising the purity or condition of samples of oils has long been felt by pharmaceutists. No tests, or system of tests at present used are free from objection. An inspection of the optical characters of the oils, whether fixed or volatile, will be sufficient to confirm the truth of this observation.

The polariscope has at best a very limited scope of application, whilst the determination of the refractive or dispersive qualities requires such precise adjustments that the suitability either of the one or the other for the purposes of a technical test may be fairly questioned. The refractive power of the oils, both fixed and volatile, has so small a variation, even between the two extremes of the scale here given, that the difference produced on the refractive power of any oil by the addition of a small quantity of another, would be barely perceptible. The objection against the measurement of the dispersive action as a means of expressing the value of an oil, is that the determination of the differences of indices of refraction for the extreme rays is at once tedious and unreliable; the scale of dispersions offers, however, a much wider range of differences.

It is probable that the comparison of two samples of oil by the irrationalities of their dispersion is worthy of some attention. I am not aware of its being applied as a test, but the samples could stand side by side with respect to the illuminating source, and their spectra projected side by side could be easily observed and compared.

The tables here given of the dispersive powers, and the irrationalities of dispersions, are by Sir David Brewster; the refractive powers are principally by the same authority.

Table of Oils.—Arranged in order according as they contract the less refrangible, and expand the more refrangible spaces (irrationalities of dispersion).

Oil of Cassia
" Bitter Almonds
" Aniseed
" Sassafras
" Fennel
" Cloves
" Turpentine
" Carraway

Oil of Nutmeg
" Peppermint
" Castor
" Nut
" Olive
" Sweet almonds
" Alcohol

Although bromine and iodine exert on some of the essential oils chemically characteristic effects, it does not appear certain to what extent the action may be modified by the addition of small quantities of other oils; consequently the chemical phenomena, as well as a knowledge of their specific gravities, and boiling points, cannot be considered as offering any assistance to the detection of accidental or intentional impurities when existing in small quantities.

OPTICAL QUALITIES OF OILS.

Name of Oil.	Index of Refraction.	Dispersive Power.	Difference of Index of Refraction for extreme rays.
Anise	1·601	·077	·044
Almond bitter	1·603	·079	·048
Almond sweet	1·483	—	—
Angelica	1·493	·051	·025
Bergamott	1·471	—	—
Cassia	1·641	·139	·089
Carraway	1·491	·049	·024
Castor	1·490	·036	·018
Camomile	1·457	—	—
Cloves	1·535	·062	·033
Cumin	1·508	.065	·033
Dill	1·477	—	—
Fennel	1·506	·055	·028
Juniper	1·473	·047	·022
Lemon	1·476	—	—
Lavender	1·462	—	—
Nutmeg	1·497	—	—
Olive	1·470	·038	·018
Poppy	1·463	—	—
Pennyroyal	1·482	—	—
Peppermint	—	—	—
Rape-seed	1·475	—	—
Sassafras	1·534	·069	·032
Spearmint	1·481	·054	·026
Alcohol	1·372	·029	·011
Turpentine	1·475	·042	·020

The process which I have to submit to you is one which has given great satisfaction in all the experiments which I have made, and was suggested by a discovery due to M. Rousseau, quoted by De La Rive,* "that olive oil when mixed with $\frac{1}{100}$th part of its volume of oil of poppies, increased the number of vibrations of a magnetic needle in a given time, when the same was included or made to form part of a voltaic circuit." This isolated fact would be of service for the determination of the purity of olive oil, if oil of poppies were the only sophisticating ingredient.†

I thought it useful to extend the observation to the effects produced by other oils when mixed with oil of olive, and to ascertain how far the process might be applied as a test for the commercial and chemical valuation of oils generally.

For this purpose I had first to measure the resistances offered by a column of each of the oils experimented on, having in each case the same length and sectional area.

From the low resistances possessed by the volatile oils, the apparatus used by M. Becquerel for ascertain-

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* Read before the British Association, in Section B.

* "Treatise on Electricity," translated by Walker).

† Since writing this I find the following note in "Paris's Pharmacologia" (1833), under the article "Olive Oil." "M. Rousseau has discovered the curious fact, that of all the oils, both vegetable and animal, olive oil most feebly conducts electricity. It may be stated that as a medium it acts 675 times more feebly than the others. Two drops of oil of beechmast, or of poppy seeds, poured into ten grammes of olive oil, renders the needle four times more sensible. This difference, therefore, furnished M. Rousseau, by means of his diagometer, a test for determining adulterations with precision."—(*Journal de Pharmacie*, t. ix. p. 587.)

ing the resistances of liquids might be employed,* but from the high resistances offered by the fixed oils I have designed a modification.

I must here acknowledge the obligation I am under to W. Hooper, Esq., for the use of Sir William Thomson's delicate astatic reflecting galvanometers, and a battery which possesses remarkable constancy, viz., that of Daniell's as modified by Minotte.

With such a galvanometer as used for these tests, the deflections obtained are strictly proportional to the resistances, and by means of noting the deflection produced through a constant resistance, by a standard current, it is easy to compare the results obtained at different times and under different conditions. The standard current represents a known relation to the full electro-motive power employed.

The resistances of the essential oils were determined with one cell; and I may remark that I was considerably surprised at the low resistances of the volatile oils, this being the reverse of what, judging from the composition of them generally, I was prepared to expect.

The adulterants of the volatile oils are principally turpentine and alcohol.†

Compared with any of the essential oils, turpentine has an immense resistance, whilst that of alcohol is enormously lower than any of them, except perhaps that of oil of bitter almonds, which is so low that I did not measure it.

The importance of this general fact is at once apparent, since the addition either of alcohol or turpentine in the smallest quantity is readily detected; and the quantity denoted by the variation in the deflection, either when compared with a standard of known purity, or by the resistances themselves.

The oils of lemon and bergamott, when mixed with a small proportion of turpentine, do not, however, show such marked differences as the generality of the essential oils. The addition of turpentine reduces the conducting power, or, in other words, increases the resistances to a very perceptible extent in all oils except lemon and bergamott, but in these two last cases it becomes, nevertheless, perceptible in the effects of increased resistance,—in these cases, however, a property not solely confined to turpentine aids in its detection, and consequently enlarges the scope of the application of this test. Large quantities of turpentine are instantly perceptible in increasing the resistances.

The addition of turpentine to oil of lavender is more strongly marked by this test than in any other case.

The following tables contain the averages of six tests on each oil, taken at different times. For reasons noted farther on, the same sample should not be used for a second test. The volatile oils were obtained from reliable sources, and were supplied as perfectly genuine and in mature condition. I am particularly indebted to Messrs. I. and H. Smith for the liberal manner in which they have supplied me with information respecting the samples obtained from them. I met with great difficulty in procuring samples of cotton seed oil, and although my samples are unauthenticated for condition or purity, I must acknowledge my obligation to Mr. Edward Mann, 7, Pall Mall East, for his kindness in procuring them.

TABLE OF RESISTANCES OF VOLATILE OILS.

Genuineness of Samples Authenticated.

Name of Oil.	Observed Deflection.	Ohmad's Resistance.
Peppermint, Ang.	224 × 8·94	800,000*
" "	274 × 8 94	652,160†
Peppermint, German	236 × 8·94	759,000
Carraways	202 × 8·94	90,000
" 2nd sample	202 × 8·94	90,000
Cloves	205 × 8·94	81,000
Bitter Almonds	—	— ‡
Aniseed	57 × 8·94	3',144,000
Bergamott	94 × 8·94	1,906,000§
Lemon	53 × 8·94	3',376,000
Lavender, Ang.	310	5,244,000‖
" Mitcham	250 × 8·94	717,000¶

* 1858 product. † 1861 product. ‡ Beyond range of observation. § Sample turbid. ‖ Deflection rising rapidly to 350. ¶ Deflection increasing slowly. This arises from electrolysis.

ADULTERATED SAMPLES.—VOLATILE OILS.

Name of Oil.	Adulterant.	Observed Deflection.	Ohmad's Resistance.
Peppermint Ang.	Turpentine	185 × 8·94	969,000*
" "	Spirit of wine	43 × 8·94	422,000
Lemon	Turpentine	52	3',444,000
Bergamott	Spirit of wine	157 × 8·94	11,600
"	Turpentine	92 × 8·94	1',949,000
Lavender, Ang.	"	104	15',630,000

* Product of 1858.

The effects produced by mixing different specimens of the same oil together are also perceptible, thus the German oils of peppermint, or foreign samples of lavender oil produce modifications in the electrolysis.

In testing the fixed oils a much higher battery power is required; this arises simply from the fact that they all possess much lower conducting powers than any of the essential oils.

For these tests thirty-two cells were used.

TABLE OF RESISTANCES OF FIXED OILS.

Samples Purchased as Genuine.

Name of Oil.	Observed Deflection.	Ohmad's Resistance.
Olive.	40 × 8·94	554', 637,600
"	68	*3,186',000,000
Sweet Almond	40 × 8·94	554', 637,600
" "	35 " 8·94	†708', 048,000
Castor Oil, Italian	91	2,242',152,000
Castor Oil, E. I. Elect.	206 × 8·94	113', 287,680
Poppy	326 " 8·94	68', 444,640
Turpentine	340	590', 040,000
‡Cotton-seeds	220	§ 14', 000,000
" 2nd sample	130	‖ 23', 500,000

* Bleached. † Bleached. ‡ Rising gradually to 500. § Sensibility of instrument increased. ‖ Rising rapidly beyond range.

From this table it will be seen that the bleached oils have even a lower conducting power than the unbleached oils; and in this respect olive oil possesses a greater difference than almond oil. It is not easy to explain this.

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* See "De la Rive's Treatise," vol. ii.

† The foreign oils are no doubt sometimes entirely substituted for the English oils, or largely diluted with them.

A singular difference exists between the Italian and the East Indian castor oils. This difference will enable one to detect a very small percentage of the one added to the other.

Cotton-seed oil and oil of poppy, as well as turpentine, are so rapidly altered in their conducting power by electrolysis, that there is not the slightest difficulty in recognising them in samples of oil.

Olive oil, when free from cotton-seed oil or oil of poppy, has its resistance increased by electrification, but if the smallest quantity of either of them exists in a sample of olive oil, it produces a contrary effect by a prolonged contact with the battery.

These results of electrolysis are alone important in determining the condition of a sample of olive oil.

I regret that I have not been able to extend these observations to commercial samples of olive oil of different qualities, and to have included a greater number of fixed oils, from the great difficulty of procuring specimens of reliability in purity or condition.

ON THE PRESENCE OF COLUMBITE IN WOLFRAM.

BY T. L. PHIPSON, PH.D., F.C.S.

I HAVE recognized the presence of columbite (niobate of iron and manganese) in a sample of wolfram from Auvergne that I have lately analyzed, which was given to me some years ago by M. Pisani. I had already remarked that wolfram from different localities sometimes contained niobic acid, sometimes tantalic acid, which can be made distinctly evident by examining before the blow-pipe the residue left when most of the iron, manganese, and tungstic acid, have been separated.

From the specimen here in question I succeeded in extracting from some twenty grammes a quantity of columbite sufficient to fill a small bottle, and to enable me to study its properties easily. The separation of this rare mineral is based upon the simple fact that wolfram is attacked by aqua-regia, whilst columbite is not. Fifteen to twenty grammes of wolfram, finely pulverized, are attacked by warm aqua-regia, and when the action has proceeded as far as possible the residue is collected, the tungstic acid separated by a solution of ammonia, and the residue again submitted to the action of aqua-regia. These operations are repeated five or six times, as long as any tungstic acid can be obtained from the residue. Finally, the latter becomes quite black, and then consists almost entirely of the mineral columbite (or niobite) mixed with some grains of transparent quartz.

After ascertaining by analysis the nature of this residue I passed it under the microscope, and saw the mineral in its ordinary aspect. It appeared in the form of angular, irregular, black fragments, more or less metallic, almost vitreous, non-magnetic, resembling up to a certain point brilliant fragments of coal; and giving all the blow-pipe reactions of columbite.

It will be remembered that M. Gustav Rose formerly ascertained that columbite and wolfram are isomorphous.

I may profit by this opportunity to state that the metal columbium, now called *niobium*, was discovered by the English chemist Hatchett in 1801, and that the metal discovered in 1802 by Ekeberg, and called *tantalum*, was really a new metal, and not the columbium of Hatchett, as Dr. Wollaston declared. The latter is niobium, a metal which has become remarkable by the persevering researches of Heinrich Rose, who has made known all its characteristic reactions. On comparing the observations of Hatchett with what is now known of tantalum and niobium, principally by the admirable analytic studies of Heinrich Rose, the fact alluded to becomes, I believe, incontestable.

London, Aug. 31, 1867.

ON A METHOD OF RECOVERING SULPHUR AND OXIDE OF MANGANESE, AS PRACTISED AT DIEUZE, NEAR NANCY, IN FRANCE.*

BY J. LOTHIAN BELL.

IN the manufacture of soda, the use of sulphur plays an important part; the office it performs being to effect, in the form of sulphuric acid, the decomposition of common salt.

The sulphate of soda obtained from this action in its turn is subjected to decomposition by exposure to heat along with carbonate of lime and coal, a process which transfers, practically, almost the whole of the sulphur to the lime, or to the metallic base of this earth, the only exception being the small portion which remains with the soda as an impurity.

This new combination of sulphur is separated from the soda salts by lixiviation, and the portions undissolved, containing the sulphides of lime and calcium, and known as soda-waste, are thrown away.

I will not dwell on the inconvenience the soda-maker is exposed to from having to provide deposit-room for such a large quantity of refuse as the mode of treatment just mentioned gives rise to, nor on the somewhat offensive nature of the soda-waste itself, rendering the alkali manufacturer's heap any thing but a desirable neighbour.

Various are the plans which have been suggested for the recovery of this, the most costly element of the soda-maker's process, but hitherto, so far as actual practice is concerned, the whole of the sulphur employed by them may be said to be still thrown aside after it has once done the duty just alluded to.

Having heard a favourable account of a method in operation, at the Chemical works of the Dieuze Company, near Nancy, by the permission of the proprietors, I visited that establishment in the month of July last, and it is to give a brief account of their process that I now have to ask the attention of this Section.

It will be convenient at this stage of the description to remind you that in most soda-works there exists another residual product, scarcely less embarrassing in its nature than that previously mentioned. The muriatic acid obtained from decomposition of common salt with sulphuric acid, along with peroxide of manganese, is employed very extensively in the manufacture of bleaching powder, a process which gives rise to the generation of a large quantity of liquid chloride of manganese mixed with chloride of iron and free hydrochloric acid. The whole of these solutions are run away into the nearest water-course in the vicinity of the different manufactories.

The process I am about to describe requires the assistance of this second equally valueless material. The operation is as follows:—The soda-waste, after being removed from the vessels in which it has been sepa-

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* Read before the British Association, in Section B.

rated from the soda, is thrown into a tank of stonework about twenty yards long, by five in width, and six feet deep. In this vessel an intimate mixture is easily effected of the soda-waste and the metallic chlorides of the refuse from the bleaching powder process, and from which latter all the free muriatic acid has been removed in a manner to be hereafter described. Were the free acid still present, a loss of sulphur by the generation of sulphuretted hydrogen would take place, and this on all accounts it is obviously best to avoid.

A few hours suffice to convert the chlorides of manganese and of iron into sulphides, when the soluble chloride of calcium generated by the action is allowed to drain off. The solid matter remaining is cast out into a heap by the side of the tank containing it. In two or three days the heap is turned over, and in a short time a considerable elevation of temperature ensues, indicative of strong chemical action. To restrain this somewhat, the mass is kept moist, otherwise spontaneous combustion would ensue, sulphur would be wasted, and the desired results generally interfered with. During this stage of the process the metallic sulphides, under the joint action of the atmosphere and moisture, are peroxidized, and sulphur is separated. The oxides of manganese and iron so obtained are by subsequent turning over brought into contact with other portions of sulphide of calcium of the soda-waste, and are again converted into their respective sulphides, which give up their sulphur a second time in the way already described. The process is continued so long as there remains any sulphide of calcium from which it is sought to separate sulphur in the manner explained. The sulphur thus liberated is taken up by another portion of the sulphide of calcium of the soda-waste, and a polysulphide of calcium results from the combination, which is soluble in water. The formation of polysulphide continues as long as other portions of the original sulphide of calcium go on yielding up their sulphur to generate the sulphides of manganese and iron.* In this way almost the whole of the sulphide of calcium originally contained in the soda-waste is converted into polysulphide of calcium, hyposulphite and oxysulphide of lime, all of which are easily dissolved in water. Something like four or five days are required to effect these changes. These soluble salts are separated from the insoluble portions of the mass exactly in the same way as the ball alkali is treated for obtaining the soda it contains. Vats resembling in construction those of the soda works, and of a capacity equal to the daily production of soda-waste, are placed near the locality in which the preceding stage of the process has been effected, and in a very speedy manner the polysulphide and other salts are run off as a deep orange yellow solution, hereafter denominated the yellow liquor.

The composition of the insoluble portion is as follows:—

$CaOSO_2$	66·248
$CaOCO_2$	1·320
CaO	20·982
Fe_2O_3 and Al_2O_3	7·000
MnO	1.500
Insoluble	2·800
	99·850

As a refuse this substance will be recognised as being of an inoffensive character to the neighbourhood so far as any subsequent chemical action is concerned. Unlike the original soda waste it contains no ingredient liable to oxidation; it cannot therefore give off any of those unpleasant compounds which more or less are to be found in the vicinity of all alkali works. It is, moreover, not unreasonable to expect that a matter consisting chiefly of $CaOSO_2$ and CaO may be found useful as a stimulant to various descriptions of soils, and thus the whole of the S used in the soda process in one shape or another may be rendered useful instead of being a nuisance, as is the case at present.

The residual products from the bleaching powder works are received into a tank so built that free hydrochloric acid does not destroy the structure. By this means any insoluble portions are separated, and the clear liquid is run off into an adjoining cistern.

To this acid solution of chloride of manganese and iron, is added that of the polysulphide of calcium and lime compounds of sulphur, obtained in the manner previously given. The presence of free hydrochloric acid causes an immediate precipitation of all the sulphur, free from the sulphide of calcium, and the accompanying substances containing sulphur; and the addition of the yellow liquor is continued until the first appearance of a black colour, indicative of all the free acid being neutralized, and the first portion of iron from the chlorine residuum commencing to be precipitated. The precipitated sulphur is removed from the liquid, and the greater portion of the accompanying water is separated by pressure. After this the remainder of the moisture is expelled by a very low heat, and the sulphur is then employed for producing sulphuric acid.

It is obvious from what has preceded that the chlorides of manganese and iron must be left in the solution from which the free hydrochloric acid has just been removed in precipitating the sulphur; and it is in this way that the neutral solutions of these metals are obtained which are required for operating upon subsequent portions of the soda-waste.

This process has been in operation at Dieuze for some months, and at the present moment by its means about one and a half tons of sulphur are being recovered daily. It will be seen that no new material is required, the only ingredients being the two waste products from the manufactory itself. The apparatus employed is of a most simple character, consisting almost entirely of tanks on which the expense for maintenance will be a mere trifle—in fact the whole cost is one of labour, which at the Dieuze works amounts to something like 40s. per ton on the sulphur obtained.

Supposing 40 per cent. of the sulphur used in this kingdom to be thus recovered, the annual saving this process is capable of effecting will amount to a considerable sum.

Instead of employing the "yellow liquor" and the chloride of manganese in the way set forth in this paper, an attempt has been made at Dieuze to employ both as a means of recovering manganese, a desideratum with bleaching powder makers as eagerly sought for as the regenerating of sulphur has been with the soda manufacturer. I shall with your permission proceed to describe the process which the owners of the establishment assured me promises to be a success.

The acid solutions from the bleaching powder works, being all required in order to precipitate sulphur by means of the free hydrochloric acid, contain a consider-

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* Along with this formation of polysulphide of calcium. there goes on at the same time a generation of hyposulphite and oxysulphides of lime due to the liberation of oxygen from the metallic oxides at the moment of being again converted in sulphides.

able quantity of neutral chlorides of manganese, and will remain on hand. To such portions of these neutral chlorides as are not used in the sulphur process itself, yellow liquor is added in a suitable tank so long as a black precipitate falls, which is variable in quantity with the varying composition of the manganese used. The black precipitate consists of sulphide of iron and free sulphur, which can be collected and burnt in an ordinary furnace for burning pyrites.

The iron being thus all separated from the metallic solution, a fresh quantity of yellow liquor is added, by which all the manganese is thrown down, the precipitate consisting of some free sulphur and sulphide of manganese.

The sulphide of manganese is burnt in the same way as that of iron, but the residue, instead of being all oxide, as is the case when the sulphide of iron is under treatment, is composed of protoxide and binoxide of the metal mixed with a certain quantity of sulphate of manganese. The oxides are separated in the usual way by water, and being almost chemically pure, are of great value to the glass-makers, to whom the presence of the iron usually found associated with the manganese of commerce, is a subject of great inconvenience.

The sulphate of manganese as a concentrated solution is added to nitrate of soda in quantities denoted by the equivalents. When heated, decomposition takes place; nitrous acid is given off, which may be used in the sulphuric acid chambers, or otherwise disposed of; and the residue consists of sulphate of soda and the protoxide and binoxide of manganese, which latter represent, so far as available oxygen is concerned, a manganese amounting to 65 to 70 per cent. of binoxide. The oxides of manganese are separated from the sulphate of soda in the usual way by washing with water, and both used for any purpose to which these two substances are commonly applied.

I may add that these operations have been carefully examined by some of the leading men of science of France, both in their practical and scientific relations, and that in the recent adjudication of prizes at the International Exhibition, at Paris, the inventors had a gold medal awarded for the service they are considered to have rendered to the industry of their country.

THE PHYSIOLOGICAL ACTION OF THE METHYL COMPOUNDS.*

BY DR. B. W. RICHARDSON, F.R.S.

The author opened by briefly recapitulating the work of the previous reports, and by noting several facts showing in a satisfactory manner the practical good that had followed their publication. Passing to the methyl compounds, the substances now to be considered, he said they were of unusual interest, inasmuch as the poisonous gas known as fire-damp, and the beneficial agent chloroform, were included in the group. He then divided the substances to be described, in regard to their physiological action, into two distinct groups, with their chemical constitutions or characters—giving first in detail a list of the methyl series as follows:—

Methylic alcohol.
Hydride of methyl; marsh gas; fire-damp.
Chloride of methyl.
Iodide of methyl.
Bromide of methyl.
Acetate of methyl.
Methylic ether.
Nitrite of methyl.
Nitrate of methyl.

The methylene series was given as follows:—

Chloroform (Terchloride). Tetrachloride of carbon.
Bichloride of methyl.

Dr. Richardson next discussed the action of the above substances in detail. The following is an abstract of his observations:—With regard to alcoholic fluids, he observed that the physiological law was that the period of time required by these bodies to produce their effects, and the period of time required for recovery, turned altogether on the boiling point of the fluid used. This was so certain that when the boiling point of one fluid and its action were known, the action of other fluids might be predicted from their boiling point. The explanation was simple. The alcohols taken into the body did not enter into any combination which changed their composition, but passed out of the body, chemically, as they entered it, and their evolution, and the time of their evolution, was the mere matter of so much expenditure of force, caloric, to raise them and carry them off. He had tested this, and found that intoxicated animals recovered more or less quickly according to the temperature in which they were placed —those in the higher degree returning the sooner to their normal condition. The practical lessons were, that in alcoholic poisoning of the human subject the most important condition for recovery was a high temperature; and that as methylic alcohol was more rapid in its action and much less prolonged in its effects than common alcohol, it could be used with great advantage by the physician in all cases where he felt an instant demand for alcohol. All alcoholic bodies are depressants, and although at first, by their calling injuriously into play the natural force, they seem to excite, and are therefore called stimulants, they themselves supply no force at any time, but take up force, by which means they lead to exhaustion and paralysis of power. In other words, the calorific force which should be expended on the nutrition and sensation of the body is expended on alcohol. Dr. Richardson added to his recommendation of methylic alcohol as a medicine the facts that when quite pure it was very palatable, and that it mixed easily with either hot or cold water.

The Hydride of Methyl.—The Hydride of Methyl occurs naturally in the form of fire-damp in mines, and as marsh gas on land. It is made artificially by heating together in a strong flask acetate of soda, caustic potash, and well dried lime. For physiological experiments the Hydride of Methyl can only be administered by inhalation. It is a pleasant gas to inhale, producing no irritation nor yet giving rise to any of those feelings of excitement which are induced by nitrous oxide gas or the vapour of chloroform. It might therefore be ranked, as Mr. Nunneley had long ago ranked it, as an anæsthetic; but, as its effect was evanescent, and the quantity of gas required to produce an effect was very great, it was practically valueless for this purpose. Dr. Richardson then proceeded:—As this gas is often a cause of death in mines, I thought it was worth inquiry —What percentage of it would prove fatal in the air? I therefore had constructed a glass chamber, through which an atmosphere charged with various quantities of the gas could be passed. To my surprise, I found that even pigeons—animals peculiarly susceptible to the influence of narcotic gases—could live in an air charged with not less than thirty-five per cent. of the gas, for the space of half an hour; while I could myself inhale the air coming from their chamber with the utmost ease. When at last by pushing the gas further,

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* Read in Section D, British Association.

death is induced—it comes as a very gentle sleep, so gentle, indeed, that it is difficult to say when the action, either of the circulation or of the respiration, is over. The lungs are left with the blood in them, the heart has blood on both sides, and the blood itself retains its natural character. The death is by the slow negation of breathing. We may gather from these facts many important lessons in regard to the risks and dangers of miners from fire-damp. I should think it is almost impossible that any body of men, or any men who were awake in a mine, could be so entrapped with fire-damp only as to die in the absence of an explosion. In accidents where this seems to have occurred, I should imagine that with the fire-damp there is also evolved carbonic acid gas. I can, however, imagine after an explosion, when the mine becomes for a moment a great vacuum, that there would be sufficient entrance of the gas to produce a fatal atmosphere. In such case death would be prolonged, but as easy as sleep; two truths, which in cases of accident should inspire thankfulness and hope—thankfulness that those who thus die for us suffer little; hope as to the possibility of rescue, which should not for days be abandoned. The best direct means of recovery of those under the influence of fire-damp is exposure to heated air, and the administration of warm nourishing drinks, such as milk. Alcoholics do decided harm. From this point the author proceeded with a description of the action of chloride of methyl, the iodide, bromide, and acetate, methylic ether, nitrite of methyl, and the nitrate, over which we must pass, to record his more general researches on chloroform and its allies.

Methylene Compounds.—Dr. Richardson spoke at length on these compounds as anæsthetics, describing the nature and action of chloroform, tetrachloride of carbon, and bichloride of methylene (with which he put a pigeon to sleep under a glass shade). He had been led to the conviction that the cause of death from chloroform was in every case due to the arrest of nervous function, and that the idea of any direct action of the agent on the muscular structure of the heart was without foundation. He had conducted eighty-seven experiments specially to determine the direct influence of chloroform on the heart, and found in every case that organ capable of reaction on its exposure to air, while the lungs were always bloodless, white, and collapsed. The best means of restoration in impending death from chloroform was the introduction into the lungs, by artificial respiration, of air heated to 130° Fahrenheit. For doing so a small pair of handbellows connected with a thin tube of platinum in a coil was found to answer well, as with a spirit lamp the coil could be instantly made hot. It was only necessary to inject the air through one nostril. The tetrachloride of carbon had recently been brought into use as a substitute for chloroform. With regard to it Dr. Richardson said—As this substance is now gaining importance, I have thought it proper to subject it to very careful experiment, and I feel it my duty to state, both on theoretical and practical grounds, that it is far more dangerous than chloroform, and if it were as generally used it would act fatally in a much larger number of cases. In its action it presents the same four stages as chloroform, but the second stage is more prolonged and intensified. In one animal, a rabbit, tetanic convulsion of an extreme character was presented during this stage. But the worst feature in the administration of this body is the slowness of its elimination—a slowness fully accounted for by the boiling point. Saturating the nervous centres, and expending their force to the fullest, it kills far more quickly and determinately than chloroform, and so completely is motion paralyzed that the muscles scarcely respond to galvanism five minutes after dissolution. In order to make an exact comparison, and it is from this comparison I draw the results arrived at—I placed animals of the same kind, at the same time, at the same temperature, in chambers of the same size, and administered the same doses of chloroform and of the tetrachloride of carbon. Pigeons and rabbits alike gave evidence of the more severe effects of the latter substance. In this opinion my friend Dr. Sedgwick, who has rendered me valued aid in these inquiries, entirely coincides.

The Bichloride of Methylene.—The last compound on our list is of great interest, from the circumstance that it promises to be a new and valuable anæsthetic. In experimenting with chloride of methyl in ether, I was so struck with its good action that I asked Mr. Robbins, the chemist who had prepared the compounds for me, to endeavour to find, from the methyl bodies, a more stable compound, having similar physical properties. In a few days he brought me the fluid I now place before the Section, made for him by Dr. Versmann. This fluid is the bichloride of methylene. It is formed by the action of sulphuric acid on zinc in chloroform, and it differs from chloroform in that one atom of chlorine is replaced by an atom of hydrogen. Its boiling point is 88° Fahr., and the odour of its vapour is sweet, and much like that of chloroform. On testing it physiologically, I found it to be a gentle and perfect general anæsthetic. Under its influence animals lapse into the third stage of anæsthesia with the slightest exhibition of the stage of excitements. The insensibility is deep and well sustained, and the recovery quiet and good. (Dr. Richardson here showed the experiment already mentioned of putting a pigeon to sleep.) In some experiments, in order to see the extreme effect, I have carried the administration to the extent of arresting the phenomena of life. I have thus learned that the respiration and circulation under the last action of this agent cease simultaneously, and that the muscles retain their irritability for even an hour after death. The lungs are left with blood in the respiratory circuit, both sides of the heart are charged with blood, and the blood itself remains unaltered in physical property. Compared with other anæsthetics, the bichloride of methylene appears to me to combine the anæsthetic power of chloroform with the safer properties of ether. It is too early to speak positively on this point, but if the expectation be fulfilled, the perfection of a general anæsthetic will have been obtained for the benefit of the world. And, even should this happy result not be accomplished, the way at least is paved towards the discovery of some intermediate body which shall answer to the required physical demand. In reviewing the facts connected with the physiological action of the methyl series, we gather that, according to their composition, they exert certain definite influences on different parts of the nervous organism. The oxide produces an influence specifically its own, that of slowly paralysing the motor function without destroying common sensibility. The nitrite and nitrate rapidly paralyse the centres of motion, while the chloride, and the iodide, together with the substitution chlorine compounds, not only paralyse motion but also destroy sensation. I conclude this report with one other observation. At first sight it may seem that the isolation of the phenomena produced by special agencies, and the discovery of a new anæsthetic are sufficient characteristics of this research. With every respect, I submit that a broader question is involved. At the meeting at Birmingham I suggested, almost with a feeling of fear, that out of these studies might spring up a fixed principle of therapeutic discovery. Now, I have the conscious happiness of knowing that the hypothesis was correct. I feel convinced by this longer experience that by continued labour we shall be able to pronounce the precise physiological meaning and value of all the organic compounds, to extend the knowledge of curative action of these compounds to every condition of disease that is physically remediable, and to bring positive Science of Therapeutics to a position that shall stand out as a leading fact in the scientific advancement which the British Association so fervently encourages, and which at once solidifies and beautifies the progress of the present age.

ON A REAL IMAGE STEREOSCOPE.*

BY J. CLARK MAXWELL, F.R.S.

In all stereoscopes there is an optical arrangement by which the right eye sees an image of one picture, and the left eye that of another. These images ought to be apparently in the same place and at the distance of most distinct vision. In ordinary stereoscopes these images are virtual, and the observer has to place his eyes near two apertures, and he sees the united images, as it were, behind the optical apparatus.

In the stereoscope which I have had made by Messrs. Elliott Brothers the observer stands at a short distance from the apparatus, and looks with both eyes at a large lens, and the image appears a real object close to the lens.

The stereoscope consists of a board about two feet long, on which is placed—1. A vertical frame to hold the pair of pictures, which may be an ordinary stereoscopic slide turned upside down. 2. A sliding piece near the middle of the board, containing two lenses of six feet in length, placed side by side, with their centres about one inch and a quarter apart. 3. A frame containing a large lens of about eight inches focal length and three inches diameter.

The observer stands with his eyes about 2 feet from the large lens. With his right eye he sees the real image of the left-hand picture formed by the left-hand lens in the air close to the large lens; and with the left eye he sees the real imagine of the other picture formed by the other lens in the same place. The united images look like a real object in the air close to larger lens. This image may be magnified or diminished at pleasure, by sliding the piece containing the two lenses nearer to or farther from the picture.

ON THE ANALYSIS OF CAST-IRON.

BY EDMUND G. TOSH, PH.D.

(Continued from page 172, American Reprint, Oct., 1867.)

Estimation of Graphite. 2 to 3 grammes of iron were treated with dilute hydrochloric acid, and when solution approached completion a considerable quantity of strong acid was added to separate the last portions of iron and manganese. The insoluble matter, consisting mostly of graphite, was collected on a carefully weighed filter, washed with hot water, dilute hydrochloric acid, solution of caustic soda, and hot water again, successively, lastly with alcohol and ether to remove oily hydrocarbons as recommended by Max Buchner.* By washing with dilute acid and with alkali, iron and silica or oxide of silicium were separated. After drying at 120° C. the filter and graphite are weighed and burned away. The small residue (a mere trace of silica or titanic acid) is weighed, and this weight subtracted from the first gives the amount of graphite. The results obtained agree very closely, as shown in the two following cases:—

1. 2·36725 grms. iron gave 0·08375 grm. insoluble matter, containing 0·013 grm. incombustible residue. Graphite per cent. =2·978.

2. 2·991 grms. iron gave 0·1095 grm. insoluble matter containing 0·01725 grm. incombustible residue. Graphite per cent. =3·0842.

In washing the graphite with solution of soda, there was always a brisk effervescence, due according to Schafhäutl † to the oxidation of oxide of silicium to silicic acid, by decomposition of water, with consequent liberation of hydrogen.

The difference between the quantities of graphite and of total carbon, is the amount of carbon in the combined state.

Estimation of Silicium. About 2 grammes of the iron were dissolved in aqua regia, and evaporated to dryness. The insoluble matter was collected, washed, dried, ignited, and cautiously deflagrated with nitrate of potash. Graphite is thus quickly burned away: the fused mass is extracted from the crucible by means of water. The alkaline silicates in solution are decomposed by hydrochloric acid in considerable excess, and the whole evaporated to dryness. To the dried mass a few drops of hydrochloric acid are added, and afterwards water. The insoluble silica is filtered off, washed, dried, and ignited till quite white. From its weight the proportion of silicium in the iron may be deduced.

Estimation of Sulphur. About 3 grammes of the iron were dissolved in nitric acid, with the occasional addition of a few drops of hydrochloric acid, and evaporated to dryness. The residue is dissolved in the least possible quantity of hydrochloric acid, and water added. The sulphuric acid in the clear, filtered solution is precipitated by chloride of barium. After standing 24 hours, the sulphate of baryta is collected. It is generally contaminated with a little iron which may be removed by treatment with dilute hydrochloric acid.

Estimation of Phosphorus. 3 grammes of the iron are dissolved in aqua regia, the solution evaporated to dryness, and the insoluble matter filtered off. The perchloride of iron solution is reduced to the state of protochloride, by heating with sulphite of soda. Although perfectly reduced, the solution still retains a yellow colour due to dissolved organic matter. All excess of sulphurous acid is boiled off, a little perchloride of iron is added, and the solution cautiously neutralised by means of carbonate of soda or ammonia, till the precipitate formed does not dissolve again. This small portion of peroxide of iron containing all the phosphoric acid, is filtered off, washed, redissolved in a little hydrochloric acid, and the phosphoric acid precipitated as ammoniophosphate of magnesia, the iron being held up in the ammoniacal solution by citric acid.

Estimation of Manganese.—3 to 4 grammes of

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* Read before the British Association, in Section A.

* Journ. f. prakt. Chem. Bd. 72, p. 364.

† Journ. f. prakt. Chem. lxvii. p. 257.

iron are dissolved in aqua regia; the solution is largely diluted and filtered, and neutralised with carbonate of soda or ammonia till of a deep brown colour. The iron is precipitated by acetate of soda, and the solution immediately boiled. The large precipitate settles quickly, the clear liquid is poured off and filtered. After three washings by subsidence and decantation, the precipitate is thrown on a large filter and again washed. The bulky solution containing all the manganese is evaporated to small volume and refiltered. The manganese is first precipitated by sulphide of ammonium, the sulphide collected and washed with sulphide of ammonium water, redissolved in hydrochloric acid, the solution boiled, and the manganese reprecipitated as carbonate by carbonate of soda, filtered off, washed, dried, and ignited till of constant weight, showing its perfect conversion into Mn_3O_4.

Titanium.—The estimation of this element in any substance is somewhat uncertain, and its determination in pig iron can scarcely be accurately accomplished. Its detection in iron is not difficult. A considerable quantity (5 grms.) of the specimen is treated with dilute hydrochloric acid, and insoluble collected, graphite burned off, and the residue freed from silica without loss of titanic acid, by heating with a mixture of hydrofluoric and sulphuric acids as recommended by Riley.* After driving off sulphuric acid, titanic acid is left behind, which may be distinguished by the violet reaction it gives with microcosmic salt and a little tin in the reducing flame of the blowpipe.

It may be approximately estimated in the following way. About 6 grammes of iron are dissolved in hydrochloric acid, and the whole evaporated to dryness. The dried mass is moistened with hydrochloric acid, water added, and the solution filtered. Part of the titanium exists in the solution (a) and part in the insoluble (b). The solution, if containing much perchloride of iron, is reduced by sulphite of soda, the excess of sulphurous acid boiled off, a little perchloride of iron added, and the titanic acid precipitated in combination with the sesquioxide of iron thus introduced, by means of carbonate of soda, as in the estimation of phosphorus. The small precipitate is quickly filtered off, washed, dried, ignited, and carefully set aside.

From the insoluble matter (b) graphite is burned off, and the silica is removed by hydrofluoric acid in the presence of sulphuric acid. To the residue after this treatment the small ferruginous precipitate from (a) is added, and the whole fused with bisulphate of potash. When cool the fused mass is extracted with cold water, and from the clear filtered solution, titanic acid and iron are precipitated by ammonia; the precipitate is slightly washed, and sulphide of ammonium added. The sulphide of iron thus formed is dissolved by sulphurous acid, while the titanic acid mixed with sulphur is undissolved, and may be collected, ignited, and weighed, after which it should be tested as to its purity.

Nitrogen was estimated by the following process of Boussingault.† 5 grammes of the iron under examination are slowly dissolved in very dilute hydrochloric acid. By this means a part of the nitrogen is converted into ammonia, and exists in the solution as chloride of ammonium, and another portion remains in the insoluble, probably combined with the titanium. This insoluble is collected, and the filtered solution is treated in a capacious flask with a considerable excess of caustic lime and boiled. A tube connected with the flask by a tight cork, dips into dilute hydrochloric acid, by which the liberated ammonia is absorbed. Nitrogen is estimated in the graphitic matter insoluble in acid by burning with soda-lime, the ammonia formed being also absorbed by dilute hydrochloric acid. Both solutions are now brought together, evaporated to small bulk, chloride of platinum added, the small precipitate collected, washed with alcohol, dried, ignited, and the remaining platinum weighed, from which the amount of nitrogen may be calculated.

Nickel, cobalt, and copper were carefully sought for by operating on large quantities of the various specimens of iron, but evidence of their presence could not be obtained. 63 grammes of iron were deflagrated with a mixture of carbonate of soda and nitrate of potash, the contents of the crucible dissolved out with hot water, and the highly alkaline solution filtered off from the large quantity of sesquioxide of iron. This solution was first neutralised with hydrochloric acid, chloride of ammonium, ammonia, and sulphate of magnesia added, and any phosphoric and arsenic acids allowed to precipitate during 24 hours. The precipitate was collected, washed with ammonia water, dissolved in acid, the solution heated, and sulphuretted hydrogen passed through it. A very small light yellow precipitate formed, which proved to be sulphide of arsenic.

In all determinations of iron, where practicable, I have used the volumetric method by means of bichromate of potash, first proposed by my former professor, Dr. Penny, of Glasgow, which is I think preferable to that of Margueritte, where permanganate of potash is employed. Bichromate is much more stable than permanganate, and the strength of the solution only requires to be determined once. Besides, in the estimation of iron by Penny's method, there is no fear of the evolution of chlorine, which has to be so carefully guarded against in Margueritte's process.

THE ELECTRIC INDUCTION OF MR. HOOPER'S INSULATED WIRES, COMPARED WITH GUTTA-PERCHA INSULATED WIRES, FOR TELEGRAPH CABLES.*

BY WILLIAM HOOPER.

The author referred to the relation existing between the different properties of insulated wires arising from induction. He showed by an extensive series of experiments that an intimate connection exists between the effects of electrification and electrostatic induction, and that the penetration of electricity into the substance of an insulator, when measured by the residual discharge, is a function of the electro-static capacity, and not simply of resistance. He has also shown that the effects of electrification are increased nearly in the same proportion as the interior inductive action is reduced.

The results render it extremely probable that in rapid signalling through long circuits, as by "waves," the rate of transmission attainable is not increased or diminished in the simple proportion of the electrostatic capacities, but in a ratio compounded of it and the interior resistance to inductive action.

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* Journ. Chem. Soc. Vol. p. 311.

† Comptes Rendus, T. lii. p. 1088.

* Read before the British Association, in Section A.

This is a matter of serious consideration, for the interior induction, unlike the surface induction, is not reduced by an increased thickness of insulator, which points strongly to the practical advantage derivable from Mr. Hooper's dielectric over gutta-percha in every respect for induction. These results are verified by Mr. Latimer Clark, Mr. Varley, Professor Sir W. Thomson, and Mr. Fleeming Jenkin.

So much superior is Hooper's dielectric to gutta-percha for insulation, that, taking the core of the electric telegraph cable connecting Ceylon with India as an illustration, at 75° Fahrenheit, the temperature at which cores are tested, it would require a core nearly 2 feet diameter of gutta-percha to equal ·38 inch diameter of Mr. Hooper's insulator (that is, 200 lbs. of Hooper's insulator is equal to 576,000 lbs. of gutta-percha), the same conductor being used in each, to obtain the same degrees of insulation.

ANTISEPTIC PROPERTIES OF THE SULPHITES.*

Dr. Richardson read a paper by Dr. Polli, in Section D, "On the Antiseptic Properties of the Sulphites." He stated that he was afraid he might not make a good representative of his learned friend, Dr. Polli of Milan. However, as he had long communicated with Dr. Polli on the subject, and knew well what that gentleman meant, he (Dr. Richardson) had chosen, instead of trying to present the paper as a whole, rather to give the facts presented by him as the results of extended observations. Sulphurous acid was said to be the most active agent in preventing or arresting all organic fermentation. As the acid, however, was not sufficiently applicable in experiment, Dr. Polli had undertaken an investigation as to the action of the sulphites of lime, hyposulphite of magnesia, sulphite of magnesia, sulphide of soda, and granulated sulphite. These substances were found to possess all the properties of sulphurous acid, with the advantage that their action was more uniform and certain and constant. In experimenting on animals and himself, he found that large doses could be taken without risk. On killing animals treated with sulphites, and others not so treated, he found that the former were most slow to decompose, and, indeed, remained quite fresh when the others were putrescent and offensive. Another series of experiments showed that in one class the administration of the sulphites, was sufficient to effect a more or less rapid cure in cases where blood-poisoning was present, as in fevers. Dr. Richardson distinctly mentioned, however, that Dr. Polli was anxious to have it clearly stated that he did not attribute this to any curative power in the sulphites, but to the fact that they arrested decomposition, and by so doing allowed the animal to recover by the recuperative power existing in its own constitution. The author thought his observations conclusive as to the excellent influence of the sulphites on the septic diseases, and remarked that it was for the purpose of thus benefiting others that he had brought his researches under the attention of the scientific world. Dr. Richardson laid some of the sulphites before the Department, and mentioned that he would be glad to let physiologists have four or five ounces of any of them for the purpose of experimenting, and that physicians might also receive a small quantity for hospital practice.

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* Read before the British Association.

ON A NEW FORM OF DYNAMO-MAGNETIC MACHINE.*

BY W. LADD.

It is now thirty-six years since Faraday first published his celebrated "Researches in Electricity and Magnetism;" the foundation then laid has been receiving additional strength as the superstructure has progressed. Faraday has gone to his rest, but the name he always tried to hide behind his philosophy will shine brighter and brighter, until the top stone is raised in future ages. The machine I am about to describe is a part of that superstructure. The repeated application to me for a machine that would give a sufficient light for the purposes of lecture demonstration and would dispense with the galvanic battery, has induced me to give considerable attention to the subject, and I must leave you to judge how far this machine meets not only that requirement, but also that of lighthouse illumination.

Perhaps the most powerful magneto-electric machine was that constructed by Mr. Wilde, the electro-magnet receiving its charge from sixteen permanent steel magnets, but Siemens and Wheatstone have shown that the residual magnetism left in soft iron, after being under the influence of a battery, or permanent steel magnets, can be augmented from the currents generated by itself, by merely applying dynamic force to the revolving armature, containing a coil of copper wire, the terminals of which are connected with the wire surrounding the electro-magnet; and although great effects were produced *in* the electro-magnet, the current itself could only be made available by its partial or total disruption—in the former case diminishing the power of the electro-magnet, and in the latter reducing it to its normal condition. But in the machine I have constructed, the power of the electro-magnet is kept up, whilst a separate current, to be applied to any useful purpose, can be drawn off by means of an independent arrangement.

It consists chiefly of two plates of iron; to both ends of each plate is fixed a portion of a hollow cylinder; these plates are then placed a certain distance apart, and insulated from each other in such a manner that the cylindrical pieces will form two hollow circular passages; into these spaces two armatures (known as Siemen's armatures) are placed. The plates are surrounded by a quantity of stout copper wire, connected together, the two terminals of which are brought into connection with the commutator of the smaller armature, so that each change of polarity in the armature will augment the magnetism. When the machine is first made it is only requisite to pass a current from a small cell of Smee's or any other element, for an instant, to give the iron a polarity; it will then retain a sufficient amount of magnetism for all future work.

If the armature in connection with the electro-magnet is made to rotate, there will be a very feeble current generated in it; this passing round the electro-magnet, will increase its power with every additional impulse. It will thus be seen that the only limit to the power of the machine is the rapidity with which the armature is made to rotate, which is entirely dependent on the amount of dynamic force employed. But the great improvement in this machine is the introduction of the second armature, which, although it takes off very powerful currents, generated in its wire by the increas-

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* Read before the British Association, in Section A.

[English Edition, Vol. XVI., No. 408, pages 169, 170.]

ed magnetism, does not at all interfere with the primary current of the electro-magnet.

The machine now at the Paris Exhibition measures about 24 in. in length, 12 in. in width, and stands 7 in. high; but this being imperfectly constructed as to its proportions, the results obtained are, no doubt, much less than they would be with a properly constructed machine. Still, I found it would keep 50 in. of platinum wire, ·10 in. diameter, incandescent, and when a small voltameter was placed in circuit with the second armature it would give off 250 cubic centimetres of gas per minute, and in connection with an electric regulator would give a light equal to about thirty-five Grove's or Bunsen's elements, the driving power expended being less than one horse.

I have now to describe a machine on the same principle as that just noticed, but which, instead of having two independent armatures running in separate grooves has two armatures fixed end to end, so as to appear like one continuous armature, but so placed with reference to each other that their magnetic axes shall be at right angles. By this arrangment there is only one opening required for the armature, enabling us to take full advantage of the horse-shoe form of electro-magnet. The shoes of the electro-magnet and armatures are so proportioned to each other that there is an actual break in the magnetic circuit with reference to each armature alternately, but by their disposition at right angles there never is an actual break in the complete magnetic circuit, but simply a shifting of the principal portion of the magnetic force from one armature to the other at the precise moment required to produce the best effect. The mechanical advantages obtained by this disposition of parts must be at once obvious, as one pair of bearings and set of driving gear is dispensed with, and from the fixing of the two armatures together the currents are made to flow perfectly isochronously. It may be found of advantage to vary the angle of position of the armatures with reference to each other, according to the speed at which they are driven, so that the current given off by the exciting armature may at the precise moment exert its full effect upon the electro-magnet, and thus produce the best effect in the second armature.

ON THE COMMERCIAL ANALYSIS OF SOME OF THE PRODUCTS AND MATERIALS OF THE ALKALI MANUFACTURE, Etc.

BY C. R. A. WRIGHT, B.SC., F.C.S.

[Continued from page 226, American Reprint, Nov., 1867.]

III. **Soda Ash.**—The commercial valuation of soda-ash is usually restricted to the determination of the percentage of "available alkali" contained therein, by this term being meant the total Na_2O contained in a state capable of saturating a strong acid, as sulphuric; and hence including hydrate, carbonate, aluminate, silicate, sulphide, sulphite, and hyposulphite. The analysis is usually performed by adding the standard acid to the hot aqueous solution of an known weight of ash until a slight acid reaction is obtained; by this means all calcium contained, and Al_2O_3 contained as aluminate, are estimated as though they were soda. Practically this error is of slight importance; it may be readily avoided by addition of a very slight excess of acid along with some litmus tincture, then adding a slight excess of standard sodium carbonate solution, boiling and filtering from the precipitated lake and $CaCO_3$: the excess of soda added is now again determined by the standard acid, and thus the exact amount of acid used to saturate the Na_2O present in the "available" state is known. An abuse, however, that has long been practised in the soda trade in connection with this is the following:—The equivalent of sodium is considered to be 24 (instead of 23·04—Stas). Hence by varying the mode of calculation, a varying error is introduced, the available alkali being always represented as more than it really is. If sodium be thus calculated, the error is $+\frac{0{\cdot}96}{24}$ or 4·0 parts in 100: if Na_2O be calculated, the error is $\frac{64-62{\cdot}08}{64}$ or 3·0 parts in 100: while if Na_2CO_3 be calculated, it is $\frac{108-106{\cdot}08}{108}$ or 1·8 parts in 100. Hence according to the plan employed in determining the standard of the acid used, according as soda salts are thus used, or other substances, an error of from 0·9 to 2·0 per cent. is introduced in the valuation of a 50 per cent ash. Hence arises the custom in many alkali works of invoicing the sales of ash at from 1 to 2 per cent. higher than their real strength, it being known that the purchaser will accept the analytical certificate calculated on this erroneous basis. This practice, which is neither more nor less than a barefaced fraud, is by no means universal, nor is it known to many of the purchasers of soda ash; in certain districts, however, it prevails, and is a constant source of vexatious complaint whenever the purchaser happens to employ on his own side a more conscientious analyst. The writer has known soda ash of identical quality invoiced part to one customer as containing 48 per cent., part to another as 49 per cent., and part to a third as 50 per cent., the actual percentage being 48½; the separate consignments being reported also in these different strengths by the analysts in the different towns to which the goods were sent; inasmuch as soda ash is usually valued as so much per cent. per cwt.,—this amounts to a direct fraud on the purchaser. It sometimes happens that an analyst, known to object to this system, finds that his connection for the analysis of soda ash becomes *nil*, being transferred to some less scrupulous rival.

When the exact composition of a sample of soda ash is required, the following method may be adopted.

(*a*). A known weight is heated to 150°—200°C., and the loss of weight considered to be moisture.

(*b*). The residue of (*a*) treated with hydrochloric acid leaves sand and insoluble matters, and in the filtrate the SO_4 may be estimated volumetrically, or better gravimetrically, by barium chloride.

(*c*). The CO_3 present is estimated in Mohr's apparatus, or in Fresenius' and Wills', with the addition of some potassium chromate.

(*d*). A known weight is treated with water, and the solution evaporated to dryness with hydrochloric acid; thus the SiO_3 is determined; the filtrate from this with ammonia throw down alumina, from which the Al_2O_4 (as aluminate) is known.

(*e*). The insoluble residue of (*d*) with hydrochloric acid and ammonia, gives the Fe_2O_3 and Al_2O_3 (not as aluminate), the filtrate from this with ammonium oxalate gives the calcium (usually only traces).

(*f*). A known weight is dissolved in nitric acid, and the Cl estimated by a standard silver solution.

(*g*). A known weight dissolved in water is oxidized by chlorine, and the sulphate thus formed determined; a known weight is dissolved in water, and the solution divided into two equal parts; in one the iodine required to yield a blue colour when starch and acetic acid are added, is determined, to the other zinc sulphate is added, and in the filtrate the iodine required after removal of sulphide is again determined; from these data on the sulphide, sulphite, and hyposulphite are calculable.

(*h*). The total "available alkali" is determined, the error due to the alumina of the aluminate being eliminated as previously mentioned; subtracting from this calculated as sodium, the sodium corresponding to the $SiO_3Al_2O_4$, sulphide, sulphite, hyposulphite, and CO_3 found, the difference is calculated as hydrate; this may be checked by adding barium chloride to a known weight, aud determining the amount of acid required to neutralise the filtrate: rather more hydrate is usually indicated by this mode than that really present, owing to the presence of a portion of aluminate, hyposulphite, etc., incompletely thrown down by the barium salt.

Carefully executed analyses according to this method have yielded the writer results adding up to 99·8—100·1.

When ferrocyanide is present, it may be estimated by dissolving a known weight of ash in hydrochloric acid, and adding ferric chloride; after standing some time, the precipitated Prussian blue may be well washed, treated with pure potash, and the ferrocyanide determined in the solution by permanganate.

(IV.) **Bleaching Powder.**—The commercial estimation of bleaching powder only extends to the estimation of the hypochlorite contained therein; the result being, however, calculated as so much per cent. of "available chlorine." Of the numerous methods proposed for the determination of the hypochlorite, the one usually employed in the trade is that depending on the amount of ferrous salt oxidised by a given weight of bleaching powder. It frequently happens, however, that instead of a perfectly pure ferrous salt (such as the ammonio-sulphate $FeSO_4 + (NH_4)_2SO_4 + 6H_2O$ precipitated by alcohol), the ordinary "protosulphate of iron" of the druggists is used, discoloured crystals being of course rejected; rarely does this substance contain 100 per cent. of the compound $FeSO_4 + 7H_2O$, and hence errors frequently are introduced, less chlorine being required to peroxidise a given weight of impure than of pure substance. Again, some analysts neglect to add an acid to the ferrous solution used, and hence the precipitated ferric hydrate is liable to carry down perceptible quantities of ferrous hydrate, again making the apparent amount of chlorine required less than that really requisite. When acid is added, an error is liable to be introduced by the peroxidation of part of the iron by chlorine compounds derived from chlorate that may be present; direct experiments have shown the writer that acid ferrous solutions are perceptibly oxidised by the presence of chlorate in small quantities in the course of a very few minutes, even at the ordinary temperature, although the peroxidation due to the whole of the chlorate is not manifest, until after standing some time at 20°C., or till after heating to ebullition. Lastly, the equivalent of chlorine is frequently taken to be 36 instead of 35·46 (Stas). All these sources of error tend to make the percentage of chlorine found higher than that really present; accordingly it frequently happens that analyses of the same sample by different analysts differ by 1, 2, or 3 per cent. of available chlorine; this error becomes of serious importance, it frequently happening that the analysts employed by the seller and purchaser differ in their reports, thus causing much annoyance, and possibly the rejection of the goods as not being of contract strength.

As regards the error introduced by the presence of chlorate in the sample analysed, many careful experiments on the subject have yielded the following results to the writer:—

1. Acid ferrous solutions are peroxidised by addition of a chlorate, at a rate depending on the strength of the solutions, the amount of free acid, and the temperature, the reaction taking place completely after heating to ebullition for a minute, and almost as completely after standing for upwards of half an hour at 20°C., time being, however, required for any temperature short of ebullition.
2. Acid solutions of As_2O_3, where a large excess of free acid is present, are scarcely affected by chlorate at 20°C., until after standing some hours; the reaction ensues completely on heating to ebullition for a minute, and completely in a few minutes' heating on a water bath.
3. Alkaline solutions of As_2O_3 (containing $NaHCO_3$ and free carbonic acid) are wholly unaffected by chlorate, either cold or boiling, even after several hours.
4. Acid solutions of potassium iodide (free from iodate). Iodine begins to separate even at 20°C. in a very few moments on addition of very little chlorate, and after some time much separates. Heated to 100° on the water bath, the whole of the chlorate becomes completely decomposed, after five minutes, in presence of sufficient free acid.
5. Alkaline solutions of potassium iodide are unaffected by chlorates even on long standing or long boiling.

PARIS EXHIBITION OF 1867.

(From our Special Correspondent.)

Doubtless the majority of your readers imagine (if they ever took the trouble to think of the matter at all, which is not very likely) that the paths of a "special correspondent" are strewn with flowers. What has he to do but to examine, compare, and write the results in as decent English as he can command? This is very true; but has it never struck you, "gentle reader," that there is, to put it mildly, a tinge of sameness about all this examining, comparing, and writing? Your correspondent would not have alluded to the subject were it not that he has fallen a victim to another set of "*concessionnaires.*" As long as one could take a chair outside the building and refresh oneself with a cup of coffee after the fatigues necessarily involved in the study of the productions of rival blacking and soda-water manufacturers, one's task was, although laborious, not without its redeeming points; but now that the chairs are removed, you must no longer expect the same regularity from your correspondent.

Our French friends here are in high glee about the Newton-Pascal affair. It will, indeed, need overwhelming evidence to disprove the mass of forged documents upon which the advocates of the Pascal theory rely. Unfortunately, the evil does not rest here. In this city there are a number of clever and not too scrupulous men of letters who delight beyond measure in mystifications of the Newton-Pascal type. Any great event in scientific, political, or artistic history is at once seized upon as a theme, and on it are erected a variety of literary edifices which, if not sound, are at least showy and attractive. To prove this it is only necessary to allude to the mass of forged letters of Marie Antoinette, upon which such opposite theories have been built. The affair of the necklace, the Man in the Iron Mask, Count Cagliostro, the Count de Saint Germain, Madame de Brinvilliers,—I could for a thousand francs obtain documents to prove any theory connected with these highly promising subjects for the skill and inventiveness of literary charlatans. At a discussion the other day upon the affair of Newton, a French literary celebrity, pressed rather hard on the question of these forgeries, stated, with the entire concurrence of the Gallic element among his hearers, that France would soon prove to the world that *all* the greatest discoveries and the grandest ideas were French. "Your English writers, what are they but imitators of us? You have invented the word "adapted" to conceal your robberies from our dramatists, and now, not satisfied with "adapting" our dramas, you "adapt" our discoveries. Your Swift and your Sternes that you pride yourselves upon so much, have they not robbed from Rabelais? They should be kicked, especially your Sternes." The roar of laughter that arose from the English portion of his hearers so disconcerted the worthy professor, that "the sitting was suspended" for some minutes; but he enjoyed the explanation which followed so thoroughly that harmony was immediately restored.

To return to our work. Messrs. Burgoyne, Burbidges, and Squire, of Coleman-street, London, have a very handsome and well-arranged collection of drugs and chemicals. They are of very fine quality. They exhibit oil of bitter almonds both in the raw state and as freed from hydrocyanic acid. Their oils of cloves, carraway, pimento, nutmegs, cubebs, pepper, etc., are apparently as good as they can be. They, like the Messrs. Howard's, exhibit a fine specimen of benzoic acid; it is in a large globe, and has a very good effect. It is not made by direct sublimation, but by boiling the powdered gum benjamin with lime and water as long as any acid remains to be extracted. The solution is then precipitated with hydrochloric acid, and the resulting benzoic acid, after being separated by filtration from the solution of chloride of calcium, is dried and sublimed. They also exhibit white crystallised benzoic acid, prepared entirely by the wet process. This kind of acid is used in Germany, and is manufactured entirely for that market. Their cyanide of potassium, prepared by Liebig's process, looks well, and is stated by the makers to contain a very high percentage of pure cyanide. There is a sample of pure nitrate of barium, which they are now manufacturing in large quantities at the price of from £35 to £40 per ton. The chloride of barium is also manufactured by this firm in a pure state at £30 the ton. They also exhibit nitrate and carbonate of bismuth perfectly free from arsenic; and potassio-tartrate of antimony in crystals and in powder. The cubebin shown is prepared from the residue left in the still after distilling oil of cubebs. There are in this case two large specimens, about sixteen inches in diameter, of piperin and caffein. Piperin forms a very easily-broken crystalline mass, and the most extreme care had to be taken to get this fine preparation safely to the Exhibition. It was suspended by India-rubber springs so as to avoid concussion, and the arrangement was then hung in gimbals like a ship's compass. Owing to this careful packing it arrived safely, and without the mass losing a single crystal.

When we think how many fine specimens, not only chemicals, but also works of art, were destroyed in transit (some by careless packing, and still more by the brutally rough usage the packages containing them received), we cannot help expressing the hope that the skill and care shown in this matter by Messrs. Burgoyne and Co. will be imitated in future by some of those exhibitors whose despair, on unpacking their cases, was so ludicrously displayed.

The very complete collection of the scale preparations of iron exhibited in this case have suffered from the prolonged exposure to light which they have necessarily undergone, and have thus lost that beautiful colour and brilliant lustre which they possessed when they first arrived.

Among the other objects interesting to Pharmaceutists, are a collection of gelatine capsules containing balsam of copaiba, castor oil, oil of male fern, and sundry other unsavoury drugs. The exhibitors were induced to undertake the manufacture of these articles owing to the low character of a vast number of the capsuled preparations found in commerce. It is well known to those behind the scenes in these matters that, owing to the viscidity of the balsam of copaiba and oil of male fern, it is a common practice among unscrupulous makers to thin them down with linseed oil in order to facilitate the process of filling the capsules. The dilution of the active ingredients of the capsules which thus takes place is often so great as to render them entirely valueless as medicines.

Messrs. Burgoyne's case is one of the handsomest and best arranged in the English chemical department of the Exhibition, and, if it contains no great novelties, at least represents in a highly creditable manner the present state of English Pharmaceutical manufactures.

Messrs. Davy, Yates, and Routledge, of Upper Thames Street, London, have also an excellent display of drugs and chemicals. Their specimen of corrosive sublimate in a dome is adapted to show the fracture, and looks well. They also show calomel in a crystalline form as prepared by the old dry sublimation process, and also as sublimed in presence of steam. The first kind becomes discoloured on exposure to light, and sometimes retains traces of corrosive sublimate; the second is free from either of these defects. The ammonio-citrate of bismuth is a comparatively new scale preparation. It is in the form of micaceous scales, containing sixty per cent. of oxide of bismuth. The fact of its ready solubility in water without decomposition, and its being "compatible" with the alkalies and their carbonates, has made this salt a great favourite with many practitioners; and, indeed, in some stomach complaints this and other preparations of bismuth appear to act almost like a charm.

FOREIGN SCIENCE.

(From our own Correspondent.)

Paris, Sept. 3, 1867.

Scientific Association at Cherbourg.—Electricity of Connecting Straps in Machinery.—The Triangulation of Prussia.

The meetings of the Scientific Association at Cherbourg were terminated on the 24th of August. Nothing was neglected to increase the interest of this re-union of savants and amateurs. Visits were made to the soda and iodine works of M. Cournerie, to the military port, where the *Dunderberg*, now christened the *Rochambeau*, and where the spur of the *Atalante* is being cast; lectures were delivered on astronomy and meteorology, at which the professors attended, furnished with instruments, due to the munificence of the government; and there were also reports, memoirs, notices, and discussions on different points of the natural, physical, and moral sciences.

M. Quesnault, sub prefect of Valognes, presented a memoir in which he demonstrated that the British Isles and the small archipelago existing on the north-west coasts of France, from the Cape la Hague to Cancale point, or rather, as far as St. Malo, formed part of the Continent, and laid down a map on which all the vestiges of terrestrial vegetation existing, or supposed to exist, under the sea were shown.

M. Lenoir, director of the telegraph at Saint-Lo, called the attention of the assembly to the electricity of connecting-straps in machinery, and the dangers which might result from it in powder works.

A fortuitous circumstance added considerably to the interest attached to this meeting. Lady Franklin, widow of the celebrated explorer of the northern regions, assisted at the meetings of the 23rd and 24th ult. The noble lady had arrived on the 22nd by the American frigate the *Minnesota*, accompanied by Miss Grinnell, a young lady, the daughter of a shipowner of New York, who had fitted out at his expense two ships for the research of Sir John Franklin. At the last meeting it so happened that M. Lambert gave a description of his project of a voyage to the North Pole. This *rencontre* of Lady Franklin and M. Lambert is described as presenting a most impressive scene to the audience. Lady Franklin was to leave on the 27th on board the pleasure yacht *Leda*.

The first volume of the report on the triangulation of Prussia had scarcely been published when it was attacked on all sides. It comprised the triangles measured in the eastern portion of the kingdom. The first attack was made by Lieut.-General de Baeyer, the former collaborator of de Bessel, who repulsed with much vivacity a criticism made upon certain triangles of de Bissel, contained in the preface of the work in question; he showed, on the same occasion, that the results obtained were in all respects inferior to those of the ancient triangulation of de Bessel, and those of the triangulation of the coasts of Prussia. He stated that, in 1863, the Prussian Government had confided to M. de Baeyer the direction of the survey, and that at that period he had already disapproved of many things in the work which had been submitted to him. The Ordnance department took no notice of his protestations, and published the work in question without even informing M. de Baeyer, who was much surprised to find all the faults pointed out by him, and as many others as they had time to add. He has hastened to disavow this publication in a letter addressed to the *Astronomische Nachrichten*. Lieutenant-Colonel de Hesse, Chief of the Ordnance Department, replied, and undertakes to justify himself, but M. Peters has taken part himself against the department. Moreover, M. Wittstein, Professor of Mathematics at Hanover, has published two articles successively in the same journal, in which he endeavours to prove that the Department of Triangulation does not know what a personal error is, and that it does not know how to *compose* the bearings observed at the same station. M. Wittstein concludes that all the calculations of the triangulation must be made over again.

M. Pisko, professor of physics at the Lyceum of Wieden, at Vienna, has related to us a curious accident which he witnessed, and which is highly interesting to physiologists. The servant of the laboratory of the Lyceum is an old corporal of the *gendarmes*, of a strong constitution and always in excellent health. In the month of February last year he was occupied in cleaning an induction apparatus, and he conceived the idea of trying it with several elements. When he had laid hold of the two handles he could not let them go. Fearing that his imprudence should be discovered he would not cry out for help though he was groaning with pain. He remained in this situation for more than ten minutes, and we cannot tell what would have happened if he had not fallen to the ground and in his fall broken the conducting wire. After some time he recovered the use of his limbs and continued his usual occupation. The following day he felt some uncomfortable and strange symptoms; when walking he fancied everything he walked upon was spherical. The next day, about 11 o'clock, these sensations became stronger, both his arms were swelled from the elbow to the fingers, and the legs from the knee to the extremity of the toes; the patient had to keep his bed. When he tried to get up it seemed as if he could not touch the ground, the swelling and the pain attaining its maximum about 2 o'clock and disappearing about 4 o'clock. A doctor was called in, but the man at first concealed the nature of his malady; he said he had stirred the acid of the battery with his hand. Warm baths ordered by the doctor produced no effect. It was only on the fifth day that M. Pisko, absent till then, was informed of the state of his assistant. M. Pisko went to see him, and on his saying he did not believe the story of the acid, the man confessed what had happened. The Professor then proposed to the doctor who attended the patient to use the same remedy as that applied in cases of lightning stroke, viz., quinine and old wine. This treatment turned out to be efficacious, and by the end of fifteen days the periodical symptoms had gradually disappeared, without leaving a trace. Nevertheless, in the month of February last, just a year after the accident, the same symptoms were renewed, though with less intensity. Treated with the same remedy as before they yielded at the end of eight days. It will be curious to see if the symptoms return in February, 1868.

The Imperial School of Pharmacie has just lost one of its most experienced savants, in the person of M. Guibourt, honorary professor. He was born in Paris, in 1790, and was, at sixteen years old, on the termination of his classical studies, apprenticed to the Boûdet pharmacy. He was author of several pharmaceutical works of the highest merit. By these and his constant studies he acquired, justly, the name of being the most distinguished savant in medical and pharmaceutical materia. Named Member of the Academy of Medicine in 1824, and Professor of the School of Pharmacy in 1832, he was also received into many national and foreign learned societies.

F. Moigno.

Paris, Sept. 18, 1867.

Anti-incrustator for Steam Boilers.—Gifford's Monster Balloon. —Prize Subjects of the Haarlem Society of Sciences.—Fluorine Compounds, Isolation of Fluorine.—Potato Disease. —Preservation of Anatomical Specimens.—Report on Unity of Weights and Measures.

M. E. Schmitz exhibits an anti-incrustator for a steam boiler, composed of small surfaced curved blades, placed in contact one with the other in the same manner as curved tiles on the ridge of a house roof, in such a way that their *ensemble* forms a sort of double bottom in the boiler and in the generators. This double bottom only leaves a thin layer of water, of unequal thickness, absorbing the caloric, in consequence, under unequal conditions. The result is that the liquid particles are put rapidly in motion according to the difference of densities produced by the heat. The direction of the motion is transversal, and the circulation is caused to move upwards; on one side the steam, as soon as it is formed, while on the opposite side the water, less hot, descends to be vaporized in its turn. This circulation, which is very rapid and continuous, produces effects which annihilate completely the two causes of destruction.

The velocity of the liquid current is propagated throughout its entire mass, and determines a sort of molecular rolling which tends to take up the heat transmitted by the heated surface of the boiler, as quickly as it is formed. The liquid mass thus becomes the regulator of the heating of the metallic envelope which contains it, and, consequently, diminishes the causes of unequal dilations. By its rapidity, the current does not allow any matter to adhere to the surface of the boiler, and carries all impurities to the surface of the water, where the steam is separated without perturbation. Thus the deposit takes place on the inner surface of the double bottom, and as there is always a current of water in contact with the bottom of the boiler, it can never get red hot. By the anti-incrustator of M. Schmidt the risk of explosion is considerably reduced.

The great news of the day is the inauguration on the 7th inst. of the anchored balloon of M. Henry Giffard, the celebrated inventor of the injector for steam engines. He has spent more than £4,000 upon the realisation of the greatest experiment of modern times. Having rented a plot of ground adjoining the extensive engine factory and machine works of M. Henry Flaud, he has erected an immense cylindrical screen of canvas fixed upon vertical poles. In this he has constructed a balloon 69 feet in diameter, holding 210,000 cubic feet of gas, formed of two webs of closely woven linen, cemented together by several layers of American black india-rubber varnish, the whole being covered with drying oil so as to prevent any of the effects of osmose or diffusion. Two series of gigantic apparatus have been constructed on the same spot, for the production of pure hydrogen. The first is composed of 100 barrels, each containing 155lbs. of dilute sulphuric acid, with a large quantity of iron turnings capable of furnishing, each, 350 to 400 cubic metres of gas. The second apparatus is a steam generator, by aid of which the steam is decomposed,

[English Edition, Vol. XVI., No. 406, page 134, and No. 407, pages 153, 154.]

by passing over red-hot charcoal or incandescent coke, into pure hydrogen and carbonic acid gas; the hydrogen is separated from the mixture by the aid of quick lime, which absorbs the carbonic acid gas and leaves the hydrogen pure, dry, and cool, to be conducted by a main pipe to the upper part of the balloon. With this second series the hydrogen only costs two-pence per mètre cube (or about 4s. 9d. per 1,000ft.), but the preparations are not quite completed; in a trial on the 9th inst. the balloon was inflated with hydrogen resulting from the action of the sulphuric acid, and the operation was finished in 8 hours, whereas the filling of the balloon with gas procured by the decomposition of water took 48 hours. The former process gave also 3,500 cubic feet of mother-water of sulphate of iron, collected in a vast subterranean basin, which can be sold to be evaporated by chemical manufacturers, and which are sufficient to disinfect the cesspools of a whole quarter of Paris. Inflated on Saturday, the balloon had lost almost nothing of its gas on Monday, and on Thursday the 12th, when we visited it, the total loss of hydrogen was only 2,100 cubic feet, or a hundredth part of the total volume of gas with which the balloon had been inflated. The osmose or the diffusion is really prevented. The closing of the upper and lower valves, ingenious beyond description, is absolutely hermetical. We need scarcely remind our readers that the cable, 984 feet long, by which the balloon is attached to the earth, is coiled and uncoiled by two different steam engines, which the mechanician can stop or set at work at will by means of cocks which serve for the distribution of the steam.

The inflation being terminated, the balloon, containing 210,000 cubic feet of hydrogen gas, was retained by the ballast; at each of the 70 ropes of the group were attached ten sand bags weighing each 33lbs.; and in spite of this weight of 22,100lbs. the car was more than 3 feet from the ground, so great was the ascensional force. A rather strong wind, that may be estimated at 33 feet per second, was then blowing, but it did not prevent the balloon from rising in a vertical direction. These experiments, suspended for some days, were to have been renewed on Saturday last. This organisation of a view of Paris from a height of 984 feet reflects great credit on M. Giffard.

The Dutch Society of Sciences of Haarlem proposes for public competition the following subjects, the essays on which are to be deposited before the 1st January, 1869:—1. Profound researches on the nature of the infecting principle of the contagious typhus of cattle, indicating at the same time the prophylactic methods, the employment of which proceeds rationally from the result of these investigations. On account of the great importance attached to the solution of this first question by the Society, an extraordinary premium of five hundred florins will be added to the gold medal. 2. Detailed examination of the different substances composing the liquid produce of the dry distillation of coal. 3. The experiments of Mr. Tyndall have demonstrated that the intensity of sound differs considerably according as it is propagated in hydrogen or in atmospheric air, even when the densities of the two gases are equal; the Society demands, on this subject, comparative experiments made with at least three different simple gases. 4. To decide experimentally if the radicular extremities of plants exude matters capable of dissolving the silicic acid which is found in the ground in the shape of quartz. 5. New researches on the mutual decomposition of saline solutions containing different bases and acids which decide between the doctrine of affinities and that of Bergmann. 6. Ulterior exact researches on the remarkable phenomena of dissociation discovered by M. Sainte-Claire-Deville.

The chemical event of the last month has been the forwarding by M. Dumas, to the Academy of Sciences, of the researches of Mr. Prat on the chemical constitution of fluorine compounds and the separation of the fluorine. Mr. Prat started from this fact that the fluorides are really oxyfluorides; that the fluoride of calcium, for example, is formed of two equivalents of calcium, one of oxygen, one of fluorine, and that, in consequence, the true equivalent of fluorine is 29·5, and not 19, and that, in order to obtain it, all that is necessary is to treat the fluoride of calcium with chlorate of potassium, or, what is better, perchlorate of potassium, for it is only with this last salt that the reaction takes place. Oxygen is disengaged and a gas is produced which silver absorbs, giving rise to a fluoride of silver, insoluble in water, soluble in ammonia, from which it is precipitated by nitric acid, and which is altered by the action of light more rapidly than the chloride of silver; the formula of the real chloride is $AgFl$, whilst that of the soluble fluoride of chemists is $AgFl,AgO$. Fluorine is obtained by heating, in a platinum retort, fluoride of lead of the chemists one part, either with nitre five parts, or with binoxide of manganese two parts. Oxygen gas and fluorine are disengaged, the oxygen is taken up in its passage by fragments of heated baryta. Fluorine is gaseous, nearly colourless, possessing an odour like chlorine, very visibly giving off fumes in the air, incombustible, and heavier than air; it dissolves indigo, reddens and discolours litmus paper, disengages fumes in contact with ammonia, decomposes water at the ordinary temperature, combines with hydrogen under the influence of diffused light, and eliminates bromine and iodine from their compounds; it unites with boron, silicium, and all the metals of the first five groups.

MM. Juette and Ponteves have succeeded in preparing tartaric acid from the skins of grapes, after they have been pressed and distilled, and can be put to no use but as manure. After distillation the skins are treated with water so as to obtain lees, to which is added 2 per cent. of sulphuric acid, and the mixture is boiled for some hours. The tartaric acid in combination is set at liberty, and, moreover, not only the sugar escaped from the fermentation is not eliminated, but the action of the sulphuric acid on the cellulose of the pulp of the grape forms a certain quantity of glucose. The liquor is allowed to ferment, and a supplementary distillation gives again an appreciable quantity of alcohol. When the decantation has been made, lime-wash is added, and tartrate of lime is produced, from which the tartaric acid is extracted by the ordinary method. According to the inventors the quantity of grape skins left after 1 million hectolitres of wine, treated by this process, would give 200,000 kilogrammes of tartaric acid the value of which is about 600,000 francs. (£24,000)

The Marquis of Havrincourt has addressed to the *Courier de Pas de Calais* the following letter: "M. Georges Ville, by following his very ingenious method of examining the vegetation of plants themselves, has just discovered the cause of the potato disease. The cryptogamia are the result and not the cause of the malady. Let any one go to the experimental grounds of Vincennes and he will be convinced as I have been. He will see there a plot of potatoes divided into five parts touching each other: the first is luxuriant and has not a sick leaf, the second is attacked by the malady; the third is as the first; the fourth is as diseased as the second; and the fifth resembles the first and third. Thus M. Georges Ville produces or avoids the potato disease at will."

The following is the process of M. Von Vetter for the preservation of anatomical specimens:—Add to 7 parts of glycerine at 22° one part of raw brown sugar, and half a part of nitre till a slight deposit is formed at the bottom of the vessel. The portion required to be preserved is then plunged, dried or not dried, and it is left in the mixture for a time proportional to its dimensions; a hand, for example, should remain eight days in the liquid; when it is taken out it is as stiff as a piece of wood, but if it be suspended in a dry and warm place the muscles and articulation recover their suppleness.

M. Jacobi has just issued his report on the Unity of Weights and Measures, drawn up in the name of the Commission of Moneys, etc., of the Exhibition.

On summing up, the commission think that the governments ought to keep the following objects in view:—The substitution, as soon as possible, but integrally, of the metric system as is practised in the west of Europe and in many other countries, in place of the old systems of weights and measures. This system introduced at once, and rendered legal but optional, cannot be rendered obligatory at first. The period of toleration varies with the state of the different people, their degree of instruction, etc., and it can only be determined by the governments. It has been remarked, however, that a too long delay does not perceptibly aid the governments in the accomplishment of their task. At all events, it is desirable that governments should take, henceforth, some necessary measures, which are—first, to order the study of the metric system in all schools, and to require a knowledge of it in public examinations; secondly, to introduce its exclusive use in scientific publications, public statistics, in post-offices, custom-houses, public works, and any other branches of the administration that the governments may deem convenient. The commission does not consider that it is part of its mission to occupy itself with the making of standards, exact copies of the prototypes of Paris, the possession of which, in a practical point of view, is the indispensable preliminary of every metrical reform. The administration of each country will appreciate the degree of exactitude suitable to the different destinations of these standards.

F. Moigno.

REPORTS OF SOCIETIES.

MANCHESTER LITERARY AND PHILOSOPHICAL SOCIETY.

MICROSCOPICAL AND NATURAL HISTORY SECTION.

July 18th, 1867.

J. B. Dancer, *F.R.A.S.*, *President of the Section, in the Chair.*

"*Some Further Observations on the Cause of Rotation in the Cells of Vallisneria,*" by James G. Lynde, F.G.S., F.R.M.S.

In a paper read by me at a meeting of the Section on the 16th February, 1863, "On the Action of Magenta Dye upon Vegetable Tissue," I described a series of experiments upon cuttings of Vallisneria, made chiefly with a view to ascertain, if possible, the cause of the rotation of the chlorophyl vesicles within the cells.

I then concluded that the rotation was due to the action of cilia on the inner surface of the cell wall, and was confirmed in this opinion not only by the appearance of the luminous stratum or so-called ciliary wave which had been observed by Dr. Branson, Mr. Wenham, and others, but also by the appearance on the cell walls of certain markings revealed by the action of the dye on the suspension of the vital action.

The above experiment was exhibited to the Section, and many of the members present attributed these markings, as I had done, to the presence of cilia.

I have since, from time to time, pursued my experiments on the subject, in the hope that I might be able to adduce more positive evidence as to the cause of the wave of light on the interior of the cell wall.

After many fruitless experiments I at length determined to try the effect of polarized light, and on the application of it with a ⅛th-inch objective, having an aperture of 130° to 162°, and so arranging Darker's series of selenite plates as to give a dark blue ground, there appeared over the surface of the surface of all the cells brilliant gold-coloured scintillations which had all the appearance of cilia in motion.

The portion of leaf under examination exhibited very sluggish circulation, and was therefore in a very favourable state for the observation.

I have since repeated the experiment several times, and have never failed witnessing the same appearance.

Notwithstanding all that I have seen I cannot say that I am convinced the appearances can be attributed to nothing else but cilia; it is possible they may be due to the presence of active corpuscles, as suggested by Mr. Wenham in his paper on the leaf cells of Anacharis alsinastrum published in the *Microscopical Journal* for 1855, which corpuscles may be Vibrionia or Zooglæ, described by Dr. Cohn in his "Researches on the Development of the Microscopic Algæ and Fungi," as representing the developmental condition of a plant, but it is only by further research that this point can be definitely settled.

The result of my observations so far appears to be that in addition to the wave of light already seen, the separate objects causing that wave may now be observed in the manner I describe; what these objects are is still a matter to be determined, but at present I am inclined to believe them to be cilia on the cell wall, while at the same time there are also independent moving corpuscles within the cell; some of these bodies have the appearance of crystals, and in one specimen I observed a great number of starch granules in the cells.

In investigating this subject the smallest step in advance cannot but be deemed of importance, and I trust that in giving the results of my observations as they occur, I may be the means of saving time and trouble to others who may be investigating the same class of objects, and of inducing them to follow up so interesting a subject, as it is only by many and independent observations the truth can be ascertained.

For the information of members I may state in detail the method of observation I have found most successful.

The microscope I made use of is one of Smith and Beck's largest size, binocular, with one of Beck's most recent ⅛th-inch objectives.

The illumination was by means of an Argand gas burner, the light passing into a right-angled prism below the stage in a line with the axis of the object glass.

Immediately beneath the stage was the achromatic condenser, used both with and without the central stop in the diaphragm, the object being seen equally well in both ways with different effects of light.

Below the achromatic condenser was fixed Darker's series of selenites, and below this the polarizing prism, the analysing prism being inserted immediately above the objective.

I made use of the low Huyghenian eye-pieces and used the microscope either as a binocular or single tube, the field being well illuminated in each case.

I need not add that the most careful centering and adjustment are essential.

In preparing the object I made a section of a small portion of the leaf laid on a piece of cork in water under a simple microscope, separating only one layer of cells; I laid this on a slip made of thin covering glass, and over it applied a thin glass cover, a small feeding bottle and thread being made use of to prevent the object being dried by evaporation.

I then covered the stage and object with a piece of black velvet, to prevent interference from other lights in the room.

BRITISH MEDICAL ASSOCIATION.

Twenty-Fifth Annual Meeting, 1867, *held in Dublin.*

In a paper entitled, "*Mode of Detecting Impurities in Tetrachloride of Carbon,*" read in the Midwifery Section, Dr. Protheroe Smith observed, that previous to the publication of his account of this anæsthetic in the numbers of the *Lancet* of last June, there were few if any pure specimens of the tetrachloride to be obtained. To this circumstance he attributes the contradictory conclusions which in some instances had been arrived at by those who had experimented with the tetrachloride. After studying concisely its physical and chemical properties, Dr. Protheroe Smith remarked that the chief cause of the failures above mentioned were the three following impurities:—

I. *Bisulphide of Carbon.*—This is easily detected by evaporating over a spirit-lamp a portion of the suspected fluid in a deep cup, when, if it contains bisulphide, a slightly bluish flame will appear, whereas if free from this impurity it would be entirely uninflammable.

II. *Free Sulphur.*—Should such exist, after spontaneous evaporation of some of the tetrachloride on a watch-glass, a fine opaque film will remain, which when heated would give off the well-known fumes of sulphurous acid.

III. A peculiar sulphur-compound, which is discovered by dipping in the suspected fluid some clean blotting-paper, which when dry will give a peculiar unpleasant smell of dirty linen.

Dr. Protheroe Smith also exhibited his inhaler. It consists of a graduated glass receiver for the anæsthetic, with a tube in its centre, by which at every inspiration air passes first through the fluid, and then through the sponge, thus becoming so highly charged with its vapour as very rapidly to induce anæsthesia.

This mode of administering anæsthetics effects a salvage of from $\frac{3}{4}$ to $\frac{7}{8}$ of the fluid, so that a drachm may go as far as an ounce when employed as ordinarily on a handkerchief. Dr. Protheroe Smith entered more fully into the comparative merits of tetrachloride of carbon and chloroform in an animated discussion on the action of anæsthetics which took place in the Midwifery Section on Friday afternoon, when Sir James Simpson called upon him to give his experience. Some of the advantages claimed for tetrachloride of carbon were that its administration is rarely followed by sickness or other derangement of health,—that it does not seem to interrupt the natural efforts of labour as often observant with chloroform,—that its effect upon the perceptive faculties very rapidly ceases,—that it can be made at much less cost than any other anæsthetic,—that a smaller quantity suffices for use, and for many other medical reasons which were given.

Dr. Smith's tetrachloride is evidently made by passing the vapour of bisulphide of carbon and chlorine through a red-hot tube. There is no doubt that this compound can be procured very cheaply by this method, and that ultimately it will be used in the arts; much of the so-called tetrachloride, however, now met with is procured by the action of chlorine upon chloroform, and is frequently a mixture of chloroform, other chlorides of carbon, and the tetrachloride.

ACADEMY OF SCIENCES.

Aug. 20, 1867.

(From our own Correspondent).

Shooting Stars.—Animal Electricity.—The Pascal-Newton Forgeries.—Photographic Registration of the Beatings of the Heart.

The correspondence was without interest.

M. Coste presented, on the part of MM. Coulvier-Gravier and Chapelas, the result of their observations on the shooting stars during the nights of the 9th, 10th, and 11th of August of this year. They showed, by a tabular statement, that from the 5th of August, the mean hourly number at midnight on a clear sky, that is to say, corrected for the lunar light and the presence of clouds, was 16·2 stars; this became 33·7 on the 9th, 49·9 on the 10th, and 28·7 on the 11th; giving an average of 37·4. Comparing this with the year 1848, which had given for the mean hourly number, 110 meteors, it is plain that the quantity diminishes very sensibly.

M. Saigey, formerly collaborator of M. Coulvier-Gravier, has just published the results of his meteoric observations made in 1845 and 1849 for all the clear nights.

M. Schultzstein, of Berlin, read a paper on animal electricity, stating that all the phenomena are reduced to simple voltaic currents in which salt acts the part of the electromotor.

M. Chasles took up the discussion relative to Pascal's and Newton's letters. He is astonished that their authenticity has been doubted, inasmuch as they are exchanged between twelve different persons. The writing of Montesquieu is well known; and there are letters from Marriotte, whose writing can be compared with that of manuscripts in the library, etc. There are about 500 letters and notes of Pascal, 200 of Newton, and 300 of Labruyère in the possession of Pascal.

M. Le Verrier declared his incompetence as a member of the commission to inquire into the documents of M. Chasles, and regretted that he could not assist this gentleman in any way in the absence of proofs; and in polite terms demanded the usual proofs of authenticity required by astronomers, so that they could judge when they received them.

M. Chasles replied in warm terms, and a rather stormy discussion ensued. He said that the number and nature of the documents communicated ought to satisfy all doubts as to their authenticity! He added that he was going to publish the most curious letters — for example, those of Molière to Rotrou, and Rotrou to Pocquelin, poetry, unpublished, and other pieces by Rotrou, also letters, etc., from Corneille to Rotrou.

M. Chevreul stated to M. Le Verrier his acknowledgment of the inutility of a commission appointed with too much precipitation.

M. De Landolle proposed in the name of several foreign botanists a change in the nomenclature of botanical classification.

M. Ozonam presented a note on an apparatus by which the beatings of the heart are registered and photographed. They are made to act on the surface of a bent tube containing mercury, the fluctuations of which are noted in the same manner as those of the thermometer and barometer are photographed. He exhibited several curves obtained by this means, and some magnified by the megascope.

Aug. 26, 1867.

Death of Dr. Velpeau.—Fluorine.—M. Chasles' Manuscripts.

A gloom was cast over the assembly by the death of the celebrated Dr. Velpeau, born on 18th May, 1785, at the village of La Briche, in Touraine. The son of a blacksmith and farrier, whom he aided in his trade, he became acquainted with the first notions of veterinary surgery. In 1816 we find him at Tours, a medical student; and in 1820 at Paris, where he was made doctor three years later. Laborious and tenacious, he rapidly amassed money, especially as his private practice was confined to the higher aristocracy. As hospital surgeon of the Pitié, as professor, as academician, he led the example of punctuality, assiduity, and practical skill in conducting operations, so that he became at once one of the best surgeons of France. In 1842 he was elected to the vacant chair at the Institute, having been previously, in 1835, nominated, though he had for a fellow competitor the celebrated Lisfranc. His titles were: Surgeon to the Charité, Member of the Institute, and of the Academy of Medicine, Commander of the Legion of Honour, Professor of Medicine, and Consulting Surgeon to the Emperor. The numerous works written by him are not so easily enumerated; we have before us the list of 18, most of which are in several volumes and accompanied with atlases and magnificent engravings; without counting all the valuable papers read at various societies, they prove that he wielded the pen as steadily as the bistoury, or the hammer on the anvil, in his early days.

Surgical and medical science have lost latterly many in their front ranks; Malgaigne, Jobert, Civiale, Trousseau, Charrière, and last, Velpeau. Funeral discourses were pronounced by Nelaton, Riche, Gosselin, Husson, Guyon, and Longett; the latter was pupil and friend of the deceased.

Among the correspondence, which was opened by M. Chevreul, was an important one on fluorine, by M. Pratt. According to this chemist we have been hitherto mistaken upon the composition of fluorides, and on the theory of fluorine. M. Pratt proposes new formulæ, which harmonise better with known analyses. These will be examined in detail when the *Comptes Rendus* appear.

M. Balard presented a note on chloride of ethylene.

M. Blanchard reminded the assembly that at the last meetings of the Academy doubts were expressed upon the existence of unpublished manuscripts of Pascal, which would contain important discoveries. He thinks that these doubts ought to be satisfied by a declaration contained in a passage of the preface of the "Treatise on the Equilibrium of Heavy Liquids," published in French. M. Blanchard states that this preface was probably written by Madame Perier. The edition in the hands of M. Blanchard is dated 1698, and is conformable to those of 1663 and 1664.

M. Faugère having been invited by the president to state his objections against the authenticity of the documents published by M. Chasles, stated that the falsificator had not even well *imitated* the handwriting of Pascal. He said that the forger was merely satisfied in adopting the writing and style of language of the 17th century.

M. Faugère confined his observations to mentioning an anachronism, according to him, most evident. There is a question, in one of the letters, on the froth of coffee; now, the use of coffee was not introduced into France before the end of the year 1669. He contested that Newton never wrote in French, also he remarked upon the common-place style of the letters attributed to Pascal, whose diction was far different, and added that the forger was caught in his own net. He hoped that the imposition upon M. Chasles would be shortly cleared up.

M. Chasles replied that all these doubts have not shaken his confidence in the authenticity of his documents, so numerous and varied.

M. Regnault observed that photography would furnish the means of recognising whether the old papers contained anterior writing that may have been made to disappear. Also there are chemical reagents calculated to revive effaced writing.

M. Chasles declared that he would place at the disposition of the Academy all the letters and documents to be submitted to all manner of tests.

M. d'Abbadie presented on the part of M. Radau a note on the ancient meteorograph and on the theory of the barometer of M. Moreland. We find in the *Journal de Physique* of the Abbè Rozier (1782), a memoir of Magellan, giving a description, accompanied with plates, of a "Perpetual Meteorograph." Seven instruments trace parallel curves on the same diagram, moved by clockwork. The pressure is registered by a wheel barometer, the temperature by a metallic thermometer, and the humidity by a hygroscope constructed of wood; the force and direction of the winds are obtained by a very ingenious anemograph; rain, evaporation and the height of the tides are indicated by apparatus furnished with floats. Nothing is wanting, and in several respects this meteorograph is superior to those recently constructed. The steel yard barometer was invented by Sir Samuel Morland, who presented it towards 1670 to Charles II. Magellan possessed a barometer on this principle, constructed by Jonathan Sisson; he improved the mode of suspension. Later, in 1791, the Rev. Arthur M'Quire le transforma en barographe en attachant un crayon au sourmet du tube (*Transactions* of the Royal Irish Academy, Vol. IV.). The theory of the barometer, complicated enough, was not as yet given exactly.

Sept. 2, 1867.

Death of Faraday.—Godard and Savigny Prizes; Father Secchi on Shooting Stars.—Stellar Spectroscope.

A letter from Mr. Dumas informed the Academy of the loss it has just sustained by the death of Mr. Faraday, one of its foreign associates. M. Dumas made this mournful communication at the request of M. Tyndall, who thought that M. Dumas could, as one of the friends of the illustrious deceased, replace, in the performance of this duty, his family.

M. Chevreul, after having read this letter, added that all the members of the Academy would certainly participate in the sentiments it expressed; he paid a solemn homage to the memory of the great English physicist, and related several facts which manifested his modesty, and the nobleness of his character.

The correspondence included some documents on the definitive disappearance of the great birds of Madagascar (Epiornis Maximus), and on the work of MM. Burden and Bourget, on heat, etc.

M. Chasles replied to the criticisms of M. Faugère. He commenced by enumerating again the very important and varied documents in his possession, among which are about 1,000 documents of La Bruyère (nearly 300 letters, a key of characters, the same which circulated at that time among the intimate friends of La Bruyère, and many other isolated pieces). M. Chasles combated, one by one, the objections raised by M. Faugère, apparently successfully. He remarked how very improbable it was that a falsificator could fabricate such an immense number of documents of a nature so different. He must have had a great imagination!

M. Chasles cited letters from Desmaizeaux to Fontenelle, in which there is doubt of the relations between Newton and Pascal; they say it was the professor of young Newton who wrote his letters. M. Bertrand remarked how singular it was that no allusion is made to this in Fontenelle's *eloge* of Newton. M. Chasles replied that Fontenelle had requested of Desmaizeaux information as to the youth of Newton before he wrote his discourse. Desmaizeaux, in his reply, said that he possessed Newton's papers, but that his family had confided them to him on the condition of not making use of them. "I will give you," he said, "notes, but which will not injure the reputation of M. Newton, so well established." Fontenelle submitted to Desmaizeaux his project of *eloge*, and the latter prayed him not to revive old souvenirs nearly forgotten. Leibnitz knew all these things; he avowed that he had papers of Pascal, *but that he makes no mystery of them, like M. Newton.*

A commission was appointed for the Godard prize, and another for the Savigny prize, founded by Mademoiselle Lettellier, in favour of young travelling zoologists.

Father Secchi made several interesting communications. The first was on shooting stars. The cholera had dispersed the small staff of assistants at his disposal at Rome; nevertheless, he had two observers who determined that on the 10th of November the hourly number was thirty-five between two and three in the morning. Comparing this number with that observed in the previous year, it appears that at Rome the diminution was not so sensible as at other places. He then presented a stellar spectroscope, of very moderate dimensions, made by Secretan; also a memoir on the actual state of the science of meteorology as regards metorographic instruments, and laid upon the table sheets on which the meteorograph of the Exhibition had traced curves.

M. Archiac presented several memoirs relative to geology.

Sept. 9, 1867.

The Pascal-Newton Forgeries.—New Compounds of Cyanide of Amyl.—Polarisation of Electrodes.

Let me say a few words on the answers of M. Chasles to the objections urged by M. Faugère, which had some appearance of gravity. The first was the allusion made in one of the letters written by Pascal, in 1652, about coffee and the froth of coffee. "Coffee," he said, "was not introduced into Parisian society before 1660, about seven years after the death of Pascal." Thus, without going further, M. Chasles finds in the Dictionary of Bouillet that coffee was drank in Venice in 1615, and at Marseilles in 1654; and in the new curious treatises on coffee, tea, and chocolate, published by Philip Sylvester Dufour, in 1684, we find "coffee has not been known in France for more than forty years." Subtracting these forty from 1684 we have 1644. Pascal, young, well acquainted with the world, and ardent in the advancement of progress, would not have been the last to have known the existence of coffee. Moreover, a forger, while treating on universal gravitation, would not have dreamt of connecting it with the froth of coffee.

To the second objection of the improbability of a correspondence between Pascal and Newton while children, as M. de Morgan and Sir David Brewster remark that he never knew French otherwise than with a dictionary, M. Chasles answers by presenting striking documents.

"PASCAL TO WALLIS.

"This 29th August, *apropos* of this young student (Newton), can you give me some tidings of him, and principally about his arrangements. Some friends assured me that the letters written by him to me, and the questions that he has submitted seem to have rather come from his professor than from him. I should like very much to have correct information. Perhaps you can give me a word about it? I am waiting for your reply."

"DESMAIZEAUX TO FONTENELLE.

"October 20, 1727.—This one (the Professor) advised his young pupil to write a letter to Pascal, and to submit to him some geometric questions or problems to be solved. It is the best way, said he, to obtain an answer. The letter was then prepared in concert with the Professor, as well as the questions, and sent by young Newton, yet a student, to M. Pascal. The latter finding, without doubt, the letter and questions extraordinary for a child, and recollecting, perhaps, that he himself had been a precocious child, ardent to learn, searching everywhere to instruct himself, replied to the young Newton. It was thus that relations sprang up between the two men of genius, which lasted till the death of Pascal. I am perfectly sure that young Newton took part in it. It could not be otherwise. However, 'M. Le Chevalier Newton' has avowed it to me himself, that it was these relations which engaged him to follow a scientific career."

Little satisfied with the peremptory answers of M. Chasles, M. Faugère returned to the charge and answered one by one his arguments without signification, insisting, above all, on the verification of the writing. Now, M. Chasles declared that he is ready to accompany him to the Imperial Library, in presence of the members of the Commission and the Academy who would wish to take part in it.

At the last meeting M. Dumas transmitted a second letter from Dr. A. W. Hofmann on a new compound of cyanide of amyl analogous to hydrocyanic acid. By pouring gradually a mixture of an alcoholic solution of ethylamine and chloroform into a retort containing pulverised hydrate of potash, a most energetic reaction takes place; the mixture becomes heated to the boiling point, and a liquid distils over, the penetrating odour of which surpasses all that can possibly be imagined. The liquid is the cyanide of amyl, transparent, colourless, lighter than water, insoluble in water, soluble in alcohol and ether, possessing an insufferable odour something like that of amylaceous alcohol and hydrocyanic acid. Its vapour possesses, in a higher degree than that of cyanide of phenyl, the property of leaving an intensely bitter taste on the tongue, and a suffocating feeling in the throat similar to that produced by prussic acid. It can be distilled without decomposition, and boils at 137°C. But little attacked by alkalies, it is decomposed by acids, with an almost explosive violence; a slight ebullition in presence of acidulated water is sufficient to transform it into formic acid and amylamen.

$$\underset{\text{Cyanide of amyl.}}{C_6H_{11}N} + 2H_2O = \underset{\text{Formic acid.}}{CH_2O_2} + \underset{\text{Amylamen.}}{C_5H_{13}N}$$

At present M. Balard, by virtue of a note inserted in the *Comptes Rendus*, and the *Treatise on Chemistry*, of M. Naquet, claims the discovery of this new analogue of cyanhydric acid in favour of M. Armand Gauthier, laboratory pupil of M. Wurtz.

M. Gaugain presented a note on the polarisation of electrodes. Several savants have sought to determine the part which each of the electrodes takes in the polarisation, and have arrived at different results: M. Poggendorff found that the two electrodes contributed equally to the production of the electromotive force developed; MM. Lenz and Sawelgew found on the contrary that the part of the cathode is greater than that of the anode. M. Gaugain tried in his turn to resolve the question by making use, as he did on former occasions, of the *method of opposition*, and the following are the dispositions he adopted. In a cylindrical glass vase he placed a porous cylinder of much smaller diameter, and both vessels were filled with the same liquid. The strips of platinum which are to serve for the decomposition of the liquid are placed in the exterior vase, and a third plate of metal is introduced into the porous cylinder; this third plate, which remains constantly out of the circuit, traversed by the current, does not experience any polarisation, and can be successively compared with each of the electrodes when these are polarised to saturation; this comparison gives the measure of the two polarisations of the anode and the cathode. The porous diaphragm serves to keep the neutral plate out of reach of the influence of the hydrogen disengaged by the electrolysis.

The following are the results thus obtained by a series of experiments carried on with a mixture of nine parts by volume of distilled water, and one part of sulphuric acid.

Polarisation of the anode...........	193
" " cathode.........	157
Total polarisation.................	352

It appears to be of little consequence, if more or less sulphuric acid be added to the electrolysed water, provided that this proportion does not fall below a certain limit; but when it becomes extremely small the polarisation of the cathode increases without the polarisation of the anode being sensibly modified. The following are the results obtained by electrolysing pure water:—

Polarisation of the anode............	193
" " cathode..........	243
Total polarisation..................	434

M. Matteucci recently (*Comptes Rendus*, Jan. 14, 1867) called the attention of the Academy to an experiment which he had made in 1838, and upon which he depended to prove that the polarisation proceeded from the gases adherent to the electrodes. In fact, polarised metals should be considered as fugitive combinations formed by the metals and gases, and the author is of opinion that in couples of polarisation as well as in Grove's gas pile, the electromotive force is the affinity exerted on one of the elements of the water by a gas associated in a particular manner to a metal.

BRITISH PHARMACEUTICAL CONFERENCE.

Fourth Annual Meeting at Dundee. President, PROFESSOR BENTLEY, F.L.S., M.R.C.S., etc.

THE President opened the proceedings on Tuesday, Sept. 3, by delivering a most interesting address on "the study of botany in connection with pharmacy." Last year he gave an address on the same subject, when he confined himself to the consideration of some of the more immediate and direct advantages which the pharmaceutist would derive from a knowledge of botany, while this year he spoke of its value as a mental training, and as a recreation. Having alluded to the great advantages to be derived from the study of such branches of natural history as botany, in training the mind to observe correctly, discriminate accurately, and to acquire orderly and systematic habits, the president expressed a hope that it would not be long before such studies will become an essential part of the education of our youth. He then spoke of the advantage which the pharmaceutist would derive from taking up the study of a natural science by the combination of scientific with the more purely practical studies of their profession, thanking the liberal and enlightened founders and subsequent supporters of the Pharmaceutical Society for doing much to drive away the erroneous idea that a pharmaceutist should confine his attention entirely to the practical parts of his business. The president then proved the study of botany to be eminently calculated to prove an agreeable and healthful recreation to the pharmaceutist, urging upon the young student of pharmacy, the importance of its study during his pupilage in order that he may acquire that knowledge of its details and technicalities, as will enable him hereafter to pursue and enjoy it as a recreation, and concluded by thanking the local committee for the kind hospitable manner in which they received the visitors and for the very satisfactory arrangements they had made for holding the meetings of the conference.

We subjoin abstracts of the papers read during the sittings.

"*On the Adulteration of White Precipitate.*" By J. B. Barnes, F.C.S.

Seventeen years ago I tested some white precipitate, in the stock of a highly respectable pharmaceutist, and found it contained 50 per cent. of chalk, it had been supplied by a large wholesale house, at the price of a pure article..

I have received from members of the Conference, residing in the principal towns, 58 samples, and three other specimens —small portions of each of these parcels were separately exposed to the action of a strong heat. Of this large number (61) four only exhibited evidence of the adulterant; from Bristol, I received four specimens, No. 1, was pure; No. 2, contained 74·7 per cent. of carbonate of lead; No. 3, contained 22 per cent. of chalk, and No. 4, consisted entirely of carbonate of lead. From North Shields, three samples, one of them said to have been obtained by the retailer from Newcastle, and contained 94 per cent. of chalk. One sample from Shrewsbury contained a trace of peroxide of iron, the remaining samples were all pure—I venture to think the result is very creditable to the members of our profession, the samples having been obtained in neighbourhoods where it might have been supposed that adulterated samples would be met with; indeed, my Bristol contributor states that he obtained his specimens from a locality where he was sure to be able to find adulterated drugs.

"*Notes on Effervescing Citrate of Magnesia.*" By E. Dymond, *Birmingham.*

The author protests against the pleasing deception which is practised in the sale of the various popular granulated effervescing compounds, being almost without exception known by names which do not express their composition and real character, and he holds that the welfare of true pharmacy is in jeopardy whilst we tacitly recognize such departure from correct chemical nomenclature, and from those obligations which we are under to the pure truths of science and morality. The writer placed upon the table a specimen of saccharated effervescing citrate of magnesia which fulfils the conditions required in this preparation of brisk effervescence during the discharge of carbonic acid, and a nearly brihgt subsequent solution.

"*Remarks on a Specimen of Seaweed Char.*" By Ed. C. C. Stanford, F.C.S.

Mr. Stanford introduced to the meeting an interesting specimen of charcoal, obtained by the carbonization of tangle. This substance consists of the long stems of *Laminaria digitata*, which are thrown up in great abundance on the western shores of the outer Hebrides; these are collected in the winter and dried in the air: these, when first thrown up, are long fleshy stems, 7 to 8 feet in length, and about the thickness of the wrist, but when dried, present hard, horny, flexible rods, about the size of the finger. These, when carbonized, swell out into a highly porous charcoal, about three times their original volume.

The char contains about 40 per cent. of salts free from sulphides and very rich in iodine.

After lixiviation the residual char has the following composition; it varies slightly, and the average proximate analysis in the dry state, is here given:—

Carbon	50
Phosph. lime	4
Carbonate of lime	20
Carbonate of magnesia	6
Silicic acid	5
Alumina	2
Sulphate of potash	5
Chlor. iodine	5

and about 1·25 per cent. ammonia.

It generally contains about 15 per cent. of water, which it is very difficult to separate, the charcoal having a powerful affinity for moisture.

Attention was called to the remarkable analogy between the chemical composition of this char and that of animal charcoal, which appeared to class it with that substance, and render it unlike any other char of a vegetable origin. This char cannot be used for sugar refining, on account of the large percentage of carbonate of lime; but it possesses decolorizing and deodorizing properties, superior, weight for weight, to the best animal char; tested with solution of caramel it decolorizes 25 per cent. more than animal char under the same conditions.

It has been subjected to continued filtration of the thickest town sewage, for several months, without the least clogging, and its efficacy under this treatment remained unimpaired.

This communication was merely preliminary, the author promising the results of further investigation on this and other specimens of seaweed char.

The tangle char was brought before the meeting as a cheap and efficient substitute for animal char in its application other than that of sugar refining; and its introduction excited an interesting discussion.

"*On Glycelæum, a Proposed basis for Ointments.*" By T. B. Groves, F.C.S.

Take of

Almond meal	½ oz.
Glycerine	1 "
Olive oil	3 "

Mix s. a. It may be effected in a mortar in the ordinary way, up to nearly the end of the operation; but it is better, I think, to use the spatula and "slice" in the last addition of oil. It will then form a soft, semi-gelatinous paste, which, when mixed gradually with water or a watery fluid, forms readily an emulsion. The glycerine it contains being protected by the oil it does not quickly deliquesce, though when exposed to the air for some time it does often somewhat. It is of course unaffected by the ordinary temperatures of the body; if it were otherwise, its softness would be an objection to its use; as it is, it leaves plenty of room for powdery admixtures of every kind.

It is only essential to remember that the body in the first place must not precipitate emulsine, in the second place must be a fluid. I have in several ways attempted to emulse lard. I have melted it and succeeded perfectly, so long as it remained fluid; but if stirred after solidification, the emulsion was at once "inverted," or as Mr. Proctor styles it, converted into a "negative" emulsion, *i.e.* the glycerine is emulsed in the fat, and not the fat in the glycerine.

The advantages I attribute to glycelæum as compared with ointments and with plasma, I imagine to be these:—Ointments are greasy, prone to rancidity, do not "touch" in a strict sense, watery surfaces, and are not easily removed from the surfaces to which they become attached; on the other hand they are cheap, they are fatty, and they are repellent of moisture.

Glycelæum has been little tried as a remedy; I have had difficulty in finding persons to make a trial of it. Dr. Tilbury Fox has, however, at Mr. D. Hanbury's suggestion, made some experiments with it, and reports "that he likes it very much; that it is a capital thing where it is a desideratum to get hardened parts into a more 'supple' condition." Although I can bring but one testimony in its favour, it must be allowed to be a first-rate one.

Still less trial has been made of glycelæum as a vehicle for the administration of oils and balsams, though it would not be difficult to find stomachs that support with difficulty castor and cod-liver oils, and balsam of copaibæ. As "oiled" melted butter is known to upset a weak stomach, whilst well-made, *i.e.* well-emulsed melted butter does not, it might be inferred that an emulsed oil would in some cases agree with the stomach when the plain oil would not. I am convinced of this, that the glycelæum copaibæ, stiffened with powdered cubeb, would form a more elegant and a more supportable electuary than the nasty and imperfectly mixed mass one commonly meets with.

I have already alluded to the fact. that it is to the emulsine contained in the oil-seed we must attribute the extraordinary emulsive power of these emulsive powders. (Certainly no organic principle has been more consistently named than it.) This I have proved experimentally, by preparing some of the substance, and trying it in its pure state. I found that five grains dissolved in one drachm of water would emulse into a jelly four drachms of olive oil (using the spatula, not the pestle). I prepared the emulsine by digesting for a few hours powdered almond meal with tepid water filtered, and added to three measures of the filterate five measures of rectified spirit, collected the precipitate and dried it at a temperature not exceeding 100°.

BRITISH ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE.

DUNDEE MEETING, 1867.

(From our Special Correspondent.)

Dundee, Sept. 5, 1867.

In spite of the great distance from London, and the depressing influence exerted by the parsimony of the railway companies, the present meeting promises to rival any of the previous northern gatherings, excepting, perhaps, the Newcastle meeting in 1863, at which the total number of attendants amounted to 3,335. At the time I write they are nearly equal to the total of the Nottingham meeting—2,300, and new arrivals are coming, both by rail and boat. The exigencies of the Post Office allow me only a very limited time to furnish this preliminary letter, so that if my narrative is imperfect in any material sense, I will communicate by telegraph, and the facts may then be interpolated by you. The lodging accommodation in the town seems as yet in excess of the demand, and the prices first quoted are being reduced. Probably this may be accounted for by the fact that a large proportion of the tickets issued has been purchased by residents in Dundee and the neighbourhood. The General Committee met yesterday, at the Panmure Street Chapel, at 1 p.m.

Professor **Hirst**, one of the General Secretaries, read the report of the Council for the year 1866-7.

Mr. W. **Spottiswoode** then read the report by the treasurer, which was also agreed to.

Mr. **Griffith**, Assistant General Secretary, read the report by the Parliamentary Committee.

REPORT OF THE KEW COMMITTEE.

Mr. J. P. **Gassiot** read the report of the Kew Committee.

The **Assistant General Secretary** then read the report of the committee appointed by the Council of the Association to consider the best means for promoting scientific education in schools. The report, after pointing out that there is already a general recognition of science as an element in liberal education, and stating that general education in schools ought to include some training in science, went on to refer to the difficulties in the way of introducing science into schools, which the committee, however, considered easily surmountable. With a view to the furtherance of the scheme, the committee made the following suggestions:—

1. That in all schools natural science be one of the subjects to be taught, and that in every public school at least one natural science master be appointed for that purpose.
2. That at least three hours a week be devoted to such scientific instruction.
3. That natural science should be placed on an equal footing with mathematics and modern languages in effecting promotions, and in winning honours and prizes.
4. That some knowledge of arithmetic should be required for admission into all public schools.
5. That the universities and colleges be invited to assist in the introduction of scientific education by making natural science a subject of examination, either at matriculation, or at an early period of a university career.
6. That the importance of appointing lecturers in science, and offering entrance scholarships, exhibitions, and fellowships for the encouragement of scientific attainments, be represented to the authorities of the colleges.

In the afternoon the grand floral fête in Baxter Park was opened under very favourable circumstances as to weather; this is considered to be the largest and finest floral and horticultural show ever held north of the Tay. It was again open this morning to private inspection of the members of the Association.

Yesterday evening the first great meeting took place in the Kinnaird Hall, the company being but little assisted on their way to the assembly by the electric light, which was exhibited in front of the High School, under the charge of Mr. Louis Schwendler. At eight o'clock Sir Roderick I. Murchison, F.R.S., acting for Mr. Grove, who was prevented by illness from attending, in a short speech formally vacated the chair to his Grace the Duke of Buccleuch, the president of this year.

The large meeting-hall was crowded in every part, and confusion had been as much as possible avoided by numbering each seat, and not admitting members or associates unless they had previously got their tickets stamped with a similar number. Your correspondent, who was not aware of this judicious regulation till the last moment, would have had a poor chance of hearing the forcible speech of the noble president, were it not that a General Committee ticket carried with it the privilege of admission to the platform.

Sir **Roderick Murchison**, in introducing the Duke of Buccleuch, expressed regret at the unavoidable absence of Mr. Grove, in whose behalf it had fallen to him as a former President, and as one who had filled nearly every office in the Association, to hand over the chair to the President elect.

About a quarter to eight o'clock, considerable excitement was caused by Sir David Brewster having been seen to become suddenly faint. He was conversing with Professor Balfour, who sat behind him, his arm being thrown over the back of the chair, when he ceased speaking, his face became deadly pale, his hand fell from the back of the seat, and he was about to fall, when Professor Balfour sprung forward and caught him. After Sir David Brewster had been extended motionless on the platform, and water brought, in a few minutes he recovered from his swoon, and was carried out of the hall.

After the confusion had subsided, Professor **Phillips** came forward and said: Our highly honoured member, Sir David Brewster, has been affected by the heat of the hall. He is now decidedly better.

The Duke of **Buccleuch**, having taken the chair, said—Gentlemen of the British Association for the Advancement of Science, and ladies and gentlemen,—As to what has fallen from Sir Roderick Murchison, I feel that, whatever bold deeds my ancestors may have done, or may have attempted, perhaps in one sense I have attempted the boldest of them all. If it were only a question of physical endurance, or dashing enterprise, I should not have felt abashed, nor shy, nor disinclined for the encounter. I think the old spirit of the Borderer would have carried me through. I have been told, and perhaps with reason, that it is better and more usual upon such a great occasion as this for the President to prepare his speech or address with care beforehand, to commit it to writing, and give an opportunity of having it put in print for the convenience of the members of the Association, and also of those whose particular vocation and duty it is to communicate to the public that which passes at public meetings. Unfortunately, perhaps, for myself, and still more unfortunately for you, I have not so done. I never in my life attempted to pen an address or to prepare a written speech to be delivered. If I had done so, and had recourse to the productions of the pens and heads of others, I might have read an address to you in flowing language—full of science, full of information; but I could not have pretended that what I read came from myself. I preferred rather to fail by speaking what I had to say direct from myself, as it came from my heart and from my head, than have recourse to the assistance—although most valuable it would have been—of the thoughts and pens of others.

When I consider the nature and intention of this great Association, I cannot but feel that one of the greatest gifts which Providence has bestowed upon man is great intellectual power. It is a talent of the highest price; it is a talent vouchsafed to but few. Happy are those men themselves, and blessed is it for this country and for the world, when they who have the intellectual power have also the will to exercise it, the power to exercise it, and to direct it aright. You will rarely see that any one man possesses the full intellectual power to make himself master of the whole. Ladies and gentlemen, this reminds me that since the last meeting of this Association, and within a very short time, one most distinguished member has been gathered to his fathers—I mean Professor Faraday—one of the most distinguished men in his own branch of science, one who having great intellectual power, and having great personal will, was determined to rise above that position in life in which he happened to be born. Happily for him he took a line, and sought a friend in one who was able to forward his views; and I believe that in his own department of science no man was more prominent than Professor Faraday lived to become. In him we have to mourn one that is lost; but when we mourn one that is lost, is it not an incentive to many others who may have been born in the same position as himself, or may, perhaps, have been born in other positions, in a higher and better position than he, with every opportunity of cultivating science, and instructing themselves in every way? Is it not an incentive to every man who may feel himself possessed of that power to push himself forward quietly, unostentatiously, but at the same time not for the personal pride of position, but for the more generous ambition of being a great benefactor to his country. We heard to-day at the preliminary meeting, a report made upon an important matter—namely, that of having science taught at our public schools—that it should form a portion of the curriculum of study in every school. I quite agree with that, but I think you must not, at all events in the first instance, attempt to push it too far. Give the youth a taste for science, and when they have acquired this taste, those who have an aptitude for the sciences will each be very much inclined to follow a particular science for himself. You can no more drive science down a boy's throat than you can teach mathematics to a horse. (Laughter.) If he has a turn for it, he will take it in; if he has not a turn for it, he will say that it is a greater bore than Latin or Greek; but to teach him the elements of science is of great importance. In every relation of life a knowledge of science is becoming more and more necessary. I need not go further than the town in which we are at present assembled. Where would the prosperity of this town have been had it not been for science. You will say we have manufactures of flax, hemp, jute, and things of that sort; there is not much science in that. Well, go to the cultivation of these plants—go on to the preparation of these plants after they are cultivated, and bring them to this country. Do we not require some amount of science to build those ships, and to navigate them? and when those vessels come to port do we not require some science to produce the docks and harbours in which these vessels lie? Then again, when you come to the manufacture of the raw material, do we not require some science in chemistry and in mechanics; and in mechanics, we require mathematics to begin with? Then, these inevitable faculties are necessary to produce the machinery by which all these raw materials are to be made into useful articles of commerce. Is it not also the case when we come to the cultivation of the soil? What do people do now? It is not the rule of thumb process—the old story, that you must put lime here and farm manure there. You ask, why? The answer is, it stands to reason —because the soil requires that. Standing to reason is a very good answer; but the man who gives it goes by the rule of thumb. We want a man with science and chemistry to tell us why we do these things—why we apply one description of manure to one soil, and one to another; and why, if we apply this description of manure to one place and to another, we apply it to the wrong place. Again, we have many other branches. Take geology for instance. I am sure it would be difficult to say how many hundreds of thousands have been sunk and lost in seeking for that wonderful vein of gold in the shape of coal, and other things which nobody would have dreamt of doing if a question had been asked of any common, ordinary geologist. I was talking a little while ago of scientific education in schools. We want scientific education in our Universities. We want to have natural and physical science taught in our Universities. It is not many months ago since a powerful effort was made to get an endowment for a geological chair in Edinburgh, which, we may say, is the cradle of geology—where it took its rise, and flourishes particularly. Unfortunately that attempt was simply snuffed out. We met with a cold reception. In vain did we say that endeavours had been made to endow the University of Edinburgh, which happens to be very poorly endowed; but the efforts and contributions of almost all those who responded to the application have been directed to founding scholarships and bursaries to enable poor students who thirst for knowledge to avail themselves of the knowledge which might be afforded there; and we thought the public purse might well afford a professor to teach that which was really of national importance. I cannot pretend to go through all the different points and sections which are taken up by this Association. I will only call your attention to one which I take some interest in—namely, meteorology. There, great efforts have been made, and with signal success, by the British Association for the Advancement of Science, more particularly at Kew Observatory. What I and others have urged on the Government of the day is the great importance of having renewed and carried on what were called the storm signals at our different ports. This is of immense value and importance; and I believe much valuable property has been saved, and many valuable lives preserved by the timely hoisting of the drum signifying bad weather. At the Firth of Forth I have seen the drum hoisted indicating tremendous storms of wind, and risks that may be run, and yet not a breath of wind blowing in that particular quarter. Some who had not this simple warning might have said, "We might just as well go out to sea, as there is not a breath of wind here." But then, there comes the newspaper, twenty-four or forty-eight hours afterwards, telling of disastrous gales of winds and shipwrecks upon no very far distant portions of the coast. Well, the great thing I may say with regard to this Association is, that it is not exclusive or repulsive. It will neither expel nor repel others, nor will it seek to include within its sphere other societies that ought more properly to be by themselves. I do not know, gentlemen, that I have any right to trespass any more upon your time—I have trespassed already too long, perhaps. I must, in conclusion, be allowed simply to thank the gentlemen of the British Association for the Advancement of Science for the honour they have done me in placing me in this chair. I know it is no slight honour—I feel it is no slight honour—I also feel it is no slight responsibility—but, so far as in me lies, I will endeavour faithfully to perform my duties, and I hope these duties will not be so performed as to show that you have been entirely deceived in having selected a person for this high position who was totally unfit to be your choice. (Cheers.)

Professor Phillips, in an eloquent and interesting speech, passed in review the progress of the British Association and the work it had done towards the advancement of science during the course of 36 years, and concluded by proposing a vote of thanks to the Chairman for the inaugural address he had just delivered.

Section B.—Chemical Science.

President:—Professor Thomas Anderson, M.D., F.R.S.E. *Vice-Presidents:*—Maxwell Simpson, M.D., F.R.S.; Professor Williamson, F.R.S.; J. Lothian Bell, F.C.S.; W. Odling, M.B., F.R.S., Dr. Gilbert; Professor J. S. Brazier; Dr. Penny. *Secretaries:*—Professor Liveing, M.A., F.C.S.; Dr. Russell, F.C.S.; Dr. Crum Brown, F.R.S. *Committee:*—J. Attfield, Ph.D., F.C.S.; Dr. Barford, F.C.S.; A. R. Catton, F.C.S.; R. Calvert Clapham, F.C.S.; W. Crookes, F.R.S; John Davy, M.D., F.R.S.; Dr. Heddle; W. E. Heathfield, F.C.S.; P. Spence, F.C.S.; J. Spiller, F.C.S.; E. C. C. Stanford, F.C.S.; Dr. R. Angus Smith, F.R.S.

In the High School.

President's address.

A. R. Catton.—Report on the Synthesis of certain Organic Acid.

A. R. Catton.—On the Synthesis of Formic Acid.

A very animated discussion took place upon these synthetical papers between Professor Wanklyn and Mr. Catton, in which each speaker defended his own views, and attacked those of the other in very energetic language.

J. A. Wanklyn and R. Schenk.—On the Synthesis of Caproic Acid.

J. A. Wanklyn.—Action of Sodium on Valerianic and similar Ethers.

P. T. Main and A. R. Catton.—On a new Synthesis of Ammonia.

J Spiller.—On the Decay of Stone.

W Weldon.—On the Regeneration of the Oxide of Manganese.

To-day the sections commenced their regular work. The committee of Section B met at half-past ten a.m., and at eleven the President, Dr. Thomas Anderson, F.R.S.E., opened the proceedings with the following address:—

On many previous occasions the British Association has met in places which have afforded the chemist most valuable opportunities of seeing the principles of his science reduced to practice, and the various papers which have been read in this Section on these subjects, and the discussions which have arisen regarding them, have formed a very interesting department of its proceedings. At the present meeting little of this is likely to engage our attention, for though the manufactures of Dundee have probably increased during the last ten or fifteen years, in a more rapid ratio than those of any other town in the kingdom, they have taken a direction which gives but little scope for the applications of chemistry, so that with the exception of a few of the simpler operations of the dyer, there is really scarcely anything which need specially attract our attention. Under these circumstances it may be fairly anticipated that the business of the Section will be more particularly occupied with the discussion of the great principles of the science which to the general public are often less interesting, and regarded as the exclusive province of those engaged in scientific study, and not sufficiently recognised as being the only sure foundation on which the superstructure of practical progress can be raised.

The consideration of these general principles is, however, at the present moment a matter of the very highest importance, for the science of chemistry is in a state of transition. The immense accumulation of facts which has been made during the last twenty or thirty years, has not only increased her bounds, but has shown the insufficiency of those principles on which the chemist was formerly ready to rely with almost implicit confidence, and introduced changes amounting to a revolution which have had the effect of unsettling the views formerly entertained by almost all chemists, without as yet introducing anything conclusive in their place. The atomic theory, which at the commencement of the present century explained with clearness and precision all the facts of the science then known, has proved itself (at least in the form in which Dalton left it) no longer sufficient for the purpose. At that time the knowledge of chemists was confined to a comparatively small number of compounds, among which those of oxygen had so preponderating an importance that the science of the time might almost be described as the chemistry of oxygen. At the present moment, if we were to attach to it the name of any individual element we should most unquestionably describe it as the chemistry of carbon, for it is in the study of its compounds that all the difficulties with which the chemist has now to contend have had their origin. At a comparatively early period indeed, doubts were expressed as to the sufficiency of the atomic theory of Dalton, and Ampère especially suggested that the chemical atom might with advantage be considered to be a congeries of smaller particles; but this and other analogous additions to the original conception of the chemical atom, having no immediate bearing on the facts then, or even now known, have never been accepted by chemists, or received from them more than a very passing notice, and were not unfairly considered to be unnecessary complications of the theory. It was left for time to accumulate facts, for which Dalton's theory supplied no explanation of any kind, and these were at first neglected, but as their number increased, their explanation was evaded by the invention of names intended to group together facts supposed to be dependent on similar causes. Such names as catalysis, allotropy, and the like really explained nothing, they are little better than scientific lumber-rooms, in which unexplained facts are stowed away until it suits our knowledge or our convenience to classify and explain them. I am far from asserting that this mode of grouping facts supposed to have something in common, has not its advantages, provided only it be distinctly understood that it is the grouping of ignorance. The risk lies in the name being accepted as an explanation, and inquiry being thereby retarded, and something of this sort has indeed occurred, for though these facts were admitted to be beyond the scope of the atomic theory, they were quietly set aside; things went on as they were before, and it was not till the introduction of the theory of atomicity, which shows itself in every chemical fact, that the doubts which had been long gathering in the minds of all thoughtful chemists found distinct expression. I do not on the present occasion propose to discuss in detail the effect which the introduction of this view has had upon chemical theory, further than to remark that it renders it necessary either to abandon altogether the atomic theory of Dalton, or to introduce into it such modifications as fundamentally alter its entire character, and make it substantially a new theory. The former is an alternative which some chemists will be greatly disinclined to adopt. They will not willingly abandon a theory which has admittedly done admirable service, which at its first introduction established order and regularity, where confusion and disorder previously reigned supreme, and under whose influence the science has attained its present goodly proportions. Others again may be of opinion that the atomic theory has done its work and in the future is less likely to act as an assistance than as a hindrance to progress, by forcing us to consider all facts in its particular light, and causing us to overlook relations which might be at once detected by an unbiassed mind.

This latter opinion has been very strongly expressed by Sir Benjamin Brodie, and in the calculus of chemical operations, which he has recently made public, we have the first systematic attempt which has been made to express the constitution of chemical compounds, by a method in which the idea of an atom has no place. As this is the most important chemical doctrine which has been put forward for many years, and must if accepted materially alter our present views, I shall venture to consider it in some detail, premising, however, that as only the first part of the investigation has yet been made public, any opinion I may now express regarding it, may be liable to modification when the entire investigation is published.

Sir B. Brodie, as has been already observed, discards altogether the idea of an atom, and compares with one another the weights of different substances in the gaseous state which are capable at the standard temperature and pressure of filling a unit of space, which is the bulk of 1,000 cubic centimetres. If we consider this space to be empty, and fill it with hydrogen, a chemical operation is performed which is represented by the symbol a, expressing the fact, that the weight so introduced is chemically indivisible. If now in place of hydrogen, oxygen be introduced, the unit of space is filled by a quantity sixteen times as great, but this weight is not indivisible, as is at once apparent if we notice what happens when oxygen is introduced into the unit of space already filled with hydrogen. In that case a second operation is performed on it, in which a weight eight times as great as that of the hydrogen is introduced, and water is the result. The quantity of oxygen which fills the unit of space must therefore be regarded as divisible, and this is expressed by assigning to it the symbol $\vartheta 2$, indicating the fact that two identical operations are required to fill the unit of space with oxygen. By the same line of argument it is concluded that sulphur, selenium, etc, must be similarly constituted, and they are accordingly represented respectively by $\theta 2\lambda 2$, etc. So far it will be observed that the system is merely a modification of that at present used by chemists for expressing the laws of gaseous combination, excepting that all substances, compounds as well as elements, are referred to the unit of space, while, according to our present plan, the former are referred to two units of space, and the latter to one. But when the compounds of chlorine and the allied elements, with hydrogen, are to be represented according to Sir B. Brodie's system, it at once becomes apparent that some further hypothesis must be introduced if they are to be referred to the same volume. When the quantity of hydrogen represented by the symbol a, unites with chlorine, the product fills two units of space, and as, according to the fundamental hypothesis, a is indivisible, the question is to obtain some means of expressing without fractions the quantity of hydrochloric acid which fills the unit of space. This end Sir Benjamin attains by assuming that chlorine is itself a compound of hydrogen with an unknown element to which the symbol χ is assigned; chlorine being $a\chi 2$, and formed by these operations, one being hydrogen, and the other two which are identical, result in the introduction into the unit of space of two quantities of a hypothetical substance χ, whose weight is 17·25, and according to this view, when hydrogen and chlorine unite, the action is expressed by the equation:

$$a\chi 2+a=2a\chi$$

On precisely the same principle iodine, bromine, nitrogen, phosphorus, antimony, and bismuth, must also be hydrogen compounds. It is obvious, therefore, that Sir Benjamin's system involves a very large amount of hypothesis, for it assumes that a considerable number of those substances hitherto regarded as elements are really compounds. I do not imagine that much difficulty will be experienced by any one in admitting the possibility of this, for I apprehend there is no chemist who imagines those bodies which we call elements to be the ultimate constituents of matter, or who doubts that the time, though still far distant, will come when they may be resolved into simpler substances. But when we come to reduce these speculations to a definite form, and seek to make them part of the science itself, it becomes essential to subject them to a very close and searching scrutiny.

In order to justify their assumption, it seems to me necessary either that they should be supported by experimental evidence, or that they should afford the means of tracing out unsuspected relations, and thus extending the bounds of the science, or, at all events, that they should involve the minimum amount of hypothesis. Now, as regards the first of these, it is unnecessary to observe that there is not one tittle of evidence to show that chlorine is a compound any more than hydrogen itself. As far as extending the bounds of the science is concerned, we must look for an answer to the future, and it may be expected that in the remaining parts of the investigation, which it is to be hoped will soon be made public, it will be shown how the method may be used for this purpose; but, in the meantime, I confess I am unable to see how it can be used so as to open up new fields of inquiry, and it is certain that it leaves unexplained all those anomalies which are encountered in the existing system. Neither can it be asserted that the system involves the minimum amount of hypothesis, for, in point of fact, the assumption of the compound nature of certain of the elements is rendered necessary by the fundamental hypothesis that a is indivisible. If it be assumed to be divisible, the necessity for holding those elements to be compound at once falls to the ground, and I confess it appears to me that we should require very clear evidence of the advantages it offers before we accept a hypothesis involving so many others. The question must at best be considered as still *sub judice*, and the method is not likely to meet with general acceptance until it is supported by a much larger body of facts than those we at present have.

While Sir B. Brodie's theory is one from which the idea of atoms is excluded, it is important to notice that it is by no means incompatible with them, and it even appears to me that though it may suit our convenience to consider matter in relation to space only, the real subject of inquiry is not the unit of space, but the unit of matter, and to it we must eventually come. If I hold, as I most undoubtedly do, that the atomic theory of Dalton must sooner or later be abandoned, it is not because I do not believe in the existence of a unit of matter. Whether we assume it to be a hard spherical particle, a centre of force, or a vortex produced in a perfect ether, is another question; but it seems evident that some kind of molecular hypothesis is indispensable for the explanation of physical phenomena, and it is scarcely possible to doubt that some connection must exist between the chemical and the physical unit of matter. In the mean time it is only by the most cumbrous and improbable assumptions that the existing atomic theory can be made to fit in with the facts which chemistry has recently discovered, and of these the theory of atomicity is one which can scarcely be connected with it at all. The fact is that theory is a merely temporary hypothesis, constructed to keep before our eyes the tendency which substances have to form compounds of certain definite forms, under special circumstances; and it is scarcely possible to doubt, that in 20 or 30 years it will have passed away and have been replaced by something of a more satisfactory character. Meanwhile its important influence on the recent progress of chemistry is too obvious to be disputed. It is only to be regretted that so many conflicting modes of considering the atomicities of the elements should have been introduced by different writers. Into the consideration of this matter I should have been glad to have entered at some length, but I feel that I have already detained you too long from the actual business of the Section, and no doubt opportunities will arise in the course of the business for individuals expressing their opinions on this and other subjects. Among these the mode of expressing the symbols of chemical compounds, which was objected to long since by Sir John Herschel, and has been again brought into prominence by the publication of Sir B. Brodie's paper merits attention. The present unsettled state of chemical nomenclature, so inconvenient to the teacher, ought also to be discussed, and it might even be well to consider whether a committee should not be appointed to ascertain how far it might be possible to adopt a uniform system. Nor do I think we ought to separate without recording our opinion on the subject of better and more extended scientific education. The events of the Paris Exhibition have brought our deficiencies in this respect very conspicuously before us, and show us how much we have yet to do. That we have made progress in this respect is not to be doubted. Science is much more cultivated than formerly,—it is becoming more and more a branch of general education. Much, however, still remains to be done in this direction, especially in Scotland, and it will no doubt surprise many of my audience to hear that chemistry and natural history are still excluded from the course of study for degrees in arts in the Scotch universities. Of late years the study of this and other departments of natural science has been introduced to some extent in schools both in England and Scotland; but, I must confess, with but little advantage, so far as my experience goes. The failure, I think, lies in the kind of instruction offered; the usual practice having been to give a course of lectures from which the discussion of principles and of everything which exercises and developes the mind, is eliminated, and only that which it is supposed will entertain or surprise is retained, and boys are thus led to look upon science merely as a pastime. They are shown enough to see the difference between this and the closer and more severe system of study pursued in the other departments of their education, and they are apt either to avoid work altogether, or to acquire their knowledge in a superficial manner. The whole system of teaching science to school-boys is a subject which merits far more attention than it has yet received, and the success of the movement must greatly depend on the method of teaching adopted. All these, however, are subjects the discussion of which would carry me far beyond the limits of those introductory observations with which it has been customary to open the business of the section. It must be left for its members to bring forward their own views on these and kindred questions.

Professor WILLIAMSON:—I rise to propose the thanks of this meeting to our worthy President for the address with which he has just favoured us. He has touched on several of the most weighty and important matters which are interesting to us as theoretical chemists, and he has very justly remarked upon the very rapid progress which our science has been undergoing of late years, especially in the way in which existing theories have been made or modified. And it is surely a most encouraging proof of the life which is in our science to see, besides the discovery of facts, these immense evidences which have manifested themselves in all directions. Our distinguished President has also touched on a peculiarly interesting topic to all persons, namely, the introduction into general education of those important results which chemists and others have attained in their special departments. It is certainly to be regretted that these results should remain as yet rather unknown in our schools. It is one illustration of the slowness with which we excuse many results of great benefit to see that these great discoveries are not yet introduced into general education; and certainly we owe our President thanks for the formal way in which he has drawn attention to the importance of introducing scientific education into schools.

The practical subjects which we heard treated of with so much interest in his able address are so fruitful and weighty that I am sure the section will concur with our president in a wish that it may form the subject of one of our meetings, and I would venture to suggest that some early day should be fixed for—I was going to say a field day on the subject of chemical theories.

It seems to me that it is one of our strong characteristics that we are quite as much inclined to the practical as well as to theoretical views, and though we are perhaps to have one field day on theories, I hope there will be many such on the more practical questions.

With regard to the practical introduction of science I ought not to omit to mention that we have present to-day a distinguished member who has collected on the Continent a great amount of facts as to the progress of scientific and technical arts, more especially metallurgy and engineering. I allude to Mr. Lothian Bell, and I hope he will favour us with some of the results of his observations on the subject.

I beg to move a vote of thanks to Professor Anderson, and I am sure the section will be glad to see the printed address of our distinguished President.

The following papers were then read:—

"*On the Decay of Stone; its Cause and Prevention*," by J. SPILLER.

For several years past I have been occupied at intervals in studying the causes of the decay of stone, and in experimenting with such chemical reagents as appeared to offer any promise of being usefully applied as means of prevention. At an early state of the investigation I arrived at the conclusion that the corrosive action of sulphurous and sulphuric acids in the atmosphere, resulting from the combustion of coal fuel, operate, in large towns especially, in a very destructive manner upon dolomite and the numerous class of limestones commonly employed in our public buildings. This chemical action, aided no doubt by the simultaneous attack of carbonic acid and moisture, and in the winter season further supplemented by the disintegrating effects of frost, must, I conceive, furnish a sufficient explanation of all the facts observed.

I would here remark that Dr. Angus Smith, Mr. Spence, and others have already directed attention to the immense scale of production of these sulphur acids, and have even quoted statistical data showing the extent or degree of pollution of the air from this cause in the manufacturing districts of Lancashire. When it is known that the *best* class of coal (and coke) contains usually one per cent of sulphur, and that this proportion, reaches a treble equivalent when stated in the form of the final oxidised product,—hydrated sulphuric acid,—it follows that a ton of coal of this high quality necessarily evolves during its combustion nearly 70lbs. of oil of vitriol. Here, then, is the origin of the sulplates which we find invariably present in the loosened crust of decayed stones, whether of calcareous or magnesian character. I have tested numerous samples of dolomite, Caen, Bath, and Portland stones fresh from the quarry, and in no instance found more than a trace of ready formed sulphate, whereas scrapings taken from the decayed portions of the stone of the New Palace at Westminster are bitter to the taste in consequence of the comparatively large amount of sulphate of magnesia formed during a few years' exposure to the sulphurous gases occurring in a metropolitan atmosphere. Caen stone from several buildings and localities, Portland stone, and even old faces of chalk cliff in the neighbourhood of Woolwich, were in like manner found to contain appreciable quantities of the sulphate of lime having undoubtedly a similar origin.*

A close examination into the circumstances attending the decay of stone at the Houses of Parliament invariably shows an increased liability to corrosion under the projecting eaves and mouldings, and at such sheltered parts of the stone surfaces as are usually covered with soot and dust, and are in a position to retain for the longest period the moisture absorbed during a season of rain. The plain ashlars are throughout very much less affected than the buttresses, gablets, and other elaborately carved and highly ornamental portions of the work, which appear to be more assailable by reason of their relatively greater superficies. In many places the disintegrated stone exhibits white crystals of the sulphate of magnesia, which, alternately dissolving and recrystallising in the pores of the stone, may be conceived to exert a disruptive action sufficient to account for the scaling and fracture of the dolomite which has been so often made the subject of complaint and regret.

With the view of overcoming some of these difficulties I

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* Caen stone, Northfleet College.
Decayed exterior portion contained of sulphate of lime 3·4 per cent.
Interior of same stone (sound) " " nil.
Caen stone. St. John's Church, Woolwich.
Scales of decayed stone contained of sulphate of lime 4·6 per cent.
Interior portions (sound) " " nil.

submitted a plan to the Royal Commissioners charged with inquiring into the decay of stone at Westminster, in May, 1861, which consisted in the application to the cleaned surfaces of the stone of an aqueous solution of superphosphate of lime,—a salt remarkable for its action in hardening the surfaces of chalk, Caen stone, or other calcareous building stone to which it may be applied either by brushing or immersion, and which acts upon the carbonate of lime in the stone, giving rise to the formation of Bodeker's salt (crystallized diphosphate of lime $=2CaO,HO,PO_5+4Aq.$). My suggestion received a practical trial in a competition to which other five candidates were admitted by the Right Hon. the First Commissioner of her Majesty's Works, in April, 1864, and in regard to the work executed on that occasion upon three faces of the Westminster Palace I fearlessly await the Government report. In the meanwhile, another promising scheme for the treatment of the decayed stone, especially applicable to dolomite, has been submitted by me to the notice of the First Commissioner, but this new proposal has not yet been selected for trial. It consists in the employment of baryta conjointly with the hardening salt, so that a base may be presented which is endowed with the power of destroying the soluble sulphate of magnesia in the pores of the stone, forming with it the remarkably insoluble sulphate of baryta, and, at the same time, engaging the magnesia in one of its most difficulty soluble combinations. On a recent occasion I have applied this process on a small scale to some Caen stone facings at St. John's Church, Woolwich, which were badly decayed.

With reference to the application of the superphosphate to decayed Caen stone I am able to refer to several successful examples of its use. In the year 1862 I applied the process upon some alms-houses forming part of Northfleet College, where the decay has been completely stopped. In 1864 I operated upon a window and buttress of St. John's Church, Woolwich; and in the following year the façade of the Grand Hotel, Brighton, was treated by my process. With respect to Portland stone, the earliest experiments were made at the Army Clothing Establishment, Woolwich, where in 1861 some decayed window-sills were treated, and with perfect success. I have some interesting results to record in connection with the treatment of Portland stone, which serve to illustrate the increased hardness and strength, and the diminished rate and capacity of water absorption attending the employment of the superphosphate. Small cubes of Portland stone, each of 1·3 inch dimensions, were treated with the phosphate solution and left to dry in the air; these were then subjected to gradually increasing pressure, until crushed, between plates of lead in the American testing machine, at the Royal Gun Factory; and the breaking weights of two precisely similar cubes of the native stone were at the same time carefully determined. The results were as follows:—

		Crushing weight.	
I.	Stone in original condition	3,650	lbs.
II.	" " "	3,800	"
	Mean	3,725	"
III.	Stone treated with superphosphate	5,375	lbs.
IV.	" " "	5,500	"
	Mean	5,437	"

Thus acquiring an increased strength, amounting almost to 50 per cent. The relative hardness of the stone before and after treatment could be readily ascertained by mutual friction of their surfaces, and also by scratching with a pointed instrument of copper, which metal proved to possess a degree of hardness intermediate between the original and treated Portland stones.

The porosity of the stone as indicated by the amount of water absorbed in equal intervals of time proved to be greatly diminished in the case of the treated cubes. On this point several experiments were made, the stone being first weighed in the air-dried condition, and then immersed in distilled water at the temperature of 60° Fahrenheit for the several periods named, and the increase of weight in each case noted.

"*Whit Bed*" *Portland.*

	Original stone. Grs.	Treated stone. Grs.
Weight of cube (dry)	1421	1420
Water absorbed in 5 min.	70	7
" " 15 min.	91	8
" " 30 min.	91	12
" 1 hr. 30 min.	92	25

"*Base Bed*" *Portland.*

	Grs.	Grs.
Weight of cube (dry)	1291	1335
Water absorbed in 5 min.	120	20
" " 15 min.	122	33
" " 30 min.	124	50
" 1 hr. 30 min.	126	78

These results have been further controlled by other experiments in which the *same block* was used in the original condition, and again after treatment with the superphosphate. It will be noticed that the advantage of the process is most clearly apparent in the case of the denser and more compact variety of Portland known as the "Whit Bed" which alone is employed for external building purposes; the other—the "Base Bed"—is softer and only fit for internal decoration, and its texture is so porous that in becoming saturated it absorbs nearly 10 per cent. of water.

Samples of Mansfield dolomite absorbed amounts of water varying in different specimens from 6 to 8 per cent. After treatment by my process the degree of absorption was reduced one-half; and the results were even more favourable in the case of Caen stone.

The cost of materials employed in the treatment of stone according to this plan is very trifling, and bears but a small proportion to the cost of labour necessarily expended upon the cleaning and preliminary preparation of the stone before the solution can be applied. One gallon of solution will cover about 300 feet superficial, when two coatings are applied upon Caen or Portland stone. The superphosphate employed must not contain any appreciable amount of sulphuric acid, and the specific gravity of the solution, when diluted for use, should be about 1,100.

The facts now stated have, it is believed, but a minor interest for the inhabitants of these parts of the United Kingdom; for with Aberdeen granite and Craigleith sandstone at command there will be no need to resort to chemical methods of preservation.

The PRESIDENT said: I am sure the section will agree in expressing their best thanks to Mr. Spiller for his very interesting communications on a subject of so very great importance; which all of us appreciate, whether we be chemists or not. The destruction of so very magnificent buildings as the Houses of Parliament has been naturally looked upon as a most serious question, and we have looked forward with the greatest possible interest to the result of these experiments, so as to prevent further decay. Mr. Spiller's account of the result of his process is, therefore, peculiarly interesting to us, and the observations he made are of peculiar value, inasmuch as they afford us some explanation of the cause of this decay. We can see what is peculiar in the decay, and it shows us how important it is for us to bear this in mind when we are making arrangements such as those in connection with the House of Parliament. At the time when the erection of the building was commenced immense care was bestowed in the selection of stone, and the peculiar magnesian lime-stone was selected because it was found that all the buildings erected with it in the middle ages were in an entire state of preservation. The president concluded by saying that it had now been discovered that the atmosphere and other influences of the city had affected the stone; but Mr. Spiller's communication would be valuable, as his discoveries were of great importance in connection with the processes for the prevention of decay.

After the President's remarks on Mr. Spiller's paper, a very important discussion took place, in which Professor Ansted, Mr. Spence, Mr. Ansell, and others, took part.

The next paper—not in order of reading, but in practical importance,—was

"*On the Regeneration of Oxide of Manganese in Chlorine Stills*," by Walter Weldon.

The author stated that the essential features of the process consisted, firstly, in the use of an *artificial* oxide of manganese, capable of liberating from a given quantity of hydrochloric acid about twice as much chlorine as could practically be obtained therefrom by means of a 70 per cent. native oxide; and, secondly, in a simple method of reproducing the artificial oxide from the "still-liquor." This recovery of the artificial oxide might be performed in the stills themselves, so that a charge of manganese, once placed in a still, might always remain therein, continually generating chlorine; and not only never requiring removal, but never undergoing diminution of properties, nor suffering loss by waste.

The "still-liquor" produced in this process contained no free acid, but was a neutral solution of protochloride of manganese, mixed only with a little chloride of calcium. The artificial oxide was recovered from this still-liquor by adding thereto an equivalent of lime, and then injecting atmospheric air. Double decomposition took place between the lime and the chloride of manganese, producing chloride of calcium, which entered into solution, and insoluble protoxide of manganese, which the oxygen of the injected air rapidly peroxidised. When the artificial peroxide thus produced had been allowed to subside, and the greater portion of the solution of chloride of calcium in which it was formed run off from it, it was ready to be treated with hydrochloric acid, from which it then liberated chlorine, with reproduction of exactly such a "still-liquor" as was commenced with. From this point the series of operations described was to be repeated as before, —and so on continually.

The oxide of manganese so obtained was *hydrated* in an exceedingly fine state of division, and it shared with all such artificial hydrates the property of being far more readily soluble in acids than a hard, compact, anhydrous *native* oxide. In fact, instead of requiring, like the native oxide, long digestion, aided by heat, in a large excess of acid, the artificial oxide dissolved, even in the cold, and with extreme rapidity, in an equivalent of acid, producing a neutral "still-liquor." Hence, the full theoretical yield of chlorine could be practically obtained from simple equivalents of acid and oxide, and this with a considerably less expenditure of time, labour, and fuel than the inferior yield obtained by means of the native oxide required. When using a 70 per cent. native oxide it was rarely found possible to obtain in the free state more than *one-sixth* of the chlorine contained in the hydrochloric acid put into the stills; but an artificial oxide of only 55 per cent. liberated *one-third* of the chlorine contained in the acid put into the stills, and in less time, and at a less cost for labour and fuel, than the native oxide required for the liberation of half the quantity. A 55 per cent. oxide thus enabled a given quantity of hydrochloric acid to yield twice as much bleaching powder as a 70 per cent. native oxide did, saving in this item alone from £5 to £7 per ton of bleaching powder.

This evening Professor Tyndall, F.R.S., will deliver an experimental lecture in the Kinnaird Hall, "*On Matter and Force*." This, although addressed to working men, promises to be the most attractive lecture of the meeting. At the same time there will be an artistic and industrial exhibition and soiree in the Volunteer Hall. I have just returned from a private view of this, and consider that it will compare favourably with similar exhibitions at former meetings of the Association. The walls of the large Hall are covered with paintings, ancient and modern, of the most valuable description. Amongst the paintings was an excellent likeness of the late Professor Faraday, but it was perched in such an elevated position that few would notice it. This might surely have taken the place of some of the local worthies on the line.

There are also several excellent photographs exhibited, including specimens of Mr. Woodbury's new micro-photosculpture process, or mode of representing in relief microscopic objects; (these will be described in the chemical section;) a very fine example of the Woodbury-type, from a negative by W. Bingham. Mr. Spiller exhibits a selection of military photographs; Mr. H. Butler shows some beautiful photolithographic reductions; and Mr. H. B. Pritchard photographs upon silk, satin, cambric, etc. There are also exhibited a large collection of fossils, shells, fishes and reptiles preserved in spirit, and other objects of interest.

Dundee, Sept. 12, 1867.

The meeting of the British Association, which has just terminated, must be regarded as most successful either from a scientific, financial, or social point of view. The actual number of members and associates at this meeting is 2,444 (this nearly equals the Aberdeen meeting (2,580) in the year 1859, when the Prince Consort was President): and the happy combination of science and relaxation which each day's programme has provided will cause the past week to be remembered with pleasure.

The attendance in the Sections was better than could have been anticipated, and the earnestness of the frequenters of Section B was shown, if not by the large attendance, at all events by the number of papers set down each morning to be read, and by the length and interest of the discussions which the more important of these papers elicited.

Every year the advantage of these autumnal meetings becomes more and more apparent. The value does not, however, arise from a diligent attendance at the Sections, but from those impalpable influences which result from a lounge in the reception-room—a picnic or excursion to some place of note in the neighbourhood—a look in at the B.'s, the Red Lions, or the Eastern Club. Men, who before only knew each other in the pages of a scientific journal, here meet in friendly companionship, and the keen scientific antagonist becomes a personal friend for life. A controversy which has been dragging on for years is settled by ten minutes' personal explanation; and opponents who are rapidly approaching the orthodox scientific intensity of hatred, carry away from such a meeting mutual forbearance and respect. These are precious results, and if the sections are of no other use, they have the inestimable advantage of drawing men of kindred pursuits together from all parts of the kingdom, and giving them an excuse for a week's holiday under the convenient pretence of attending a scientific meeting.

The liberality extended to visitors by residents in the neighbourhood has been unbounded, every mansion having been full of guests. Lord Kinnaird has shown especial hospitality to the frequenters of Section B, and amongst the chemists staying at his seat—Rossie Priory—have been Dr. Angus Smith, Mr. Crookes, Mr. Spiller, and Mr. Ansell, the inventor of the fire-damp indicator.

Last week I mentioned that the reports of the Council and of the Parliamentary Committee had been presented to the Association. These contain some remarks which cannot fail to interest those readers of the Chemical News who are advocating the introduction of scientific education into schools. At the last meeting of the Association, the committee of recommendations referred to the Council certain resolutions which had been adopted by the committees of two sections relative to the teaching of natural science in schools. The Council, fully impressed with the importance of the subject, appointed a special committee for the purpose of inquiring into the question, and of preparing a report thereon. This committee consisted of the general officers of the association, the trustees, the Rev. F. W. Farrar, M.A., F.R.S., the Rev. T. N. Hutchinson, M.A., Professor Huxley, F.R.S., Mr. Payne, Professor Tyndall, F.R.S., and Mr. J. M. Wilson, M.A. 'The Council having considered the report presented by this committee, adopted the recommendations contained therein, and resolved that the report be submitted to the general committee at Dundee. These recommendations were given in our last number.

The Parliamentary Committee state that the attention of the public appears to have been awakened to the necessity for introducing scientific teaching into our schools, if we are not willing to sink into a condition of inferiority as regards both intellectual culture and skill in art, when compared with foreign nations. The voluntary efforts of the masters of two of our great schools to add instruction in natural science to the ordinary classical course are deserving of all praise; and some evidence of their success may be derived from the interesting fact—disclosed in the able report of the committee appointed by the Council of the Association to consider this subject—that some of the boys at Harrow have formed themselves into a voluntary association for the pursuit of science.

This has formed the subject of a special discussion in the Committee of Section B, who have recommended "that a committee be appointed to inquire into the present methods of teaching chemistry and physics in schools of various classes, and to suggest the best means of furthering it in accordance with the recommendations of the report."

I mentioned last week that a lecture was to be delivered on Thursday evening,

"*On Matter and Force.*" By Professor Tyndall, F.R.S.

This lecture was not addressed to members of the British Association, but to working men, of whom nearly 3,000 were present, the large Kinnaird Hall being crowded to suffocation. To give the lecture at length would occupy more pages than can be well spared. I cannot refrain, however, from quoting some extracts from one of the most eloquent addresses which this gifted orator has ever delivered. The subject matter of the lecture offered no special interest to those who are in the habit of listening to this philosopher in his home—the Royal Institution, but not a word of experiment was lost upon the auditors, who testified their admiration of the noble and high-toned sentiments by silent rapt attention, and of the brilliant experiments by enthusiastic applause, occasionally so prolonged that the lecturer was obliged to beg them to moderate their cheering, or he should be obliged to extend the hour and a half allotted to him to two hours. Professor Tyndall's language is always elevated in tone, and his speculations bold and fearless; and those who listened to the climax of tumultuous applause which followed his splendid peroration, looked in vain for any evidence of that intolerance of free-thought which is supposed to be a national characteristic north of the Tweed.

Professor Tyndall commenced by remarking on the avidity with which the working men of London seized the opportunities afforded them by the evening lectures delivered every year by the Professors in the Royal School of Mines. It was a noteworthy fact that these lectures were but rarely of a character which could help the artisan in his daily pursuits. It was a pure desire for knowledge, as a thing good in itself, and without regard to its practical application which animated these men.

"They wish to know," he continued, "more of the wonderful universe around them; their minds hunger for this knowledge as naturally as their bodies hunger for food, and they come to us to satisfy this intellectual want. It is easily argued as a plea for science that it affords great material benefits. So it does. No doubt of it. Without science your Dundee would be a very small place indeed. But still the scientific discoverers—those high priests, so to say, of Science—they are, I assure you, in this work very rarely actuated by a desire for practical application at all. Take that great man whose name I can hardly trust my lips to utter—that man whom I loved—and who has lately gone from this earth—that man Faraday. That man—a poor book-binder's apprentice—has done more for practical science than dozens of your practical men added together; and he has done it without ever caring to gain a shilling by it. He did it because he loved science; and I say it behoves the Legislature to know that the real workers in science are not those who are always trying to turn it to a practical account. They love science for itself, and their desire is to understand and know all the phenomena of this glorious universe.

Whether it be a consequence of long-continued development, or whether it be an endowment conferred once for all on man at his creation, we find him here gifted with a mind, curious to know the cause of things, and surrounded by things which excite its questionings, and raise the desire for an explanation. It is related of a young Prince of one of the Pacific Islands, that when he first saw himself in a looking-glass, he ran round the glass to see who was standing at the back He wished to know the cause of what he saw. And thus it is with the general human intellect when it regards and ponders the phenomena of the external world. It wishes to know the causes and connections of these phenomena. What is the sun, what is the earth, what should we see if we came to the edge of the earth and looked over? What is the meaning of thunder and lightning, of hail, rain, storm, and snow? Such questions early presented themselves to men, and by and by it was discovered that this desire for knowledge was not implanted in vain. After many trials it became evident that man possessed the power of solving such questions—that within certain limits the secret of the universe was open to the human understanding. It was found that the mind of man had the power of penetrating far beyond the boundaries of his five senses; that the things which are seen in the material world depend for their action upon things unseen; in short, that besides the phenomena which address the senses, there are laws and principles and processes which do not address the senses, but which must be, and can be, spiritually discerned.

Now, there are two thing which form, so to say, the substance of all scientific thought. The entire play of the scientific intellect is confined to the combination and resolution of the ideas of matter and force. Newton, it is said, saw an apple fall. To the common mind this presented no difficulty and excited no question. Not so with Newton. He observed the fact; but one side of his great intellectual nature was left unsatisfied by the mere act of observation. He sought after the principle which ruled the fact. Whether this anecdote be true or not, it illustrates the fact that the ordinary operations of nature, which most people take for granted as perfectly plain and simple, are often those which most puzzle the scientific man. To the conception of the matter of the apple Newton added that of the force that moved it. The falling of the apple was due to an attraction exerted mutually between the apple and the earth. He applied the idea of this force to suns and planets and moons, and showed that all their motions were necessary consequences of the action of this force of attraction. He proved that the planetary motions were what observation made them to be, because every particle of matter in the solar system attracts every other particle by a force which varies as the inverse square of the distance between the particles. He showed that the moon fell towards the earth, and that the planets fell towards the sun, through the operation of the same force that pulls an apple from its tree. And this all-pervading force, the conception of which was necessary to Newton's intellectual peace, is called the force of gravitation. All force may be ultimately reduced to a push or a pull in a straight line; but its manifestations are various, and sometimes so complex as entirely to disguise its elementary constituents.

Long thinking and experimenting on the materials which compose our world have led philosophers to conclude that matter is composed of atoms from which, whether separate or in combination, the whole material world is built up. The air we breathe, for example, is mainly a mixture of the atoms of two distinct substances, called oxygen and nitrogen. The water we drink is also composed of two distinct substances, called oxygen and hydrogen. But it differs from the air in this particular, that in water the oxygen and hydrogen are not *mechanically* mixed, but *chemically* combined. In fact, the atoms of oxygen and those of hydrogen exert enormous attractions on each other, so that when brought into sufficient proximity they rush together with an almost incredible force to form a chemical compound.

One consequence of the clashing together of the atoms is the development of a great amount of heat. What is this heat? How are we to figure it before our minds? I do not despair of being able to give you a tolerably distinct answer to this question. Here are two ivory balls suspended from the same point of support by two short strings. I draw them thus apart and then liberate them. They clash together, but, by virtue of their elasticity, they quickly recoil from each other, and a sharp vibratory rattle succeeds their collision. This experiment will enable you to figure to your mind a pair of clashing atoms. We have, in the first place, a motion of the one atom towards the other—a motion of translation, as it is usually called. But when the atoms come sufficiently near each other, elastic repulsion sets in, the motion of translation is stopped and converted into a motion of vibration. To this vibratory motion we give the name of heat. Thus, three things are to be kept before the mind—first, the atoms themselves; secondly, the force with which they attract each other; and thirdly, the motion consequent on the exercise of that force. This motion must be figured first as a motion of translation, and then as a motion of vibration; and it is not until the motion reaches the vibratory stage that we give it the name of heat. It is this motion imparted to the nerves that produces the sensation of heat.

It would be useless to attempt a more detailed description of this molecular motion. After the atoms have been thrown into this state of agitation, very complicated motions must ensue from their incessant collision. There must be a wild whirling about of the molecules. For some time after the act of combination, this motion is so violent as to prevent the molecules from coming together. The water is maintained for a time in a state of vapour; but as the vapour cools, or, in other words, loses its motion, the water molecules coalesce to form a liquid. And now we are approaching a new and wonderful display of force. No one who had only seen water in its vaporous or liquid form could imagine the existence of the forces to which I am now about to refer; for, as long as the substance remains in a liquid or vaporous condition, the play of these forces is masked by the agitation kept up by the heat among the molecules. But let the heat be gradually withdrawn, the antagonist to their union being removed, the molecules begin to form new combinations. Like the particles of iron in our magnetic experiment, the water molecules are endowed with attractive and repulsive poles, and they arrange themselves together in accordance with these attractions and repulsions. Solid crystals of water are thus formed, to which we give the familiar name of ice. To the eye of science, these ice crystals are as precious as the diamond—as purely formed, as delicately built. Where no disturbing causes intervene, there is no disorder in this crystalline architecture. By their own structural power molecules build themselves on to molecules with a precision infinitely greater than that attainable by the hands of man. We are apt to overlook the wonderful when it becomes common. Imagine the bricks and stones of this town of Dundee endowed with locomotive power. Imagine them attracting and repelling each other, and arranging themselves in consequence of these attractions and repulsions so as to form streets and houses and Kinnaird Halls; would not that be wonderful? No less wonderful is the play of force by which the molecules of water build themselves into the sheets of crystal which roof your ponds and lakes every winter. To use the language of the American poet, Emerson, "the atoms march in tune," moving to the music of law, which thus renders the commonest substance in nature a miracle of beauty to the mental eye. It is the function of science, not as some think to divest this universe of its wonder and its mystery, but, as in the case here before us, to point out the wonder and the mystery of common things.

Over a plate of perfectly clean glass I pour a little water in which a crystal has been dissolved. A film of the solution clings to the glass; and now I wish to make this film crystallise before your eyes. By means of a microscope and electric lamp, I throw an image of the plate of glass upon the screen. The beam of the lamp, besides illuminating the glass, also heats it; evaporation is thereby promoted, and, at a certain moment, when the solution has become supersaturated, splendid branches of crystals shoot out over the screen. A dozen square feet of surface are now covered by those beautiful forms. Here we have crystalline spears shooting over the screen, feathered right and left by other spears. Molecule thus closes with molecule, until, finally, the whole subsides into crystalline rigidity. I move a new portion of the film into the beam. From distinct nuclei in the middle of the field of view the crystalline spears shoot with magical rapidity in all directions. For a moment the whole film appears to be alive, and now it has sunk into molecular repose. The film of water on a window pane on a frosty morning exhibits effects quite as wonderful as these. Latent in this formless solution, latent in every drop of water lies this marvellous structural power, which only requires the withdrawal of opposing forces to bring it into action. These experiments show that the common matter of our earth—"brute matter," as Dr. Young calls it—when its atoms and molecules are permitted to bring into free play the forces with which they are endowed, arranges itself under the operation of these forces, into forms which rival in beauty those of the vegetable world. And what is the vegetable world itself but the result of the complex play of these molecular forces. Here, as elsewhere throughout nature, if matter moves it is force that moves it; and if a certain structure is produced it is through the operation of the forces with which the atoms and molecules composing the structure are endowed. These atoms and molecules resemble little magnets with mutually attractive and mutually repellent poles. The attracting poles unite, the repellent poles retreat from each other, and vegetable forms are the final expression of this complicated play of molecular force. In the formation of our lead and silver trees, we needed an agent to wrest the lead and the silver from the acids with which they were combined. A similar agent is required in the vegetable world. The solid matter of which our lead and silver trees were formed was, in the first instance, disguised in a transparent liquid; the solid matter of which our woods and forests are composed is also, for the most part, disguised in a transparent gas, which is mixed in small quantities with the air of our atmosphere. That gas is formed by the union of carbon and oxygen, and is called carbonic acid gas. Two atoms of oxygen and one of carbon unite to form the molecule of carbonic acid which, as I have said, is the material from which wood and vegetable tissues are mainly derived. The carbonic acid of the air being subjected to an action somewhat analogous to that of the electric current in the case of our lead and silver trees, has its carbon liberated and deposited as woody fibre. The watery vapour of the air is subjected to a similar action; its hydrogen is liberated from its oxygen, and lies down side by side with the carbon in the tissues of the tree. The oxygen in both cases is permitted to wander away into the atmosphere. But what is it which thus tears the carbon and the hydrogen from the strong embrace of the oxygen? What is it in nature that plays the part of the electric current in our experiments? The rays of the sun. The leaves of plants absorb both the carbonic acid and the aqueous vapour of the air; these leaves answer to the cells in which our experiments on decomposition by the electric current took place. In the leaves the solar rays decompose both the carbonic acid and the water, permitting the oxygen in both cases to escape into the air, and allowing the carbon and the hydrogen to follow the bent of their own forces. And just as the molecular attractions of the silver and the lead found expression in the production of those beautiful branching forms seen in our experiments, so do the molecular attractions of the liberated carbon and hydrogen find expression in the architecture of grasses, plants, and trees.

In the fall of a cataract or the rush of the wind we have an example of mechanical power. In the combinations of chemistry and in the formation of crystals and vegetables we have examples of molecular power. Before proceeding further I should like to make clear to you the present condition of the surface of the globe with reference to power generally. You have learned how the atoms of oxygen and hydrogen rush together to form water. I have not thought it necessary to dwell upon the mighty mechanical energy of their act of combination, but, in passing, I would say that the clashing together of 1 lb. of hydrogen and 8 lbs. of oxygen to form 9 lbs. of aqueous vapour, is greater than the clash of a weight of 1,000 tons falling from a height of 20 feet against the earth. Now, in order that the atoms of oxygen and hydrogen should rise by their mutual attractions to the velocity corresponding to this enormous mechanical effect, a certain distance must exist between the particles. It is in rushing over this distance that the velocity is attained. This idea of distance between the attracting atoms is of the highest importance in our conception of the system of the world. For the world may be divided into two kinds of matter; or rather the matter of the world may be classified under two distinct heads—namely, of atoms and molecules which have already rushed together and thus satisfied their mutual attractions, and of atoms and molecules which have not yet rushed together, and whose mutual attractions are, therefore, as yet unsatisfied. Now, as regards motive power, the working of machinery, or the performance of mechanical work generally by means of the materials of the earth's crust, we are entirely dependent on those atoms and molecules whose attractions are as yet unsatisfied. Those attractions can produce motion, because sufficient distance intervenes between the attracting molecules, and it is this molecular motion that we utilise in our machines. Thus we can get power out of oxygen and hydrogen by the act of their union, but once they are combined, and once the motion consequent on their combination has been expended, no further power can be got out of the mutual attraction of oxygen and hydrogen. Their mutual attractions are then satisfied, and as dynamic agents they are dead.

Now, if we examine the materials of which the earth's crust is composed, we find them to consist for the most part of substances whose atoms have already closed in chemical union—whose mutual attractions are satisfied. Granite, for instance, is a widely diffused substance; but granite consists, in great part, of silicon, oxygen, potassium, calcium, and aluminium, the atoms of which substances met long ago in chemical combination, and are therefore dead. Limestone is also a widely-diffused substance. It is composed of carbon, oxygen, and a metal called calcium. But the atoms of those substances closed long ago in chemical union, and are therefore dead. And in this way we might go over the whole of the materials of the earth's crust, and satisfy ourselves that though they were sources of power in ages past, and long before any being appeared on the surface of the earth capable of turning their power to account, they are sources of power no longer. And here we might halt for a moment to remark on that tendency so prevalent in the world, to regard everything as made for human use Those who entertain this notion hold, I think, an overweening opinion of their own importance in the system of nature. Flowers bloomed before men saw them, and the quantity of power wasted before men could utilise it is all but infinite compared with what now remains to be applied. The healthy attitude of mind with reference to this subject is that of the poet, who, when asked whence came the rhododendron, replied—

> "Why wert thou there, O rival of the rose?
> I never thought to ask, I never knew,
> But in my simple ignorance supposed
> The self-same power that brought me there brought you."

A few exceptions to this general state of union of the particles of the earth's crust—all-important to us, but trivial in comparison to the total store of which they are but the residue—still remain. They constitute our main sources of motive power. By far the most important of these exceptions are our beds of coal, composed chiefly of carbon, which has not yet closed in chemical union with oxygen. Distance still intervenes between the atoms of carbon and those of oxygen, across which the atoms may be impelled by their mutual attractions; and we can do nothing more than utilise the motion produced by this attraction. Once the carbon and the oxygen have closed together, so as to form carbonic acid, their mutual attractions are satisfied; and while they continue in this condition, as dynamic agents they are dead. Our woods and forests are sources of mechanical energy, because they also have the power of uniting with the atmospheric oxygen, and the molecular motion produced in the act of union may be turned to mechanical account. And let it be remembered that the source of motive power here referred to is also the source of muscular power. A horse can perform work, and so can a man; but this work is at bottom the molecular work of the elements of the food and the oxygen of the air. We inhale this vital gas. We bring it into sufficiently close proximity with the carbon and the hydrogen of the food. They unite in obedience to their mutual attractions, and their motion towards each other, properly turned to account by the wonderful mechanism of the body, becomes muscular motion.

One fundamental thought pervades all these statements: there is one tap-root from which they all spring. This tap-root is the ancient maxim that out of nothing nothing comes; that neither in the organic world nor in the inorganic is power produced without the expenditure of other power; that neither in the plant nor in the animal is there a creation of force or motion. Trees grow, and so do men and horses; and here we have new power incessantly introduced upon the earth. But its source, as I have already stated, is the sun. For he it is who separates the carbon from the oxygen of the carbonic acid, and thus enables them to recombine. Whether they recombine in the furnace of the steam-engine or in the animal body, the origin of the power they produce is the same. In this sense we are all "souls of fire and children of the sun." But, as remarked by Helmholtz, we must be content to share our celestial pedigree with the meanest living thing. The frog, and the toad, and those terrible things, the monkey and the gorilla, draw their power from the same source as man.

Some estimable persons here present very possibly shrink from accepting these statements; they may be frightened by their apparent tendency towards what is called materialism—a word which to many minds expresses something very dreadful. But it ought to be known and avowed that the physical philosopher, as such, must be a pure materialist. His inquiries deal with matter and force, and with them alone. The action which he has to investigate is necessary action, not spontaneous action—the transformations, and not the creation, of matter and force. And whatever be the forms which matter and force may assume, whether in the organic world or the inorganic, whether in the coal beds and forests of the earth or in the brains and muscles of men, the physical philosopher will make good his claim to investigate them. It is perfectly vain to attempt to stop investigation as to the actual and possible combinations of matter and force. Depend upon it, if a chemist, by bringing the proper materials together, could produce a baby he would do it. And why not? There is no command forbidding him to do it—his inquiries in this direction are limited solely by his own capacity and the inexorable laws of matter and force. At the present moment there are, no doubt, persons experimenting on the possibilities of producing what we call life out of inorganic materials. Let them pursue their studies in peace; it is only by such trials that they will learn the limits of their powers.

But while I thus make the largest claim for freedom of investigation—while I as a man of science feel a natural pride in scientific achievement, while I regard science as the most powerful instrument of intellectual culture, as well as the most powerful ministrant to the material wants of men, if you ask me whether science has solved, or is likely to solve, the problem of this universe, I must shake my head in doubt. We have been talking of matter and force; but whence came matter, and whence came force? You remember the first Napoleon's question, when the *savans* who accompanied him to Egypt discussed in his presence the problem of the universe, and solved it to their apparent satisfaction. He looked aloft to the starry heavens, and said—"It is all very well, gentlemen, but who made all these?" That question still remains unanswered, and science makes no attempt to answer it. As far as I can see, there is no quality in the human intellect which is fit to be applied to the solution of the problem. It entirely transcends us.

The mind of man may be compared to a musical instrument with a certain range of notes, beyond which in both directions we have an infinitude of silence. The phenomena of matter and force lie within our intellectual range, and as far as they reach we will at all hazards push our enquiries. But behind, and above, and around all, the real mystery of this universe remains unsolved; and here the true philosopher will bow his head in humility, and admit that all he can do in this direction is no more than what is within the compass of an ordinary child. Fashion this mystery as you will, with that I have nothing to do. But be careful that your conception of the Builder of this universe is not an unworthy conception. Invest that conception with your grandest, and highest, and holiest thought, but be careful of pretending to know more about it than is given to man to know. Be careful, above all things, of professing to see in the phenomena of the material world the evidences of Divine pleasure or displeasure. Doubt this all ye who would deduce from the fall of the Tower of Siloam, the sin and wickedness of those who were crushed by the fall! Doubt this all ye who pretend to see in cholera, cattle plague, and bad harvests, the evidence of Divine anger! Doubt this—but it requires some courage to say these things in Scotland—all ye who assert that the depreciation of railway scrip is a consequence of railway travelling on Sundays. You know nothing about it. To such I say in substance, what was said by one of the mightiest Scotchmen living or dead—Thomas Carlyle—to the followers of Dr. Pusey—

> "The Builder of this universe was wise,
> He formed all souls, all systems, planets, particles;
> The plan he formed his worlds and Æons by,
> Was—Heavens!—was thy small nine-and-thirty articles!"

I now resume my report of the proceedings.

SECTION B. CHEMICAL SCIENCE.

Friday, September 6th.

The following papers were read:—

"*On the present use of Lichens as Dye Stuffs*," by Lauder Lindsay.

In the absence of the author this paper was read by Dr. Odling. He said—It had been expected that the aniline dyes—a product from the distillation of coal tar—discovered a few years ago, would supersede the lichenous dye stuffs previously in use, and that the latter would speedily disappear, in consequence of the breaking up of the Highlands by railways—and the improvement of the communication between Glasgow, Edinburgh, and the south. To him, however, it seemed that all such predictions were at least premature. He confessed that such was the eminence and experience of the authorities, that for a time he acquiesced in their conclusions, and took it for granted that they were well grounded; but in the course of his investigations for a work which he had in preparation on lichenology, he had found that there existed abundant evidence of the use of lichens in commercial manufactures on a large scale, as well as for domestic purposes. Dr. Lindsay then stated that from facts which he had learned at the Exhibition of 1862, from his investigations while on a tour through the Orkney and Shetland Islands, and parts of the Highlands, and from numerous writers whom he quoted, he had come to conclusions favouring the belief that lichens would not be superseded, at least for a long time to come. He then proceeded to give numerous details of the use of lichen dyes for commercial and domestic purposes, to remark on the unsatisfactory state of the nomenclature and classification of the lichens, and to advocate a uniform system.

"*A Note on Messrs. Wanklyn, Chapman, and Smith's Method of Determining Nitrogenous Organic Matters in Water*," by Dugald Campbell, F.C.S.

At the last meeting of the Chemical Society, June 20th, Professor J. A. Wanklyn read an extract from a paper by himself, Mr. E. T. Chapman, and Mr. M. H. Smith, "*On Water Analysis, and the Determination of Organic Matter in Water*," in which they proposed a mode of estimating the amount of nitrogenous matter contained in water, whether it exists as ammonia, or urea, or as albuminous matter; a report of this is given in the Chemical News of July 5, p. 7. (*Am. Reprint, Sept.* 1867, *p.* 134.)

Having for a number of years worked specially on the estimation of nitrogen in solutions containing ammonia, urea, and albuminous matters, I was much struck with the results which were given by these gentlemen, and at the time when the paper was read I expressed my doubts as to the accuracy of their observations, my own having been so diametrically opposite; still, although nothing was said on the reading of the extract about the actions being different with *moderately strong* and *very dilute* solutions, yet after my remarks something was said by one of the gentlemen (Mr. Chapman) in the discussion about strong solutions requiring different treatment; and remembering that my experiments had all been made upon what might be termed moderately strong solutions, certainly not very dilute solutions, I was silent, as I thought this might possibly account, at least to some extent, for the great differences in our results; still I did not think this could wholly account for them.

Since then, however, I have made a number of experiments with very dilute solutions of both urea and albumen, the results of which in a great measure, if not entirely, confirm my views stated at the time, viz. that urea is not perfectly decomposed by distilling with sodic carbonate as described by these gentlemen, and likewise that I had invariably detected ammonia in the distillate from a solution of albuminous matter when it was distilled under similar circumstances.

As regards the first part of the process, which is the estimation of ammonia in the water, and which is in my opinion a very material one, but which appears to me to have been passed over by these gentlemen as a thing of very little importance, all we are told of it is that "when the quantity of ammonia present in the original water is large it is determined directly, unless the water is too much coloured." In all the experience which I have had in determining the amount of ammonia in water, by Nessler's test, working on the usual scale, I have never been able to apply it with any degree of accuracy to the original water, but always had to distil the water with some alkali and test the distillate; and this I know is the general experience; and if urea is so readily broken up in the distillation of water, as it is said to be by these gentlemen, I should have thought that some means of distilling the water in order to obtain from it the ammonia, without decomposing the urea, should have been given, and that a process for estimating the ammonia, urea, and albuminous matters in water would scarcely be complete without it.

After the determination of the ammonia, the next operation, we are told, is to introduce a litre of the water "into a retort with two grammes of sodic carbonate, and rapidly distil, the distillate being collected in a flask containing 100 c.c. When this is filled the receiver is changed, and another 100 c.c. distilled off; this is repeated a third time, and it is now found that all the nitrogen present in the original water, in the form of urea, has passed over in the form of ammonia. The ammonia in the distillate is then determined by Nessler's test."

I may state that in Parliament, and generally throughout the kingdom, when reports are made upon the analysis of waters, the results are required to be given in grains, or parts of a grain, in a gallon of the water of 70,000 grains at 60° Fahrenheit, and they are thus much more readily understood by engineers and the public generally, who are interested in these matters, than when otherwise expressed. I have, therefore, in the following experiments thought it best to state that the waters operated upon contained so many parts of a grain of the substance being experimented upon per gallon, and also to use grains in making the solutions in other parts of the experiments; but I have retained the litre and cubic centimeter measures, as I wished to follow explicitly the experiment as it is given above, as to the quantity of water taken for distillation and the amount of each of the distillates, in case anything might be said of my not having done so.

The first experiments were made with a solution of urea equal to the $\frac{1}{20}$th part of a grain of urea in a gallon of water, Prof. Wanklyn having given this as a quantity by which his process could be tried, and with which he said it would work satisfactorily, as it had been described in their paper. The following are the details and results:—

A litre of water was taken in which was dissolved an amount equal to 0·011 grain of urea containing the elements of 0·0062 grain of ammonia, and introduced into a retort capable of holding twice this quantity. 30·87 grains (two grammes) of sodic carbonate were then added, and the distillation was proceeded with rapidly, and the distillate collected exactly as described above. The ammonia which was found only in the two first 100 c.c., and estimated by Nessler's test, gave 0·0026 grains, or about one-third of the ammonia present in the urea taken.

There being no trace of ammonia in the third 100 c.c., I should from the statements of Messrs. Wanklyn, Chapman, and Smith, have concluded that all the urea was decomposed into ammonia, had I not known the quantity of urea which was taken at the outset, and also from my former experiments upon stronger solutions of urea which acted very similarly; but knowing this, a solution of potassic hydrate was introduced into the retort, and the distillation proceeded with exactly as before described; the two first 100 c.c., which were distilled over, contained 0·002 grains of ammonia, and the third distillate containing no ammonia; potassic permanganate was added in crystals to the liquid in the retort until it was deeply coloured, and the distillation was again proceeded with as above; the remainder of the ammonia, which was estimated at 0·0015 grain, was in the first and second, but principally in the first 100 c.c., and none in the third. Altogether the ammonia obtained was 0·0061 grain, the calculated quantity being 0·0062 grain.

This experiment was repeated as above described, but with somewhat different results, the distillations with the sodic carbonate giving 0·0021 grain, the potassic hydrate 0·0016 grain, and the potassic permanganate 0.0025 grain, altogether 0·0062 grain of ammonia, which is exactly the quantitity of ammonia in the urea taken.

Experiments were next made with a solution of urea equal to $\frac{1}{40}$th part of a grain of urea in a gallon of water, and the following are the details and results:—

A litre of water was taken in which was dissolved a quantitity equal to 0·0055 grain of urea, containing the elements of 0·0031 grain of ammonia, and distilled as above with sodic carbonate; in the first two 100 c.c., the ammonia present amounted to 0·001 grain; in the third 100 c.c., there was no ammonia; potassic hydrate gave in the first two 100 c.c. 0·001 grain of ammonia, in the third none, and potassic permanganate gave in the first two 100 c.c., but principally in the first, 0·0011 grain of ammonia, making altogether 0·0031 grain of ammonia, which is the exact quantity of ammonia in the urea taken.

This experiment was repeated exactly as above, but as in the former case, with slightly different results; sodic carbonate gave 0·001 grain, potassic hydrate gave 0·0001 grain, and potassic permanganate 0·002 grain ammonia, making altogether 0·0031 grain of ammonia.

Experiments were next made with a solution of urea equal to $\frac{1}{80}$th part of a grain of urea in a gallon of water, and the following are the details and results:—

A litre of water was taken, in which was dissolved an amount equal to 0·0025 grain of urea, containing 0·00155 grain of ammonia, and treated in the same manner as in the preceding experiments; with sodic carbonate it gave 0·0013 grain ammonia in the first two 100 c.c., and no ammonia in the third; with potassic hydrate 0·0001 grain in the first two 100 c.c., and none in the third; and with potassic permanganate, gave 0·0001 grain of ammonnia; altogether within a fraction of the amount of ammonia in the urea employed.

This experiment was repeated, and the results were as follows:—With sodic carbonate, 0·0008 grain, with potassic hydrate 0·0003 grain, and with potassic permanganate 0·0004 grain; altogether 0·0015 grain of ammonia, and making the same amount of ammonia as the last, but evolved differently.

Experiments were next made with a solution of urea equal to the 100th part of a grain of urea in a gallon of water, and the following are the details and results:—

A litre of water was taken in which were dissolved 0·0022 grain of urea containing 0·00136 grain of ammonia, and distilled as above; with sodic carbonate in the first two 100 c.c. there were 0·001 grain of ammonia, and no ammonia in the third 100 c.c.; and by potassic hydrate in the first two 100 c.c. 0·0003 grain; whilst by potassic permanganate there was no ammonia. The ammonia distilled off was altogether 0.0013 grain, which is a fraction less ammonia than was contained in the urea employed.

This experiment was repeated with a nearly similar result.

Experiments were next made with a solution of urea equal to the $\frac{1}{120}$th part of a grain of urea in a gallon of water, and likewise with still smaller portions of urea, when it was found in all cases that with sodic carbonate all the ammonia contained in the urea distilled over in the first 100 c.c.

From these experiments it would appear that urea is decomposed entirely when distilled in the ordinary manner with sodic carbonate, but only when in extremely dilute solutions; requiring the assistance of potassic permanganate to decompose it even when so dilute as in the proportion of the $\frac{1}{80}$th part of a grain of urea in a gallon of water, or one part in five millions; and requiring the assistance of potassic hydrate to decompose it even when diluted to an extent above one part in seven millions; and that is only decomposed by sodic carbonate alone when diluted somewhat above this point.

I may add that these and all other experiments were made with distilled water, which tried in every way showed not a trace of ammonia by Nessler's test, and that every experiment was made by dissolving at the time fresh crystals of urea; or, in other words, that no solution of urea was employed which had been made or kept any time. I may also add that I found a considerable difference in the various specimens of urea which I examined; all gave distinct indications of ammonia by the Nessler test; the larger, less coloured, and finer the crystals the more was the ammonia. I need scarcely add that working upon such very dilute solutions as I was doing, the latter specimens yielded sensibly more ammonia by the sodic carbonate and potassic hydrate than those showing less indications of ammonia by the Nessler test; still I do not doubt that working with any ordinary urea, the results will not be far different from my own, although variable.

The next part of the process to which I wish to call attention is the estimation of what is termed the "albuminous substances."

According to Messrs. Wanklyn, Chapman, and Smith, albumen, such as from white of egg, yields no ammonia, ascertained by Nessler's test, when a solution of albumen is distilled with the proportions of sodic carbonate, 30·87 grains (2 grammes), which they say they use to decompose the urea; whereas, as I stated before, in all my experiments I had invariably found that ammonia was evolved under such circumstances. I may observe that hitherto my experiments had been made upon what might be termed rather strong solutions (one grain of dry albumen in a gallon of water), and hence I was led to make the following experiments with what I conceive to be weak solutions:—

The first experiment was made with a quantity about equal to the $\frac{1}{20}$th part of a grain of dry albumen to 1 gallon of water.

100 grains of the white of new-laid eggs, dried, gave 12·00 grains of dry residue containing 1·884 grains of ammonia; or 100 grains of such white of egg contained 1·884 grains of ammonia.

A litre of water was taken in which was dissolved an amount equal to 0·093 grain of white of egg containing 0·00175 grain of ammonia, and was distilled with 30·87 grains (2 grammes) of sodic carbonate; in the two first 100 c.c. of the distillate 0·0006 grain of ammonia were estimated by Nessler's test, in the third 100 c.c. there was not a trace of ammonia; potassic hydrate was then added, and the distillation was gone on with as before, when not a trace of ammonia was discovered in any of the three 100 c.c. distilled; potassic permanganate was then added, and the distillation proceeded with, when it gave, principally in the first distillate, 0·001 grain of ammonia. Altogether there was a loss of 0·00015 grain of ammonia in the albumen.

This experiment was repeated with somewhat similar results, only rather more ammonia was evolved by the sodic carbonate, and again none by the potassic hydrate, and the remainder by the potassic permanganate.

The next experiment was made with about $\frac{1}{40}$th part of a grain of dry albumen to a gallon of water, as follows:—

A litre of water was taken, in which was dissolved an amount equal to 0·0466 grain of white egg, containing 0·00087 grain of ammonia, and distilled with the sodic carbonate. The two first contained 0·0007 grain of the ammonia; potassic hydrate gave no ammonia in any of the three 100 c.c., and potassic permanganate gave 0·0001, which is a loss of 0·00007 of a grain of the ammonia contained in the white of egg employed.

Another experiment was made with a quantity equal to the $\frac{1}{80}$th part of a grain of dry albumen to a gallon of water, as follows:—

A litre of water was taken in which was dissolved an amount equal to 0.0233 grain of white of egg, containing 0·000435 grain of ammonia, and distilled with the sodic carbonate, the first two 100 c.c. gave 0·0002 grain of ammonia. By the potassic hydrate no ammonia was given off, and by the potassic permanganate 0·0002 grain, showing altogether a loss of ammonia equal to 0·000035 grain.

Another experiment was made with a quantity about equal to the $\frac{1}{160}$th part of a grain of dry albumen to a gallon of water, as follows:—

A litre of water was taken in which was dissolved 0·01165 grain of white of egg, containing 0·000217 grain of ammonia, and distilled with the sodic carbonate. In the first two 100 c.c. of the distillate all the ammonia was evolved, in the third 100 c.c. there was none, nor when distilled with potassic hydrate or potassic permanganate was there a trace to be found.

This experiment was repeated and with the same results, all the ammonia being evolved in the first two 100 c.c. when the solution was distilled with sodic carbonate alone.

In all these experiments it will be seen that with the sodic carbonate a distinct quantity of ammonia is evolved, and likewise that in all these experiments, strange though it may appear, after the ammonia has been evolved as far as is possible by the sodic carbonate, that potassic hydrate evolves not even a trace, and it is only on the addition of potassic permanganate that the final quantity of nitrogen is expelled as ammonia. It will likewise be seen that, as in very dilute solutions of urea, all the nitrogen is expelled as ammonia by the sodic carbonate, so likewise this is the case in very dilute solutions of albumen.

The whole of the above experiments, both with the urea and the albumen, were conducted in ordinary retorts of not less than twice the capacity of the liquid to be distilled, and as nearly all alike in shape and size as it was possible to get them, and every endeavour was made to distil the solutions at a rapid speed and as nearly under the same circumstances as possible, otherwise I do not think the results obtained would have been so regular as they are, but as far as I can judge from other experiments quite the reverse; still on the whole they would be generally confirmatory of the results I have obtained.

Since making these experiments, my attention has been directed to a paper by Mr. Chapman, "on Nessler's test for Ammonia," wherein that gentleman says that in comparing a solution to be tested with the ammonia standard, after adding the test he allows the liquids to stand for ten minutes before he compares them. My own experience, and I know it is that of others, is that this time is not nearly sufficient to develope the colour properly, and this is especially the case with very dilute solutions of ammonia; that being so I do not see why such solutions should not be allowed to stand until the colour is fully developed in them, although it may take a longer time. in which case experience has taught us that the results are a deeper and more defined colour, which admits of better, I might almost say, of perfect comparison. In carrying out the above experiments sufficient time was invariably allowed for the proper development of the colour when determining the amount of ammonia in all of the distillates.

"*A Description of a New Ether Anemometer.*"

By ALFRED E. FLETCHER,

GOVERNMENT INSPECTOR OF ALKALI WORKS FOR THE WESTERN DISTRICT.

The construction of this apparatus is based on the fact that a current of air passing across the open end of a straight tube causes a partial vacuum in it.

An application of this principle is seen in a small toy in common use, in which a liquid is made to ascend several inches in a vertical tube by blowing through another tube across its open end. It rises by virtue of the partial vacuum caused by the current of air which crosses it.

If then a straight tube is inserted through a hole in the brickwork of a chimney or flue, so that the current of air in the flue passes across its open end, a partial vacuum will be formed in it, greater or less in proportion to the velocity of the current.

A tube in such a position will, however, communicate a suction arising from that of the chimney itself, besides that suction produced by the current of air passing across its open end, and for the present purpose these two must be distinguished.

To effect this *two* tubes should be inserted in the chimney, one of them having a straight and the other a bent end, the bend to be turned so as to meet the current of air; both tubes are open. In each of these tubes will be experienced the partial vacuum due to the suction of the chimney itself. In the straight tube, however, this will be increased by the suction caused by the passage of the current of air across its open end, while in the case of the bent tube this will be diminished by the pressure caused by the current of air blowing into it. The difference therefore between the suction in the two tubes will be due to the action of the current of air in the chimney, and it remains only to measure this difference in order to measure the velocity of the current itself.

To effect this let these tubes be connected with a U tube containing water, one with each limb; then the water will be raised up in one limb to a degree corresponding with the difference of suction, so that the difference of level of the water in the U tube, being a measure of the difference of suction in the tubes, becomes a measure of the velocity of the current of air in the chimney. By this arrangement the suction power of the chimney itself is eliminated, for it operates equally on each limb of the U tube, while the difference of pressure experienced will be due only to the different action of the current of air in the flue on the tube with the straight end and the one with the bent end.

It remains, then, to register accurately this difference of level of the water in the U tube, and to construct a formula connecting it with the speed of the current of air in the flue, so that by measuring the one the other may be measured also.

Experiment showed that for high speeds of air the measurement of the difference of this water level was easy, but that or speeds below 5 feet per second the amount became too minute and uncertain for practical use.

Many plans were then devised for constructing a pressure gauge which should be more delicate than the ordinary U tube.

Efforts were first made to modify the U tube so that its range might be increased and its indications magnified. This might be done by drawing out its lower bend horizontally and increasing the size of the vertical portions till it assumed the form of two vertical cylinders connected by a long horizontal tube. If now a pressure were exerted which would cause a depression of the water in one limb, the motion so caused in the narrow column of water in the horizontal tube would be so much greater, as its sectional area was smaller than that of the vertical tubes. It was found, however, that in proportion as a greater range in the scale of the instrument was thus obtained, a greater amount of friction must also be encountered, and that thus the advantage of the one was neutralised by the evil of the other.

It is necessary to see this clearly in order to arrive at the conclusion that *all* methods of increasing the actual motion of the fluids or of magnifying it by any mechanical arrangement of levers or otherwise, must be open to the same objection. This proposition seems clear now, in the light shed by a long series of failures encountered in the attempt to act contrary to it, but it was not clear before.

The simple U tube was therefore returned to, and means adopted for accurately seeing and measuring its slightest indications. In the first place, the limbs were increased until they were no longer small tubes of about 0·4 inch internal diameter, but cylinders of 4 inches diameter; these were connected at the bottom by a small tube. Thus the power exerted by the pressure communicated through the connecting tubes, operating on the extended surface of the liquid in the cylinders, was increased a hundred-fold over that operating in the smaller U tube; but the friction could only have been increased tenfold, giving therefore a tenfold increase of delicacy. In order to observe accurately the rise and fall of the liquid in the cylinders floats were introduced, on each of which was engraved a very fine horizontal line; and to measure accurately the comparative elevation or depression of these two lines, a finely divided scale and vernier were added, working with a delicate screw adjustment. With this it is possible to measure an elevation or depression of $\frac{1}{1000}$ inch, which is sufficiently accurate for the purpose in view.

On trying now to apply the instrument so constructed, and attempting to measure very minute variations of pressure, failure still seemed imminent; for although the motion of the water in the increased limbs of the U tube could be measured to $\frac{1}{1000}$ inch, the water refused to move, except under pressures exceeding that which would be indicated by so small a column; in other words, the water seemed to stick in the cylinders. It was necessary, therefore, to make experiments with various liquids in order to choose one more suitable than water; for this purpose a very thin plate of metal was suspended from the beam of a delicate balance, and the amount of power required for its immersion in, and subsequent withdrawal from, various liquids thus measured. This resistance is due to what is often called capillary attraction and repulsion; it is shown to exist largely in water, by the fact that a needle may be made to rest on its surface without sinking. In the case of water 20 grains were needed to overcome it, while with many other liquids a much less force sufficed, and in the case of ether $\frac{1}{100}$ grain was sufficient. Ether was, therefore, chosen as the liquid which offered the least resistance, and also on account of its low specific gravity.

After substituting ether for water, the action of the manometer was quite satisfactory, the lines on the floats always return exactly to their original position after any disturbance, and its indications could be relied on to $\frac{1}{1000}$ inch.

It remained now to ascertain the value of these indications when applied to the measurement of the velocity of air.

Calculation might lead to this, but it was thought much better to depend, if possible, on actual experiment, and by testing the instrument with currents of air of known velocity to draw up a table for future use.

The following plan was adopted and found perfectly successful:—

A flue 14 inches diameter, 100 feet long, was constructed of iron pipes, one end was connected with the base of a high chimney, the other left open. A sliding plate of metal, capable of cutting off the connection between the chimney and the flue, served to regulate at will the amount of air drawn through it. At the open end of the flue a red-hot brick was placed, and on it was thrown at stated times a few drops of sulphuric acid. This raised instantly a dense cloud of white vapour, which, passing along the flue, could be observed to reach the other end, by looking through two holes bored through opposite sides of it. It remained now only to note the time occupied by the passage of the cloud of vapour along the flue to know the speed at which the air was passing through it. At the same time the tubes in connection with the ether manometer were in the flue and under the influence of the same current of air, so that the simultaneous reading of the instrument could be taken. By now altering the position of the slide which regulated the admission of air to the flue, the speed from time to time was altered, and a corresponding observation by the manometer obtained.

By the aid of this ether manometer the speed of any current of air in flues or chimneys can be measured by simply boring a hole one inch diameter through the brickwork and inserting two tubes, one with a bent, the other with a plain straight end as already described, and making the necessary observation of the floats; and in this operation neither soot, heat, nor corrosive vapours can prove any hindrance.

So sensitive is the apparatus that on a windy day the effect of each successive gust of wind is observable, as it causes variations in the draught of the chimney.

The instrument may be used as a wind gauge by fixing through the roof of an observatory a small vertical pipe, presenting a plain open end to the wind. The lower end of this pipe brought down into the observatory and connected with the ether manometer would communicate the varying pressures due to the varying speed of the wind.

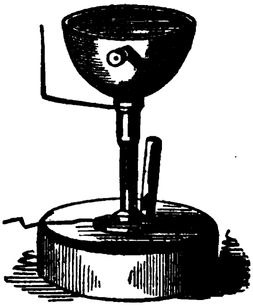
"*On an Apparatus for Indicating the Presence and Amount of Fire-damp in Mines,*" by George F. Ansell.

The idea embodied in the apparatus was founded on the law of diffusion announced by Mr. Graham, that gases diffuse in the inverse proportion to the square root of their densities, or, more popularly, that light gases diffuse more rapidly than heavy ones. Mr. Ansell showed, by experiment, that when a tube closed at one end by plaster of Paris was filled with common coal gas, the lighter part of the compound was rapidly diffused through the plaster, as was at once seen by the yellow flame and slight explosion which ensued on bringing a lighted match close to the closed end. Hence, Mr. Ansell said, his proposition. In a pit the case is the reverse of that of the tube. There the gas is ready to escape into the galleries, and the apparatus must therefore be modified to suit the varying circumstances. The essential parts of the apparatus may be described as consisting of an alarm bell and a telegraph needle—the former being rung and the latter deflected by an electric current, which was set in motion by the action of the dangerous gas. The means by which this was effected consisted of an iron cup, on which was fixed a disc of white Sicilian marble, standing on a U-tube, which contained a quantity of mercury. The marble here represented the plaster which closed the end of the tube in the first experiment, and through it the dangerous gas was diffused. As it did so, the mercury was pressed up into the other extremity of the tube, completed the previously broken circuit, and an alarm was given by the ringing of the bell and the deflection of the needle.

Mr. Ansell proceeded to say that in coal-fields there are many casualties—including the bulk of the accidents arising from explosion,—caused by a sudden irruption of fire-damp (carburetted hydrogen), to such an extent as to render the atmosphere in even a mile of space explosive in a few minutes, and there are cases on record where an enormous space has been so polluted in a few seconds; but the common event is to find that a fall of roof, or the breaking in of a thin part of the sides or floor of a gallery, liberates an amount of gas which by mixture with the ordinary air of the pit renders the whole explosive. This mixture travels on slowly with the ventilation till it meets a light, and possibly an hour after its first formation destroys many lives. A source of great danger is that state of the pit which arises from the gradual bleeding of gas from coal. As one walks in a pit one hears a continual "click," somewhat like the noise of a cricket. In some pits this may arise from the settling down of the strata and cracking of the coal, but the experienced ear soon knows the difference. Should any obstruction arise to the ventilation this bleeding very gradually raises the atmosphere from zero (the point of purity) to the point of explosion, or it may be that a gradual fall of the barometric pressure admits of the oozing out of gas either from a goaf, or from the mass of coal, and this, although minute, may be to such an extent as to render explosive the whole air of the pit if the ventilation be not very good. There are parts of a pit where gas may be so accumulated in half-an-hour, others where it may be two hours, and again others a whole day in rising to a dangerous mixture.

It is no uncommon thing to find thirty per cent. of gas next the roof, at six inches below twenty per cent., and at fifteen inches no gas at all. I propose to fix the instruments* side by side, one for *sudden* and the other for *slow* accumulations, in pigeon-holes cast-in iron posts, such as are used to support the roof, and to be used in addition to the ordinary supports for no other purpose than to carry the indicators, the pigeon-holes being clear all through, so that the gas can surround or sweep over the instruments while they are thus perfectly protected from falling substances; for the gas as it occurs in the pits is very curious in its habits, and from causes too minute to enumerate here, it "goes away" from a spot with very little disturbance. The pigeon-holes being formed in iron posts would protect the instruments from falling roof, etc., while grooves may be cast in the sides of these posts for the telegraph wires. It has been objected to by some that these instruments would cause greater destruction of life than now obtains, but these persons forget that my instruments are not intended to displace other means of safeguard. They are simply proposed as additional means of knowledge.

For the indication of carbonic acid I make a necessary alteration, which will be seen in the figure:—



This hardly needs description, for it will be seen at a glance that the current is completed by the rising of the mercury to the wire within the precincts of the closed chamber formed by the neck of the funnel, and is adjusted for use by turning the base on which it stands, when a cork rises against a leather bag and presses the mercury up to the required height. Whether marble will stand for a long period in contact with carbonic acid, and without disintegration, has to be determined; if not, I can replace it by another septum. This instrument is proposed for use in those mines where carbonic acid becomes a dangerous substance for the miner. It has been sought by the French vine growers as a means of telling the time of the commencement of fermentation, and it seems probable that the English brewers will use it for a similar purpose.

In the event of fire-damp being known to exist, either when found by the fixed indicators or by the safety lamps, I propose for the use of the miner, the manager, or his deputy, an aneroid indicator, which is not intended for the *detection of gas in the pit*. The intention of this particular instrument is that it shall be used to determine the amount per cent. of fire-damp or carbonic acid where they are known or suspected to exist; and for these purposes it must be used in rigid accordance with the instructions given with it, not according to the fancy of the user, or as *he thinks it should be used*.

It must be mentioned that the same amount per cent. of fire-damp in different mines requires a varying time for diffusion through the same tile. The cause of this is not known, but it is under investigation. This variation is from 45 to 60 seconds; but in the same pit it is invariably of a uniform time, so that, once determined, there is no trouble. That this is a property of the gas, is proved by the fact that 10 per cent. of gas in one pit will explode violently, while 10 per cent. in another explodes with much less violence. The underviewers call one a sharp gas and the other a slow gas, and the appearance is perfectly well marked if observed for a few times in the Davy lamp.

A mine, to be well ventilated, should be so supplied with air that a considerable eruption should be detected below the point of explosion. Ventilation which is only sufficient for ordinary occasions must be considered entirely insufficient. The Act of Parliament at present in force specifies that the working places shall be so ventilated as that "under ordinary circumstances" they shall be "in a fit state for working passing therein." This clause of the Act should be altered, and

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* These instruments were fully described in the Chemical News, vol. xv., p. 13.—[*Eng. Ed.*]

the Act should specify that five per cent. of fire-damp should be the maximum ever permitted to attain, because seven and a half per cent. is the minimum explosion point, and five is two-thirds of seven and a half, which is surely as near as could be allowed with safety.

It has been objected that my instruments are too delicate for use in the pits—that, in fact, they demonstrate the existence of so small an amount of gas as to render their adoption a probable source of trouble by "showing the presence of 1½ per cent. of fire-damp, and thus causing alarm, while that amount is actually harmless." To this objection I would reply that I have made my instruments so delicate, because in their former state it was objected that they would not show *small quantities*—that is smaller quantities than could be detected by the safety lamps now in use. But this objection has no weight, because the instruments, though capable of being so delicately set, need not be so adjusted unless by the will of the responsible person. I prefer that they should be so adjusted as to give warning only when the quantity of gas present reaches the limits of the minimum explosion point; still I would urge that the fact of an instrument showing small quantities as well as large does not necessarily render it valueless.

When I was permitted to exhibit and explain my instruments in committee-room D of the House of Lords, Mr. J. M. Day inspected them, and in behalf of the Mining Association of Great Britain, he made to me the following proposition:—"That I should make experiments in the coal pits he would select, and if my instruments would effect what I said they would do, they would prove to be very valuable and would be adopted—if, on the other hand, they failed in such trials there would be an end of the matter."

In accordance with this suggestion I visited such pits as Mr. Day appointed, and which he purposely selected, that I might meet every variety of circumstance. My experiments were admitted to be successful. I have just (between 26th August and 1st September) completed an exhaustive series of experiments in the Monkland pits at Airdrie, and Mr. Murray gives me permission to say that he has satisfied himself that my indicator is practicable and reliable.

From facts within my own knowledge, I have reason to believe that one of the reasons why my instruments are not adopted is, that the colliers would not go down the pits or remain at work if it were demonstrated to them that there was a dangerous accumulation of fire-damp. That the men have fear of fire-damp is demonstrated by the fact that the bench of magistrates of Wednesbury did, on the 2nd November, 1866, convict and sentence to fourteen days' imprisonment three colliers for refusing to work in a coal mine because the said mine was full of fire-damp. Yet the owners took no measures to prove to the men the absence—thus admitting the presence of fire-damp, but contented themselves with the statement that the complaint of the men, although supported by witnesses, was an excuse for a holiday. I am of opinion that coal-owners and their agents will not voluntarily adopt any new proposition, they having for years blindly ignored the Davy lamp, and they still refuse in a very great measure to adopt safety-cages, safety-hooks to prevent overwinding, and the necessary number of ventilating shafts, abiding only just inside the law of the land.

I wish to remark that the state of the barometer *does not* give warning of the irruption of fire-damp till some hours *after* it has taken place; and in such cases mining engineers admit that an instrument would be of real service.

While the French have offered considerable prizes for the invention of means devised for the saving of *life* in mines, English mine owners have contented themselves with offering prizes for coal cutting machines devised only for the saving of *labour*, but I humbly submit that if I am right in my supposition that my instruments are calculated to save life, they ought to be adopted, and all I ask is that they should be fairly tried. I am permitted to refer enquiries to Mr. F. Murray, Monkland House, Airdrie, who has some of these instruments in his pits, and who will kindly exhibit them in action.

In consequence of a wish expressed by Francis Murray, Esq., Monkland House, Airdrie, I sent him some instruments that he in my absence might form an opinion as to the practical value of my indicators, and that the colliery people, workmen as well as officers, might find out any objection to the instruments. Mr. Murray, in a note to me, says:— . . . "the instrument was carried into the suspected places and put on the top of a post close to the roof of the working, in less than a minute after the connection was made the bell rang, the aneroid, (a pocket indicator) at the same spot indicating 45 per cent. of gas. . . . . I allowed the bell to ring for about a minute and then screwed up the screw until it ceased to touch the mercury, in less than a minute the alarm sounded again; I screwed up again with the same results until the screw would go no further. Mr. Murray then placed the instrument "on the floor where the aneroid indicated no fire-damp . . . re-adjusted the mercury, and repeated the above experiments with the same results."

"I then by means of the aneroid ascertained the height at which 7½ per cent. of gas was present, and then the alarm sounded in about five minutes. I then got all the gas driven out of the place, the aneroid indicating nothing at any height, and fixed the syphon at the height which formerly contained 7½ per cent. of gas. In one hour the alarm bell sounded. One turn of the screw was given, and the alarm sounded again in ten minutes. The aneroid at the moment of the second alarm indicated 8 per cent. of gas. These experiments were made with a new instrument covered with ⅝ inch marble, and an old instrument the thickness of marble I do not recollect." This was an old instrument which had been in use a year in some pits and is covered with ½ inch marble.

On Tuesday, August 27, I went into the pits with Mr. Murray and some of his people, experimenting till 11·30 p.m. without a single failure. Quite the last thing, we chased the pit of gas and set four syphon instruments with a view to see if when gas accumulated slowly in the night the instruments would give warning and maintain their action till the morning. These instruments were visited in the morning by Mr. Crum and the fireman of the pit. Mr. Crum writes to me as follows:—"The place in the pit was filled with fire-damp till past the battery and the bell. The bell rung with 1½ inch of zinc above top. All observations in the dark—no light even in main road—no Davy lamp."

Mr. Murray was present at part of these experiments, and the fireman, accompanied by a collier, was present at the whole of them reading the barometer indicator with me. Mr. Murray says, "There is no doubt from these experiments, which extended over a fortnight, that the instruments are quite reliable and quite easily managed. We worked under great disadvantages, the place was very low, I think the men said three feet four inches; but there is no difficulty in obtaining the results needed."

Professor ANDERSON believed that this was the first attempt to apply the law of diffusion to practical purposes, and it was certainly an invention of the highest importance. The only drawback to the instrument was that its nicety required it to be worked, not by ordinary colliers, but by intelligent men.

Mr. J. L. BELL, of Newcastle, feared that the very nature of the work of a colliery, which would necessitate the perpetual movement of the apparatus, might impede its use by the workmen; but thought that, in the hands of the coal viewers, it might be turned to good account, as the accidents most feared by coal owners were those in which the atmosphere of a whole gallery, or a portion of one, was polluted. The instrument would be likely to be of immense value in the prevention of that wholesale destruction of life we had so often to lament. He was afraid, however, that familiarity would breed contempt, and after the novelty of the invention had passed away, the men would be apt to neglect it.

Mr. ANSELL, in reply, stated that Mr. Murray's ordinary working colliers had used the instrument with perfect success. Mr. Murray had suggested that the whole apparatus should be enclosed. When this was done the workmen could in no way misuse it.

SECTION B. CHEMICAL SCIENCE.

"*Notes of Analyses of Gold Coins of Columbia, New Grenada, Chili, and Bolivia; with some Account of the Operations of Gold Mining in Nova Scotia, Dominion of Canada.*"

By George Lawson, PhD., LL.D.,

Professor of Chemistry, Dalhousie College, Halifax, N.S.

The first part of this paper was principally devoted to the history and description of the gold coinage of the above-mentioned countries, with physical and chemical analyses. Some information was then given respecting the composition of the native gold of coining countries, and a useful list was appended, showing the principal gold coins of various countries, with their weights, fineness, and values; and a synopsis of the results of assays and analyses of native gold from the chief mines of the world.

The author then proceeds in the following words:—"Writing from a gold country, it may not be out of place if I refer briefly to the results of gold mining in this province. The geological relations of our auriferous quartz veins, so far as ascertained, have been detailed by several able geologists. I shall, therefore, merely offer a few statistics, serving to show the extent and present condition of the mines, which, although they have not proved so profitable as was anticipated by speculators, are still vigorously worked in most of the districts."

The general results of the gold mining operations in Nova Scotia up to the end of last year (1866) are shown in a table, the facts in which may be thus summarised:—"Total amount of quartz, etc., crushed, 101,946 tons; gold obtained, 91,958 ounces, nearly three tons weight, worth £380.861; giving an average of 21½ grains of gold for every 100 lbs weight of quartz crushed, and an average annual yield of gold of 20 oz. for every man employed, or a daily return of 6s. 10d. per man for every working day. This return has to meet, not only the wages of men, but likewise interest on capital, royalty, tools, machinery, and expenses of management.

"The gold of all the Nova Scotia mines compares very favourably with that obtained in other countries; in other words, our gold is remarkably pure. Whilst Californian gold, as shown by thousands of assays, contains on an average 11½ per cent. of silver, and the gold of soveral of the South American States a much larger proportion, our Nova Scotia gold contains (on an average of analyses) less than 4 per cent., and some samples less than 2 per cent. of silver. The gold would be still purer were it not that the various metallic minerals that occur in the quartz veins and adhering slate and quartzite are crushed up with the gold in the process of extraction; and, no doubt, likewise whilst iron and copper are abstracted from the stamping boxes and machinery during amalgamation, lead, silver, etc., are probably introduced from impurity of the mercury employed. The principal minerals of the auriferous quartz veins here are zinc blende, galena, iron and copper pyrites, and mispickel (arsenical pyrites); which last abounds in some mines, and causes inconvenience to the workmen, if the ore be roasted before crushing."

In all our mines the gold is separated by the method of amalgamation. The quartz is pulverised in a strong iron stamping box, which rests on a heavy granite block to resist the continual action of the heavy piston hammers. The stamping box contains mercury, and has a continuous supply of water. The powdered quartz is continually floating out, as a silt or "slime," through a wire gauze arrangement, and is carried in a small hollow sluice over little sunken cups or pools filled with mercury, then through a series of sluices or "shaking tables" lined with copper, the surface of which has been rubbed or amalgamated with mercury; these shaking tables are arranged one under the other like steps of a staircase, and have a continued oscillating motion, whilst the stream of water (hot or cold) runs from one to the other, carrying the slimes. Thus the process goes on continuously,—a "crusher," as it is called, being in reality a mill in which the various processes of pulverising, washing, and amalgamating go on during the week without interruption. A crusher usually consists of a number of stamping boxes, and relative sluices, and other arrangements, such as described, arranged parallel to each other, so that two or three men may conveniently feed the whole of the stamping boxes. The mercury takes up the gold from the sluices as they pass over its surface, and the amalgamated surfaces of copper slowly become incrusted with a dull pasty coating. This pasty substance of a leaden colour is the gold amalgam, a chemical compound of gold and mercury. Once a week the mill is stopped, or it may be once a fortnight, or once a month, and the pasty incrustation is carefully scraped away from the shaking tables and sluices, the mercury is taken out of the cups or grooves, and the whole put into an iron retort; heat is applied, the mercury is distilled off to be used again for the absorption of fresh supplies of the precious metal, and the gold is found at the bottom of the retort in the form of a more or less porous and impure mass. It is re-melted to get rid of impurities, and is then ready to be sent in the form of a brick or bar to the bank or mint.

The process lately invented by Mr. Crookes, by which sodium amalgam is added to the mercury, has not yet been taken advantage of to any extent in our mines. I have experimented to a considerable extent on the effects of sodium amalgam, and find it to exert a very remarkable power in facilitating the absorption of gold by mercury, quite independently of any action of the soda necessarily formed during the operation. I believe, therefore, that much benefit would result from the use of Mr. Crookes's amalgam. The coating of the copper surfaces with mercury alone has been found practically to be a troublesome and tedious operation; but the use of a little sodium amalgam added to the mercury enables the coating to be given by a simple rubbing without any waste of time. Some illustrations of the advantages of Mr. Crookes's process were given in a paper which I published last year in the *Transactions* of our Institute of Natural Science.* It was stated that in some experiments undertaken in conjunction with Dr. Krackowizer, formerly of Vienna, at the Lake Major Mines, a quantity of pyrites collected from the tailings (after passing through the mill in the usual way) was re-subjected to the action of mercury, to which sodium had now been added, and by this means the waste material from which all the gold was supposed to have been abstracted, yielded a fresh supply at the rate of five ounces of gold per ton of pyrites. In the washing of alluvium, during which there is a great loss of mercury from "flouring," the advantages of Mr. Crookes's process were equally obvious.

Professor Anderson, in proposing thanks to the author of this paper, asked Mr. Crookes, who was in the room, if he could give any further details respecting the sodium process of amalgamation of which he was the inventor.

Mr. Crookes said that there was one thing which ought especially to be attended to in employing this process, and that was to avoid introducing too much sodium. Every failure which had come under his notice had arisen from ignorance of the action which the sodium was intended to exert on the mercury. If too much were added it exerted a chemical action, reduced the iron, copper, lead, etc., which might be present in the ore, and loaded the mercury with base metals, destroying its power of wetting gold, and causing it rapidly to flour away when triturated in a stream of water. If, however, only a trace of sodium were introduced (say 1 in 10.000, or 1 in 100,000), it acted physically rather than chemically; it put the mercury into a highly electro-positive state, and by greatly widening the electric interval between this metal and gold, increased their mutual affinity.

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* "On some recent Improvements in the Amalgamation Process for Extracting Gold from Quartz." By George Lawson, LL.D. *Trans. Inst. Nat. Science of Nova Scotia*, part iv., pp. 71-76.

Professor **Williamson** said that some specimens of auriferous pyrites had been brought under his notice, containing as much as 25 ounces of gold to the ton, and which yielded nothing whatever by the ordinary mercurial treatment. He was anxious to know from Mr. Crookes whether his sodium process would extract the gold, and would also enquire whether it was known in what state of combination the gold existed.

Mr. **Crookes** replied that the general opinion was that the gold existed in auriferous pyrites in the metallic state, and not as a single or double sulphide. The gold was sometimes visible in plates between the crystals of pyrites, and was frequently left behind in the form of brilliant spangles and crystals on dissolving the pyrites in acid.

Although mercury in its ordinary state would not extract the gold from pyrites, the addition of a trace of sodium conferred this property upon it, and in skilful hands it was easy to obtain almost the assay quantity of gold without appreciable loss of mercury by flouring.

Monday, September 9th.

The meeting was very well attended this morning. The first paper was one entitled,

"*Remarks on the Calculus of Chemical Operations.*" By Dr. A. Crum Brown.

This was followed by an exceedingly animated discussion, in which Professor Clerk Maxwell, F.R.S., Rev. Professor Harley, F.R.S., Mr. A. R. Catton, Professor Wanklyn, Dr. Odling, F.R.S., and Dr. Williamson, F.R.S., took part. The line of argument was somewhat similar to that adopted on the occasion of the discussion of Sir B. Brodie's paper at the Chemical Society, and the general feeling was adverse to the introduction of so radical a change as that advocated by Sir B. Brodie, although most of the speakers appeared disposed to reserve their opinion till the publication of the second part of the "Calculus of Chemical Operations." Time will not admit of our preparing an abstract of the paper and discussion in time for publication this week.

This was followed by a paper—

"*On certain New Processes in Photography.*" By J. Spiller, F.C.S.

I have the pleasure of submitting to the notice of the Section several interesting results and improvements in photography, based, it may be said, on the chemistry of gelatin. The processes to which I refer are the various modifications of the Woodbury type, including the new method of micro-photo-sculpture, the art of photo-lithography, as practised in the Royal Arsenal at Woolwich; and some illustrations of the use of gelatin or albumen, on a foundation of silk, satin, or cambric, the work of H. P. Pritchard, of the War Department.

The Hon. H. Fox Talbot was one of the first to describe and make a practical use of the action of light upon a mixture of gelatin and a soluble bichromate, and after him Colonel Sir Henry James, Mr. Swan, of Newcastle, and Mr. Woodbury, of Manchester, have applied the same chemical principle in new directions. It is known that the chemical rays of light have the effect of rendering insoluble gelatin to which a bichromate has been added. It would appear that this oxidising salt hardens the animal substance, by forming with it a combination of chromic oxide. In proof of this view, it may be stated that Mr. Swan has lately devised a mode of working, in which a minute quantity of chrome alum or sulphate of chromium is used instead of the red chromate, and it is found that when dried this mixture is not again affected by water. The carbon prints of Mr. Swan, which were exhibited and so much admired last year at Nottingham, are illustrations of the use of a chromate in conjunction with gelatin and pigments. Mr. Woodbury's process is also based on the insolubility of the chromo-gelatin after exposure to light, and upon the subsequent action of water upon a sensitive film, which has been in different degrees influenced by insolation under an ordinary photographic negative. The depths of tint in the original are represented by variations in the thickness of the film of gelatin left unacted upon by water, and this dried may then be used as a matrix to produce a corresponding series of depressions upon a surface of lead or type metal by the aid of a powerful hydraulic press. The blocks so produced serve for printing off a great number of proofs when they are liberally "inked" with warm gelatin, highly charged with Frankfort black or other suitable pigment, and pressed down upon a smooth sheet of paper until the excess of ink is forced out on all four sides of the block, and so removed from the space constituting the picture, which, when set, is, lastly, protected with a varnish of collodion. (Specimens of the Woodbury type were exhibited.) A glass plate may be used instead of paper to receive the ink, and this, backed with another (opal) glass, gives an excellent result, suitable for a variety of ornamental purposes. (Specimen shown.)

Mr. Woodbury has lately perfected a modification of his process, which is applicable to the representation in high relief of microscopic objects. The method consists in spreading a warm solution of gelatin, containing a little sugar and chromate of potash, over a glass plate previously coated with collodion. The film sets on cooling, and is then placed in contact with an ordinary photographic negative of the microscopic objects to be delineated, exposed to light, submitted as before to the action of water, and the soluble portions washed away. When the surface moisture has evaporated, a mixture of plaster of Paris, containing a small proportion of alum, is poured over the relief to the thickness of half-an-inch, and left to set. When dry it will be found, owing to the alum in the plaster hardening the surface of the gelatin, directly on coming in contact therewith, to leave the gelatin easily, without any fear of adhesion. To give a finished appearance to the resulting casts, this intaglio, when dry, may be placed in a lathe, and a suitable border turned on it, which will be represented in the resulting proofs by a raised border, similar to what is seen on medallions or plaster casts. The name of the object may also be neatly engraved on the intaglio, to appear in raised characters on the reliefs. This intaglio should then be well waxed to fill up the pores, and is ready for taking any number of impressions in plaster; or a better plan is to take one in plaster, and having smoothed away any defects to mould a reverse in sulphur, which will give a greater number of fine impressions. (Specimens exhibited.)

Great progress has been made during the last year in perfecting the details of photo-lithography, and the results which I now exhibit are illustrations of the practical use of this art as a means of procuring on a reduced scale printed reproductions of the large series of lithographs used for the use of the British army by the Royal Carriage Department. Negatives of the required size are taken in the first instance by the collodion process, this service being performed in the photographic establishment of the War Department at Woolwich, under my supervision. The pictures are then copied upon a sensitive surface, prepared by floating a sheet of bank post paper upon warm chromo-gelatin solution made as follows:—I. Gelatin, 3 oz.; hot water, 40 oz. II. Bichromate of potash, 2 oz.; hot water, 10 oz. The two solutions are mixed together, and should then be kept from the light. The prepared side of the paper is taken dry, laid against the negative, and for a short time is exposed to light. It is then greased all over by spreading a thin layer of "litho-transfer ink" upon stone, and passing through a lithographic press, and the whole surface is in the next place submitted to the action of warm water thickened with gum. The ink resting upon the unexposed portions of the print is thus removed, the gelatin in these parts remaining perfectly soluble, and the paper is washed with dilute gum water, using a sponge to assist in detaching the loosened layer of ink, and finally washed with warm water alone. This sheet of paper is an accurate transcript in lithographic ink of the original photograph. All the lines should be clear and sharp, and there will be no difficulty in transferring to stone and printing off any required number of impressions by following the details of the ordinary lithographic process. The cost of production is very trifling, and a large number of prints, both plain and coloured, have been executed in this manner by Mr. Henry Butter, of the Royal Carriage Department. I have, lastly, to show a few specimens of photographs printed on silk, satin, and cambric, the work of Mr. H. B. Pritchard, of the W. D. He produces them by salting the fabrics with the following solution:—Common salt, 5 grammes; water, 500 cubic centimetres; albumen, the whites of four eggs. The air-dried fabric is then sensitised, printed, and fixed, in the ordinary manner, but with as little delay as possible. This method furnishes a means of reproducing photographs upon a stronger and more flexible basis than paper, and is particularly applicable for diagram purposes and architectural plans; we have used it in the Royal Arsenal for preparing sketches, and illustrated descriptions for military instruction and use in the field.

A discussion followed.

The next paper was one

"*On a new Polarising Photometer.*" By W. Crookes, F.R.S., which will appear in our next.

MEETING OF THE GENERAL COMMITTEE.

A meeting of the General Committee was held on Monday afternoon for the purpose of determining the place of meeting of the British Association next year. His Grace the Duke of Buccleuch presided, supported by Sir Roderick Murchison, Dr. William Fairbairn, Professor Phillips, Sir John Lubbock, Prof. Tyndall, S. M. E. Grant Duff, Esq., Sir John Bowring, and the Earl of Enniskillen.

Deputations were present from Plymouth, Exeter, Norwich, Liverpool, and Edinburgh, inviting the British Association to meet in their respective towns next year. After a discussion the claims of Norwich carried the day, and it was decided that the British Association should meet in that city, in August 1868, under the presidency of Dr. Joseph Dalton Hooker, M.D., D.C.L., F.R.S., etc.

The following gentlemen were appointed Vice-Presidents for the next year's meeting of the Association:—The Earl of Leicester, Lord-Lieutenant of Norfolk; Sir John Peter Boileau, Bart.; Sir John Lubbock, Bart., F.R.S., the Rev. Adam Sedgwick, M.A., F.R.S., etc., Woodwardian Professor of Geology in the University of Cambridge; J. Couch Adams, Esq., M.A., D.C.L., F.R.S., Lowerdean, and Professor of Astronomy and Geometry in the University of Cambridge; Thomas Brightwell, Esq.

Dr. Dalrymple, Rev. Canon Hind Howell, and Rev. J. Crompton were appointed Local Secretaries; and S. Gurney Buxton, Esq., and Roger Kerrison, Esq., were appointed Local Treasurers for the meeting at Norwich.

In the evening Professor Alexander Herschel, F.R.A.S., of Glasgow, delivered a lecture on Meteors and Meteorites, in the Kinnaird Hall, which was crowded by a very brilliant audience.

A report of this lecture is unavoidably postponed for want of space.

On Tuesday the Chemical Section met as usual, when the following papers were read:—

I. Lothian Bell.—*On the present State of the Manufacture of Iron in Britain, and its position as Compared with that of some other countries.*

J. B. Lawes and J. H. Gilbert.—*Preliminary notice of Results on the Composition of Wheat, grown for twenty years in succession on the same land.*

R. F. Smith.—*On the Gaseous Products of the Destructive Distillation of Hydrocarbons, obtained from Shales and Coals at low and high temperatures.*

P. Spence.—*On the Economisation of the Sulphurous Acid in Copper Smelting.*

W. L. Scott.—*On the Bisulphide of Calcium as a preservative of animal substances.*

W. L. Scott.—*Note on the Artificial Production of Oil of Cinnamon.*

T. T. P. Bruce Warren.—*On the Electrical Resistances of the Fixed and Volatile Oils.*

Full reports of these papers are in hand, and will appear next week.

CONCLUDING MEETINGS.

A meeting of the General Committee was held on Wednesday, Sept. 11. The Secretary read the grants of money for scientific purposes, from which we extract the following:—Dr. Anderson, Synthesis of Organic Acids, £60; Mr. E. R. Lankester, Investigation of Animal Substances with the Spectroscope, £15; Dr. Bennett, Action of Mercury on the Secretion of Bile, £25; Dr. Richardson, Physiological Action of the Methyl Series, £25. The Secretary then read the recommendations not involving money grants, which included resolutions relating to the continuation of storm signals; and the introduction of the metric system to government schools. These were referred to the Council of the Association. It was also recommended, in Section A.—That the Metrical Committee be re-appointed; that Professor Stokes be requested to continue his researches in physical optics; that Mr. Low, Mr. Glaisher, Dr. Moffat, Mr. C. Brooke, Dr. Anderson, and Dr. Ward Richardson be a Committee for the purpose of promoting accurate meteorological observations on ozone; that Dr. Tyndall, Dr. Lyon Playfair, Dr. Odling, Rev. C. Pritchard, Professor Kelland, Professor W. A. Miller, and Professor Foster, be a Committee for the purpose of enquiring into the present system of teaching the elements of dynamics, experimental physics, and chemistry in schools of various classes, and of suggesting the best means of promoting this object, in accordance with the recommendations of the report of the Committee appointed by the Council of the British Association, and that Professor Foster and Dr. Odling be the Secretaries. Section B.—That Dr. Matthiessen be requested to continue his researches on the chemical constitution of cast iron; that Mr. Fairley be requested to continue his researches on the polycyanides of the organic radicals; that the Committee on Scientific Evidence in Courts of Law, consisting of the Rev. William Vernon Harcourt, Professor A. W. Williamson, the Right Hon. J. Napier, Mr. W. Tite, Professor Christison, Mr. Carpmael, Dr. Tyndall, Mr. J. Heywood, Mr. J. F. Bateman, Mr. G. Webster, Sir B. Brodie, and Professor W. Allen Miller (with power to add to their number) be re-appointed, and that Professor Williamson be the Secretary. The Secretary then read a communication:—That the President of the Association be requested to communicate the report of the Committee appointed by the Council to consider the best means for promoting scientific education in schools, to the President of the Privy Council, and to the Parliamentary Committee, on the part of the Association; and that the general officers be authorized to take steps to give publicity to the report. Dr. Richardson intimated that at the next meeting of the General Committee he would move that a Section for Applied Science should be formed. The concluding general meeting of the Association took place in the afternoon, at three o'clock. Mr. Griffiths made the following statement of the number of tickets which had been issued on the occasion of the Association meeting in Dundee:—167 old life, 25 new life members, 193 old annual, 118 new annual, 1,163 associates; 771 ladies, and 7 foreigners; total, 2,444.

Sulphurous Acid and Hydric Sulphide.—S. de Luca and T. Waldini. The mutual decomposition of these two bodies is not correctly expressed by the equation:

$$2HS + SO_2 = 2HO + 3S$$

for the formation of pentathionic acid is observed during the reaction, which, however, decomposes again, setting free sulphur. The sulphur, liberated, appears in two modifications, of which one is soluble in carbonic disulphide, the other insoluble.—(*Comptes R.* lxiv. 1200.)

FARADAY.

A TRULY great man is, alas! gone from among us, and a man, moreover, whose place cannot be filled. We have great chemists, and great physicists left, but we have not, and probably never shall again have, a Faraday.

It is not often given to the world to possess a man so nearly without a blemish, either in his morals or his intellect, —it is still less often given to the world to possess a man of whom all men speak well. In this respect Faraday stood absolutely alone. But in how many more respects did he stand alone? A man of humble birth, and of high—the highest—aspirations, he sought no social distinctions: a man born poor, and yet who never coveted riches. He was the only man, we say, who has raised himself to the first rank in science in this country, whose every attribute we may fearlessly hold up as a model to our children! Davy had as great, certainly not a greater genius, but his vast ambition, eternal pining after rank, and his hauteur, made up an *ensemble* which it is not for us to imitate. Wollaston was as great a manipulator, and possibly a greater chemist, but his secretiveness and his coldness forbid us from entertaining even the faintest interest in him as a man. Despite the recent discoveries (?) of our French brethren, we cherish a feeling of veneration akin to awe for Newton, but there is no tinge of affection in our admiration. But Faraday's intellect, while it burnt as brightly as Davy's, was as deep searching as Wollaston's, and as reverent as Newton's, had nothing in it which could repel us, chill us, or forbid our affection.

We will not suppose for a moment that our readers are not to some extent aware of the principal incidents in the career of Faraday, but we cannot, while announcing his death, omit, in a few words, a slight sketch of his history, premising that less of it is known than we could wish; but this defect will soon be remedied, for too much cannot be known about him, and unlike most great men, he will never require a Froude to rehabilitate his name.

Michael Faraday was born in 1791, at Newington, in Surrey. His father was a blacksmith, and we deeply regret that we have no authentic record of his youth until the time he was apprenticed to a book-binder. It is certain, however, that at the time of his apprenticeship he was enthusiastically fond of science, and had even made an electrical machine and other scientific apparatus. The almost incredible skill which he had with his hands (a skill which is born in a man, and which, in its perfection, cannot be taught), induces us to believe that he would find much less difficulty than most men in acquiring the power of using the *materia technica* of chemistry and physics; and the readiness with which Sir Humphry Davy received him as an assistant into his laboratory, is a pretty strong evidence that at that time he knew enough of chemistry to make himself exceedingly useful.

The turning-point in his career really begins with his construction of the electrical machine and apparatus to which we have alluded. His master happening to point them out to a Mr. Dance, a member of the Royal Institution, that gentleman took him to hear some lectures of Sir Humphry Davy's. The result may easily be guessed. Every one knows that there was an almost magical charm about Davy's lectures. His wonderful discoveries, his enthusiasm, his brilliant experiments, his great reputation—if all these advantages could so enchain his audiences, that the women would fall in love with him, and send him letters, no wonder that the intellect of the noble boy was captivated and fired by them. In the purity and simplicity of his heart he thought that the priests who cherished the sacred fire were free from the meannesses and weaknesses of ordinary men.

Unhappily, scientific men are by no means what young Faraday thought them, and although science proved to him an indulgent mistress, she too often is seen as the stern goddess who can only be propitiated by the sacrifice of life; or, like the dames in the old romances of chivalry, by the performance of labours which tax the strength and courage of her votaries to the very utmost. The whole history of science is the history of a struggle for pre-eminence among its students, who, too often, take more delight in demolishing the reputation of the one man who has raised himself above his fellows, than in assisting the ninety and nine poor students who vainly appeal to them for help. The war between professors is a war to the death, and woe be to the weaker sword; and as the crusaders crammed the true faith down the throats of the unbelievers beneath the banners of the Cross, so professors slaughter each other's reputations in the gentle name of truth. As poor Mulder said of his tormentor, "It is in the name of truth that he plunges his branding iron into the fire, and it is while shouting 'truth' that he presses it on the forehead of his victim, and rejoices in the ascending vapour!"

Faraday, knowing nothing of these things, took copious notes of Sir Humphry's lectures, and forwarded them to him with a letter, in which he stated his anxiety to leave trade, and devote himself to Science, so that he might associate himself with men, who, purified by the grandeur and sacredness of their calling, were free from the littlenesses and weaknesses of other men. Sir Humphry received him kindly, and (no one being better fitted for the task) dispelled his illusions regarding the disinterestedness and simplicity of men of science. He also made him his laboratory assistant, a position for which, perhaps, no man in the world was so well qualified. This was in 1813, and for several years he worked unremittingly for Davy, who does not seem to have regarded him in any other light than as a good assistant, out of whom as much work as possible was to be got, and of whom it was expected that he should never forget the vast difference in their relative positions. Even during their stay at Paris, Davy is said to have been annoyed at the attentions that were shown to Faraday. Still, but for Davy, Faraday's progress would probably have been much slower, and there is little doubt that the prestige of being Davy's assistant was of no small value to him in his career.

What Faraday did it is not possible for us here to recapitulate. He discovered benzol, and determined the composition of naphthalin. The first discovery has led to one of the greatest industrial sucesses which chemistry has ever achieved, and the study of the second led Laurent to some of the most important theoretical discoveries of the age.

As early as 1820 Faraday discovered the chloride of carbon, and it is to him that we are indebted for the information that the chloride of olefiant gas is formed by the union of equal volumes of its constituents. It must not be forgotten that at the time of this observation being made, chemists had not their present definite views about combination by volume.

In the year 1821 Faraday made his brilliant discovery of the rotation of a wire carrying an electric current, round a magnetic pole, and *vice versâ*. This cardinal fact excited immense attention, and in addition to inducing him to devote himself for many years to electricity with almost unparalleled success, was the means of causing numerous investigators to pursue the same track. In 1821 he published his brilliant paper on the condensation of the gases, in which he enunciated, for the first time, the important axiom (now obvious enough) that gases are, in fact, simply the vapours of volatile liquids. In 1824 he was elected a Fellow of the Royal Society, chiefly, it is said, through the influence of his unwavering friend Richard Phillips, and in spite of the unwillingness of Davy; who, however, did not take any active measures to prevent it, and whose unwillingness appears to have been of a purely passive kind.

In 1827 Faraday published the first edition of his "Chemical Manipulation." This work, which has long been out of print, is a most extraordinary proof of the versatility of the author's chemical knowledge. It shows that there was no branch of chemistry cultivated in his time with which he was not practically familiar. In it we see the key to most of Faraday's success, namely, to omit no precaution. The style in which it is written, although clear, is verbose, and far from elegant. This is the more remarkable, inasmuch as he was almost unrivalled as a lecturer, not only for clearness, but conciseness, and the power of rousing the enthusiasm of his audience.

In 1829 he was appointed Lecturer on Chemistry at the Royal Military Academy, Woolwich; and in 1833, Fullerian Professor of Chemistry in the Royal Institution. In 1839 he published the first of his three volumes of "Experimental Researches in Electricity." The second volume appeared in 1844, and the third in 1855.

In 1846 he received the Rumford medal of the Royal Society, for his discovery of the rotation of the plane of polarisation of light under the influence of magnetism; and in 1847 he announced the magnetic character of oxygen, and the relations towards magnetism of gases generally.

So long ago as 1835 he received, at the recommendation of Lord Melbourne, a pension of £300 a-year from Government.

His scientific titles were almost too numerous to recapitulate. In addition to being a member of all the Academies of Science of any note in Europe, he was a Doctor of Civil Law of Oxford, Knight of the Prussian Order of Merit, of the Italian Order of St. Maurice and Lazarus, Officer of the Legion of Honour, one of the eight Foreign Associates of the Imperial Academy of Sciences of Paris, and an Associate of the Paris Academy of Medicine.

Faraday, in addition to. and beyond all his titles, was a true gentleman. His manners were characterised by an extreme gentleness and tenderness for the feelings of others. No one could write to him for advice or assistance without receiving it, and his advice was sure to be wise and good. He was entirely free from jealousy of the scientific discoveries of others; indeed he delighted in doing justice to the merits of his scientific contemporaries.

It is pleasant to know that in 1858 the Queen gave him a residence in Hampton Court.

It would be ungrateful not to put on record a few of the personal impressions which have stored themselves in our memory, in the course of the many years during which we have had the honour and happiness of knowing Faraday. We have seen him at work, we have attended his lectures, we have asked his advice, we have consulted him in our difficulties, and in every position in which we have known him, he has more, far more, than realised the ideas we had formed of him.

We can speak of him in his capacity as a lecturer with more confidence perhaps than most persons, no matter how often they have heard him, for we have followed him word for word in reporting two of his courses of lectures, viz. those on the "Various Forces of Matter," and also on the "Chemical History of a Candle." His delivery was by no means rapid, and shorthand writers followed him with far more ease than they did most persons. His language was well chosen, and when surrounded by his apparatus, he seemed almost inspired. The most simple experiment in his hands told its tale so well, and, by the manner in which it was done, assumed such marvellous freshness, that we forget that we had performed it hundreds of times ourselves, and gazed upon it as eagerly as the veriest tyro in the theatre.

How valuable this gift of enthusiasm is in a lecturer we need not say; it is, as it were, contagious, and in his case at least the earnest lecturer always secured an attentive, nay, a rapt audience. To see him perform an experiment was in itself a most instructive study. A failure was with him almost a thing unknown. His readiness of resource was wonderful, and if, in the course of an experiment, an unforeseen phase developed itself, if instructive, it was commented on and turned to account as an illustration of those forces, in the delightful study of which he passed his life; if, on the other hand, the experiment took a turn which threatened to defeat the object in view, he was ready in an instant with a remedy.

A most characteristic act of Faraday was that which *Punch* (who can be serious enough at times) illustrated by a cartoon headed "Faraday presenting his card to Father Thames." Faraday, in the course of a trip in one of the London steamboats, made some very important observations on the state of the river, and, in order to acquire a tolerably exact idea of the extent to which it was polluted by solid matter, threw pieces of card into the stream, and noticed at what depth they became invisible, owing to the opacity of the water. The information thus gained formed the nucleus of a letter to *The Times*, which did more to call attention to the dangerously foul state of the river than any number of letters from less gifted and venerated writers.

We have said that no one ever asked the advice of Faraday in vain; and certainly, no more golden words were ever uttered than those in which he told to a young inquirer the secret of his uniform success: "the secret," said he, "is comprised in three words—"Work, Finish, Publish." It must be confessed that young chemists of the present day follow this advice, carefully omitting the second word.

Faraday was married, but, like Davy, Berzelius, and Wollaston, left no children to inherit his glorious name.

On Sunday last he died, and it will, indeed, be long before we shall look upon his like!

He is to be buried this day. The funeral will leave Hampton Court in time to be at the Royal Institution, Albemarle Street, at 2 o'clock p.m. Thence it will proceed to Highgate Cemetery. His funeral will be private, but let us hope that his country will not fail to erect a monument to his memory, worthy of his genius.

We have no Public Laboratory in this country, as they have in France, where really deserving students may carry out their researches at the public cost. What would be a more fitting monument to Faraday than such an institution, bearing his name?

NOTICES OF BOOKS.

The Elements of Natural Philosophy; or, an Introduction to the Study of the Physical Sciences. By Charles Brooke, M.A., F.R.S., Pr.M.S., etc. Based on the treatise by the late Golding Bird, M.A., M.D., F.R.S., F.L.S. 6th edition, 3rd by present author, amended and greatly enlarged. London: Churchill and Sons, 1867.

Although entitled a 6th edition this may be considered in all essential respects a new work, as not only the illustrations and facts, but even the theories adopted are those current at the present time. The author, in an introduction entitled, "On the Nature of Energy and the Correlation and Transmutations of its various Physical Forms," explains the views now held by scientific men in place of the old ideas of imponderable fluids, the conversions of energy from form to form, and the probable mode of propagation of the so-called wave motions through material substances. It is rather unfortunate that in an introduction such as this to a purely scientific work, and immediately after endorsing the opinions of our leading scientific men as to the connection and relations of force and matter, the author should expressly condemn those who would advance science a step further, and by hypotheses such as Darwin's, endeavour to connect the varied forms of life existing around us. If this be presumptuous and beyond the domain of science, who shall say which of the questions treated of in this work are not? Those who persecuted Galileo evidently held the same opinions concerning astronomy, yet they were defeated eventually, as all must be who attempt to impose limits to human knowledge, and would say to it, "Thus far shalt thou come and no further."

With the exception of this passage, which should never have been introduced in a scientific work, and might advantageously be left out in the future editions that are sure to be needed, the work appears a most excellent one, well adapted either to be a manual for the student or a work of reference for the scientific inquirer; a copious index at the end of the volume renders it specially suitable for the latter purpose.

A work of this kind, treating of subjects that are altering from day to day, is naturally valuable in proportion as it includes recent inventions and improvements,—with the older parts of the subject, there are already many means of becoming familiar; the author seems to have taken this view, and acted accordingly; indeed it would be hard to say what invention or discovery of any general interest is omitted. An illustration and a full description of Ansell's fire-damp indicator is given as exemplifying the diffusive power of gases, while the chapter on the principles of mechanism is aptly concluded by a full account of Babbage's difference engine, and that on magnetism includes the various methods of correcting the compass errors in iron ships, and the principles upon which they are applied; none of the methods now employed, however, seem to be quite satisfactory,—at all events, for the first year or so after the ship is built, and before the iron has had time to assume its normal condition. In electricity, especially, we find much new and very valuable matter introduced; *ex. gr.*, Sir William Thompson's beautiful electrometers and galvanometers recently employed in the laying and working of the Atlantic cables; Wheatstone's electric balance and ingenious automatic printing telegraph; Siemens's polarised relay, now so generally used in connection with the Morse instrument; Wilde's magneto-electric machine, and Wheatstone's, Siemens's, and Ladd's modifications; the galvanic cautery, etc.; while the theoretical part comprises Ohm's laws, the method used for determining the standard of electrical resistance known as the B A unit, the principles of testing cables, localising faults, etc., and the latest discoveries of Becquerel and Marcus in thermo-electrity.

Where there is so much in the book that is excellent it seems invidious to find fault, but it is difficult to conceive how, whatever mode of classification be adopted, oxygen and hydrogen can be classed together as electro-negative elements, while platinum and potassium both come under the title of electro-positive elements. Whether a body be positive or negative, of course depends upon the substance it is compared with, and the fluid in which it is immersed; but in no fluid ever tried will oxygen and hydrogen, platinum and potassium, appear other than at opposite extremities of the scale. The extent to which a body is positive is usually considered to depend on its affinity for oxygen, water being the exciting fluid; but here the author, if we understand his table rightly, would actually make hydrogen negative to platinum! It is true the author states in explanation that many of the elements are arranged according to their chemical analogies, still the chemical dissimilarity between oxygen and hydrogen is certainly as great as the electrical.

The latter portion of the book is devoted to the consideration of light and heat, both of which forces have lately received much attention from our leading scientific men. Spectrum analysis, and the wonderful discoveries, terrestrial and celestial, effected by its means, forms a very interesting portion of the chapter on light, and is illustrated by diagrams of many of the more curious spectra of substances and celestial bodies; the difficult subject of polarized light is also very fully dwelt upon, and the generally received theories to account for its phenomena are clearly explained.

The chapter on heat contains a great amount of new matter, comprising the recent researches of Professor Tyndall on the powers of absorbing radiant heat possessed by various bodies; in treating of regelation, however, the author seems rather to have misunderstood the explanation of the phenomenon recently arrived at by the experiments of Professor Tyndall when he speaks of it as a "plastic property of ice," and says, "This action is probably analogous to the welding of two pieces of iron, depending on a plastic or viscous condition of the immediate surface, intermediate between the solid and the fluid states." This explanation is contrary to the meaning of the term "regelation," and, though at one time it was commonly received, later researches have satisfactorily proved it to be erroneous. With the exception of one or two passages, like those we have mentioned, which are not of very great importance, and may readily be corrected in a future edition, the work appears carefully written, and the views enunciated such as are now held by our leading physicists; as a book of reference, therefore, it will be found extremely valuable, and may, we think, be very safely depended upon.

Tables of the Spectra of Metals from the Original Drawings. By C. Kirchhoff and R. Bunsen. London: W. Ladd, Beak Street, W.

We have received Table 2 of this set of spectra diagrams. The characteristics of the various elements are shown very clearly, the field being about two feet in length. The spectra of indium, carbon, boron, manganese, lead, copper, cobalt, nickel, and iron are exhibited in this diagram, as obtained from their chlorides. The colouring is good, and in most cases the bright lines represent the actual spectra better than could be expected by those who have experienced the difficulty of imitating the spectrum by aid of a paint-box.

CORRESPONDENCE.

Technical Education.

To the Editor of the Chemical News.

Sir,—Having just returned from a fortnight's study of the Paris Exhibition, I read the paper on "Technical Education" in your last number with very great interest, and it induces me to offer a few suggestions, more especially in connection with the scientific training of the artizan class.

The advantages of such a training to those who are engaged in the mechanical and manufacturing industry of the country are so generally admitted, that it would be unwise, not to say positively injurious, to make any distinction in this respect between the employer and the workman. The latter, from the very nature of things, is often more likely to suggest improvements in processes which the former would overlook. Hence the value of a scientific training for the one is, at any rate, quite as important as it is for the other.

Assuming that our alleged inferiority in the present Exhibition is owing to the want of technical education, one is naturally led to inquire into the operations of the Science and Art Department—a branch of the Government recently created for the special purpose of promoting the scientific education of the people.

As far as my own experience goes, I am of opinion that the action of the department is far too limited to render any great service to the country. It serves to ascertain, by means of training and examination, that there is a very considerable amount of latent talent in the country, but takes no further steps to turn this talent to account. So far as the adult artizan is concerned, I know not that any more can be done than to put him in the way of applying the scientific knowledge he has gained to the more intelligent performance of his work. But with regard to the sons of artizans, who pass through this preliminary course with credit, and who give evidence of superior talents, is it right or even expedient, to allow these talents to fall into decay for want of a higher culture?

Now, if we had possessed in England such schools as the *Ecole Centrale* in Paris, or the *Ecoles des Arts et Métiers* of Châlons, and other places, where youths of all classes are admitted to compete for admission, and, in cases where their parents or relatives are too poor, are provided for, either partly or wholly at the country's expense; I will venture to say that, far from occupying a position of inferiority to other nations in manufacturing industry, we should, with such advantages, have maintained a decided supremacy.—I am, Sir, etc.,

A Teacher of Science.

Co-operative Chemical Club.

To the Editor of the Chemical News.

Sir,—I was very much pleased with Mr. Durham's letter in your last number (*American Reprint* Chemical News, *Oct.*, 1867, *p.* 198), as it embodies an idea that had many times flitted before my mind without taking any tangible form. There are many, no doubt, who would be glad to avail themselves of the privileges of a Chemical club, with a laboratory and library attached, and if such were started in this city by a few influential gentlemen I believe it would be self-supporting and be the means of reviving a taste for scientific pursuits that I fear is now much upon the decline.

In the metropolis no doubt there are some establishments of the kind, but in smaller cities no such advantages are to be obtained, and those who wish to pursue experimental chemistry must fit up a room in their dwelling-houses, which as they are constructed in these modern times, are quite unsuited for the purpose, and a source of inconvenience. I trust, Sir, you will give this question the assistance which your valuable paper affords, and that much good fruit may come of it.—I am, etc., Inquirer.

The Prevention of Bribery.

To the Editor of the Chemical News.

Sir,—With your permission I will now endeavour to show how the bribery and corruption described in my former letter on "the tricks of Trade" (*American Reprint* Chemical News, *Aug.* 1867, *p.* 95) may be, to a great extent, prevented. Firstly, all travellers who solicit orders should be told—The only condition on which we can do business with you is that you give no gratuities to our men. They have not, in our establishment, the power of selecting wares. A corresponding intimation should be given to every foreman on his engagement. I should even suggest the formation of a protection society among master dyers, printers, etc., in which the names of all detected offenders—whether givers or receivers of bribes—should be confidentially circulated.

Secondly, all mordants, colours, etc., on arrival, should be delivered, not into the dye-house, but into a ware-room, to be issued out to the dyers from day to day, as may be requisite. The books of the establishment will then show, with tolerable accuracy, how much of any particular ware is needed for dyeing such and such goods; and any intentional waste, such as pouring the contents of a bottle down the sink, will be at once detected.

Thirdly, all wares should be carefully weighed upon arrival—a step frequently omitted, lest the warehousemen should be bribed to pass deficient weights; this process should from time to time be watched by the master, manager, or head-clerk. All package should likewise be tared as soon as empty. Bottles of liquids should be tried with the hydrometer, to see whether they have all the same, or nearly the same specific gravity. Any package or bottle which appears to have a private mark or sign upon the label should be at once impounded for further examination.

Fourthly, the dyer using any lot of ware should be called upon to give his opinion in writing as to its quality. These papers should then be carefully preserved. If the dyers can be kept in the dark as to the precise number of bottles, etc., arriving from any place on a given day, so much the better. But the main method for frustrating bribery is exemplified in the following incident:—A foreman dyer had long been complaining of the extract of quercitran-bark supplied. To put him to the test the maker was requested to obliterate all marks of ownership on a cask, to fill it with the very same extract, and send it by a strange cart. This was done, and the extract was pronounced excellent, and nearly double in strength to the previous lot! If a dyer is suspected of praising a bad article or condemning a good one, out of corrupt motives, ply him with samples merely numbered, or marked in cypher, and require his opinion as to their comparative value. If he has not been acting honestly, he cannot avoid committing himself.

These recommendations will doubtless involve a little trouble at the outset, but if perseveringly acted upon I feel confident that they will abate, if not destroy the evil in question. I know that honest drysalters and manufacturing chemists will be very happy to co-operate in the proposed measures.—I am, Sir, yours, etc., W.

Gas from Iron.

To the Editor of the Chemical News.

Sir,—In the valuable article of Dr. E. G. Tosh, "On the Analysis of Cast Iron" (vol. xvi. p. 94. *Amer. Reprint, Oct.*, 1867, *p.* 171), mention is made of the observation of Schnitzler that in Weyl's process for the estimation of the carbon, bubbles of gas are evolved from the metal during solution. Rather than entirely accept the explanation proposed by the author, or entertain that of Schnitzler, I am more inclined to venture to draw attention to another possible explanation. I have noticed that under similar circumstances, gas is disengaged from thin iron wire, and it struck me as being the natural gas occluded by the metal. May it not be so with the cast iron? In the *Journal Chem. Soc.* (vol. v. p. 287), Graham states wrought iron probably carries about 6 or 8 times its volume of occluded carbonic oxide, and as much as 12·55 volumes of natural gas have been extracted; now according to this 4 grains cast iron, say of 7·5 sp. gr., might contain from 3·18 c.c. to 4·24 c.c. or even as much as 6·36 c.c. of a mixture of hydrogen and carbonic oxide, the former varying in the proportions of 21 to 35 per cent., that is, supposing cast iron to occlude gases in the same way as wrought, which I believe has not yet been shown, and from its crystalline nature might not be anticipated. Nevertheless we find that white variety of cast iron is of a pasty consistence when fused, and the grey iron is always of a porous nature, conditions which lead one to expect them to have the property of occlusions; further, Deville has extracted carbonic oxide from a cast steel tube. The gas which escapes during electrolytic solution has a peculiar odour, so also has that naturally occluded by malleable iron.—I am, etc.

Walter Noel Hartley, F.C.S.

September 3, 1867.

Baking Powders.

To the Editor of the Chemical News.

Sir,—In a recent number of the Chemical News (*Amer. Reprint, Sept.* 1867, *p.* 133) you published some remarks on the above subject from your Paris correspondent; the paragraph was copied into various journals, and as the remarks referred to are calculated to mislead the public, perhaps you will allow me to say a few words on the subject.

The question as regards "baking powders" does not relate to their composition so much as to their excessive use as a substitute for more proper ingredients; carbonate of soda, tartaric acid, and a small proportion of rice-flour, which form the compound, are so inexpensive that there is no temptation to the manufacturer to employ inferior or cheaper articles, if even they could be found. There exists, therefore, no necessity for those prominent warnings, which are continually published, against "imitations" of certain baking powders; but the public need guarding against the belief that these powders can adequately supply the place of butter and eggs in pastry and puddings.

Baking powders may be perfectly genuine and proper as far as they go, but they must be pernicious to health if used habitually as a substitute for the nutritious elements which ought to have a place in articles of daily food.

The sum total of the matter is this, that baking powders may be perfectly genuine and harmless in themselves, but they become injurious to health if employed to adulterate food into which they are introduced.

I do not deny that a small proportion of baking powders may be used with advantage in pastry *in addition* to the usual ingredients, but not as a substitute for any of them; and this is all that can be said in favour of the compound.—I am, etc. SANITAS.

September, 1867.

The Alkali Trade.

To the Editor of the CHEMICAL NEWS.

SIR,—By the aid of your valuable paper, I would be glad to ask your readers if any of them could advise me in a few law and other points connected with the alkali trade. I am the manager of works which are situated in a village, and though I know no preventable gas escapes, and the inspector gives us credit for a very complete condensation of muriatic gas at the condensers, still I am constantly annoyed by receiving complaints from persons living, or having works in the vicinity of ours. We have, like all other alkali and bleaching powder works, to cause a trifling unpleasantness for a short time, when running off our stills, but unlike many others we are situated in a populated neighbourhood, and still more unfortunately the still-house is placed next our neighbour's wall. I keep the nuisance at a minimum, but still I hear reports of action which make me desire to know (if any of your readers can kindly inform me), whether there is any law about a case of this description, where the gas causing the nuisance is not muriatic acid, and therefore does not come under the supervision of the government inspector. I would also like to know whether the inspector have any control over the muriatic gas that escapes from the furnaces, because it is well known that a close or "blind" furnace does not throw off its gas so well as an open one, consequently with a "blind" furnace there is a great deal more comes off a batch of "sulphides" when being drawn.—I am, etc., "NUISANCE."

Specific Gravity Problem.

To the Editor of the CHEMICAL NEWS.

SIR,—The following solution of the Specific Gravity Problem, proposed by your correspondent Henri du Chemin-creux, may satisfy him, although I do not bring it forward with the idea that there may not be a shorter way of solving it.

The question may be summarised thus:—

To 1,000 grains of liquid of Sp. Gr. 1·314
x " " " 1·000 are added,
giving $1{,}000+x$ " " " 1·286
Find x.

Now, the relation which subsists between these two groups of quantities is, that the sum of the products of the number of grains of the two liquids taken separately into their respective specific gravities is equal to the product of the number of grains of the two liquids taken together into the specific gravity of the mixture thus obtained. We have then—

$$1{,}000\times 1\cdot314+x\times 1\cdot000=(1{,}000+x)1\cdot286.$$

Simplifying this equation, we obtain—

$$0\cdot286x=28$$

From which $x=\dfrac{28}{0\cdot286}=97\cdot9$ grs., the quantity required.

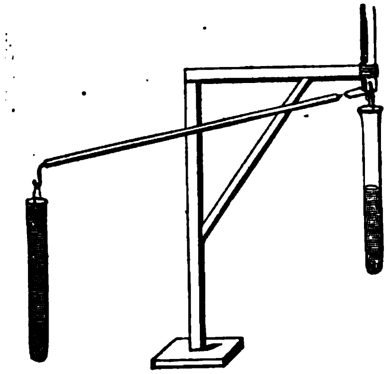
In conclusion, I may state that I tried this process with a solution of common salt, and arrived at a satisfactory result. F. J. R. C.

Hydrostatic Paradox.

To the Editor of the CHEMICAL NEWS.

SIR,—The following simple experiment will illustrate the important principle of hydrostatic pressure; and I therefore take the liberty of sending it for the amusement of your readers.

Let two test tubes of equal dimensions, one nearly full and the other about half full of water, be suspended at opposite ends of a beam, turning freely on a pivot, and let the second tube be so hung as to move in the same vertical straight line, during the vibration of the beam. Directly over this tube let a glass rod of smaller diameter be made to slide vertically through a fixed wire spring capable of holding it steady in any position. On lowering the rod carefully into the second tube so as not to touch its inner surface till the water therein is raised by displacement to the same level as that in the first tube, the two tubes will balance each other, though the original weight of water in each is different.



For if a be the area of the aperture of the tubes, h and h' the heights of the two columns of fluid, w the weight of a unit of water, and P, P′ the pressures on the base, we shall have $P = wah$, and $P' = wah'$, the weight of water in the two tubes respectively. Therefore, when $h = h'$ we have $P = P'$.

If the glass rod had been suspended from the beam of a balance during the experiment, it would of course be found to have lost the weight of the water displaced, or just the additional weight needed to counterbalance the full tube. This additional weight, so to say, was given to the second tube by means of hydrostatic pressure.

Instead of the glass rod a small cylinder of ice attached to a thin piece of wire may be employed, of such dimensions that, when the cylinder is entirely immersed in the water of the second tube, the fluid shall stand at the same level in both, and both be in equilibrium. Now, a given weight of water occupies less space than the same weight of part ice and part water. Consequently, as the ice melts, the column of fluid will sink in the second tube, and be unable to counterbalance the column in the first. The additional weight of water does not exactly replace the hydrostatic pressure withdrawn. The second tube will, therefore, rise.

If the balance be furnished with a long index, the movement will be more easily perceived, and curiously illustrate the different specific gravities of ice and water.—I am, etc. EDWIN SMITH.

Nottingham, Sept. 2, 1867.

CHEMICAL NOTICES FROM FOREIGN SOURCES.

Ethers, Contribution to the History of.—Ch. Girard and P. Chapoteaut. If one equivalent of alcohol is mixed with one equivalent of stannic chloride, and rise of temperature has been guarded against, crystalline compounds are formed which are volatile almost without decomposition; they dissolve in water, and then decompose gradually, forming alcohol, chloride of alcohol-radical and stannic oxychloride. Heated with one equivalent of an alcohol they form ether and chloride of the alcohol employed, besides stannic oxide, and chloride. The formula of the ethyl-compound is $C_4H_5,HO,SnCl_2$. Alcalic hydrates decompose them into alcohol and stannic oxide, and boiling alcohol into ordinary or mixed ether. These reactions show that the part taken by stannic chloride in the formation of ether is similar to that of sulphuric acid, and this is still more apparent if the action of the stannic chloride on mixtures of acids and alcohols is considered; in this case the compound of alcohol and stannic chloride, originally formed, acts upon the acid and produces by mutual decomposition the mixed ether. The authors have in this way prepared the methylic, ethylic, and amylic ethers of formic, acetic, tartaric, lactic, butyric, benzoic, palmitic, and stearic acid. The action of stannic chloride is explained in the following equations.

1. On alcohols (ethylic alcohol as example)

$$C_4H_6O_2+SnCl_2=C_4H_5O,SnCl_2,HO$$
$$C_4H_5O,SnCl_2,HO+C_4H_6O_2=2(C_4H_5O)+(SnCl_2,2HO);$$

2. On a mixture of alcohol and acid (ethylic alcohol and acetic acid as example)

$$C_4H_6O_2+SnCl_2=C_4H_5O,SnCl_2,HO$$
$$C_4H_5O,SnCl_2HO+C_4H_3O_3,HO=$$
$$=C_4H_5O,C_4H_3O_3+(SnCl_2,2HO)$$

(*Comples R.* lxiv. 1252.)

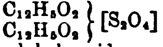
Synthesis of Methylallyle.—A. Wurtz. It has been shown by the author, some years ago, that the action of zincic ethyde upon allylic iodide gives rise to the formation of ethylallyle $=\begin{matrix}\text{C̵}_3H_5\\ \text{C̵}_2H_5\end{matrix}$ which is isomeric with amylene. Zincic ethyde and brominated propylene scarcely act upon each other, either in the cold or at an elevated temperature; nor do zincic methide and allylic iodide. But if, in the latter case, sodium be added, and the temperature raised to 120°, an energetic reaction takes place, in course of which a very volatile hydrocarbon is formed, which, combined with hydriodic acid, has the composition C̵_4H_8HI. A more ready method to obtain this body is the following: A mixture of methylic and allylic iodide, diluted with twice its volume of dry ether, is heated together with sodium to 100° for several hours in sealed vessels. After the completion of the reaction, the contents of the vessels are distilled, the distillate saturated with bromine, the excess of the latter removed by potassic hydrate, and again distilled. When all the ether has gone over, the distillation is continued in a vacuum and stopped when the temperature reaches 100°. The residue solidifies on cooling and consists of diallylic tetra-bromide, the portion distilled off is subjected to repeated fractional distillations, and finally a colourless bromide is obtained. This bromide is readily decomposed by sodium, and the hydrocarbon $\text{C̵}_4H_8=\begin{matrix}\text{C̵}_3H_5\\ \text{C̵}\ H_3\end{matrix}$ formed, which is a gas at ordinary temperatures, but may be condensed to a liquid by being cooled to $-12°$; it then boils between $-4°$ and $+8°$. The hydriodate of methylallyle boils between 116° and 118°.

Butylenic hydriodate, although of the same boiling-point and sp. gr., is not considered by the author to be identical with the former, on account of the hydrocarbon having a considerably lower boiling-point.—(*Comptes R.* lxiv. 1088.)

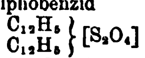
Tyrosin, derivatives of.—G. Beyer. Tyrosin was obtained by boiling one part of horn turnings with two of sulphuric acid and ten of water; the liquid was neutralised with calcic hydrate, filtered, and evaporated to half its original bulk. The calcium-compound of tyrosin was then converted into the corresponding lead-compound; this decomposed by sulphuretted hydrogen and the solution evaporated to crystallisation. It was converted into the nitrate, and then, according to Städeler's method, into nitrotyrosin. This nitro-compound is reduced to amidotyrosin, $\text{C̵}_9H_{12}N_2\Theta_3$, by the action of tin and chlorhydric acid. The amido-compound is very deliquescent, but may be obtained as a crystalline powder by concentrating its aqueous solution at 100°, and cooling under the desiccator in a vacuum. It is anhydrous, sparingly soluble in hot alcohol, and is not decomposed when heated to 100°. It dissolves readily in diluted acids, forming well-defined salts. Of these the hydrochlorate $\text{C̵}_9H_{12}N_2\Theta_3$, $2HCl+H_2\Theta$—two sulphates $\text{C̵}_9H_{12}N_2\Theta_3$, $2H_2S\Theta_4$, and $\text{C̵}_9H_{12}N_2\Theta_3$, $H_2S\Theta_4$—and a double sulphate of amidotyrosin and zinc, $Zn,S\Theta_4+2(\text{C̵}_9H_{12}N_2\Theta_3,H_2S\Theta_4)$ are described.—(*Arch. Pharm.* [2] 130 44.)

Lecture Experiment.—A. Baeyer. When a glass rod, moistened with chlorhydric acid, is plunged into a flask, containing a few drops of an alcoholic solution of propargylic ethide $\text{C̵}_3H_3.\Theta.\text{C̵}_2H_5$, thick, white clouds, like sal-ammoniac, are formed. This phenomenon evidently consists of an addition of ClH to the other; by which the compound $\text{C̵}_3H_4Cl.\Theta.\text{C̵}_2H_5$ is formed, and may serve as an illustration of the similarity which exists between non-saturated carbon-compounds and ammonia.—(*Ann. Chem. Pharm.* cxlii. 326.)

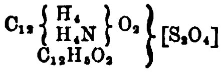
Oxysulphobenzid.—L. Glutz. It is still a matter of uncertainty whether phenol is to be considered as the hydrate of phenylic oxide $C_{12}H_5O.HO$, or as oxybenzol $C_{12}H_5O_2.H$. Supposing the latter to be the correct view, the action of sulphuric acid upon phenol will give rise to the formation of an oxysulphobenzid


$$\left.\begin{matrix}C_{12}H_5O_2\\ C_{12}H_5O_2\end{matrix}\right\}[S_2O_4]$$

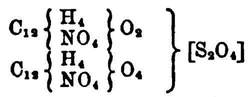
corresponding to the sulphobenzid


$$\left.\begin{matrix}C_{12}H_5\\ C_{12}H_5\end{matrix}\right\}[S_2O_4]$$

from benzol. This reaction does indeed take place when two parts of phenol and three of sulphuric acid are heated together to 160°—170°. Oxysulphobenzid is sparingly soluble in cold water, readily in hot water, alcohol, and ether. It has the properties of a weak acid; when dissolved in ammonic hydrate, and left to evaporate at ordinary temperatures, the compound

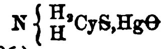

$$\left.\begin{matrix}C_{12}\left\{\begin{matrix}H_4\\ H_4N\end{matrix}\right\}O_2\\ C_{12}H_5O_2\end{matrix}\right\}[S_2O_4]$$

crystallises out. Nitric acid converts it into nitro-oxysulphobenzid


$$\left.\begin{matrix}C_{12}\left\{\begin{matrix}H_4\\ NO_4\end{matrix}\right\}O_2\\ C_{12}\left\{\begin{matrix}H_4\\ NO_4\end{matrix}\right\}O_4\end{matrix}\right\}[S_2O_4]$$

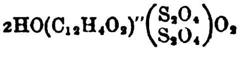
which is insoluble in cold, sparingly soluble in hot water, soluble in alcohol. Strong sulphuric acid dissolves oxysulphobenzid at the ordinary temperature without decomposing it; when heated, decomposition takes places, in course of which oxyphenylsulphuric acid is formed.—(*Zeitschr. Chem.*, N. F. iii. 435.)

Mercuric Sulphocyanides.—T. Philipp. The white precipitate which is formed by adding potassic sulphocyanide to mercuric nitrate, and which is soluble in an excess of either salt, is mercuric sulphocyanide, $HgCy_2S$. Potasso-mercuric sulphocyanide, $HgCy_2S_2+KCyS$, is formed when mercuric nitrate is added to potassic sulphocyanide until the originally white precipitate is converted into a yellowish crystalline mass. The double-salt of mercuric cyanide and potassic sulphocyanide, $HgCy_2+KCyS+2aq.$ is obtained by mixing solutions of its two constituents together. Compounds of mercuric iodide, bromide, and chloride with potassic sulphocyanide may be obtained in a similar manner. The basic mercuric sulphocyanide of Claus, which is formed by adding ammonia to potasso-mercuric sulphocyanide, has the composition,


$$N\left\{\begin{matrix}H,\\ H\end{matrix}\right.CyS,Hg\Theta$$

(*Pogg. Ann.* cxxxi. 86.)

Oxyphenylendisulphonic Acid.—C. Weinhold. This acid is obtained, besides phenol sulphuric acid, from phenol by the action of sulphuric acid. The author prepares it in the following manner: sulphuric anhydride is distilled into a well cooled flask containing crystallised phenol; the reaction which thus takes place with moderate energy is completed by exposing the contents of the flask to the temperature of the water-bath for a couple of hours. The mixture is then diluted with water, and the new acid separated from the excess of sulphuric acid by fractional precipitation with plumbic carbonate. The solution of the neutral lead-salt is readily decomposed into a soluble acid and difficultly soluble basic salt; and by adding further quantities of plumbic carbonate, the whole may be converted into the basic salt, and thus be freed from phenol sulphuric acid which remains in solution. The oxyphenylendisulphonic acid is now isolated by means of sulphuric acid and sulphuretted hydrogen. It is a dibasic acid; its composition is:



$$2HO(C_{12}H_4O_2)''\begin{pmatrix}S_2O_4\\S_2O_4\end{pmatrix}O_2$$

It is readily soluble in water and alcohol, and with difficulty obtained in crystals. Its salts, with the exception of the basic lead salt, are also very soluble in water and alcohol.—(*Ann. Chem. Pharm.* cxliii. 58.)

MISCELLANEOUS.

Quekett Microscopical Club.—The monthly meeting was held at University College, on Friday evening, September 27, Mr. A. E. Durham, President, in the chair. Mr. Glade read a paper on "snail's teeth," in which he described those organs of mollusca known as the tongue or palate, consisting of a long and narrow strip of membrane on which are arranged, in various patterns, successive series of strong recurved teeth, by the rasping action of which the animal is enabled to obtain its food. By this means the carnivorous mollusca bore through the shells of the animals on which they prey. The numbers, arrangement, and shape of these teeth afford to naturalists a means of determining species. Dr. Maddox exhibited a collection of beautifully executed micro-photographs of deep sea soundings, many of the objects being magnified 3,000 times.

The Director of the Paris Mint.—M. Dumas, chemist and senator, has been appointed to succeed the late M. Pelouze as Director of the *Commission des Monnaies* of Paris. M. Dumas had previously resigned his appointment of professor in the faculty of science in the University of Paris, and inspector-general of the high schools of France.

A Patent for Seeing Ghosts.—The "vital-force" patent on which we commented some little time since, has been out done by a scheme which has just come across the Atlantic. According to Dr. Van der Weyde, in the *American Journal of Mining*, a spiritualist of New York has lately applied for a patent for an arrangement to make ghosts or spirits visible. It consisted in a room from which light and air was almost excluded, only air was admitted by a stop-cock, which was opened from time to time, and light was passed, in a very small quantity, through a piece of dark-blue or black glass, or fluid, so that when first entering the room nothing was seen, but remaining in it for any length of time a very faint view of the interior was obtained. The inventor asserting that the bodies of ghosts or spirits are so attenuated that common light passes straight through them and makes them invisible.

Carbonic Disulphide, Hydrate of.—E. Duclaux. When carbonic disulphide is rapidly volatilised, a white crystalline mass is formed which is a hydrate of carbonic disulphide. The crystals are very unstable, they decompose at $-30°$; their composition is $2CS_2+H_2O$.—(*Comptes R.* lxiv. 1099.)

Pascal and Newton.—The impudent attempt on the part of some French academicians to deprive Newton of the glory of the Law of Gravitation has now been effectually exposed. It will be remembered that when our Paris correspondent alluded to them a fortnight ago, we characterised Pascal's letters as forgeries. It will be seen from the following statement that Newton's letters are also forged. Sir David Brewster writes as follows to the *Athenæum*:—"As the biographer of Sir Isaac Newton, and the only living person who has examined his letters and MSS. in the possession of the Earl of Portsmouth, I feel that I am called upon to expose the forged correspondence between him and Pascal which has recently been presented to the French Academy of Sciences, and published in successive numbers of the *Comptes Rendus*, etc. After perusing this correspondence, I communicated to M. Chevreul, the President of the Academy, the most satisfactory evidence that the letters are forgeries; but as my letter may not be published till the Committee of the Academy give in their report, I am anxious that the truth, in so far as I can state it, should be known in this country. If the correspondence in question is genuine, Pascal has anticipated Newton in the discovery of the Law of Gravity; and our French foe across the Channel might justly charge Mr. Conduitt, Bishop Horsley, and myself—who, I believe, are the only persons who had thoroughly examined the papers of Sir Isaac Newton—with having destroyed the letters of Pascal, in order to give to Newton the honour, and to England the glory, of so great a discovery. 1. In the Portsmouth papers there is not a single letter from Pascal to Newton, nor any letter or document in which his name is mentioned. 2. Pascal is alleged to have heard of Newton's *precocious* genius as a mathematician, and to have written to him encouraging letters, when he was only *eleven* years of age! Newton was not a precocious genius. His great powers were very slowly developed. Till he was *sixteen* he was occupied with water- and wind-mills and dials; and, as he himself told Mr. Conduitt, his *first* experiment was made in 1658, when he was *sixteen*—an experiment, too, indicating very little genius. 3. Newton's mother, under the name of *Anne Ayscough*, thanks Pascal for his attention to her son; but *Anne Ayscough* ceased to have that name when Newton was only *four* years old, and had she written after that time it could only have been as *Hannah Smith*. 4. The letters of Newton are signed *I. Newton* and *Isaac Newton*. Newton's letters of correspondence were always signed *Is. Newton*; the only exception I know being when he signed *Isaac Newton* to a long scientific communication to Boyle. 5. According to the alleged correspondence, Newton received *at least two hundred* manuscripts and notes from Pascal, which he offered to return; but it does not appear that the offer was accepted. 6. Newton never wrote in French; his letters to Varignon and other French *savants* were always written in Latin. 7. The letters contain internal evidence that they were not written by Newton. He never could have expressed an *eternal* gratitude for the kindness of his friend. 8. An examination of the handwriting and of the paper by an English expert will, doubtless, add to the evidence given above, that the correspondence in question is not genuine."

Gun-Cotton Explosion.—Messrs. Prentice and Co. write to explain that the temperature of 170 would have been more correctly written 170°C. (or Centigrade) which is equivalent to about 349°F. (or Fahrenheit), the more usual English scale. This has long been considered the ordinary explosive point of gun-cotton. Since the introduction of the present improved system of washing the material after it has been reduced to a state of pulp, they have not an instance in their experience, extending over several months, where the explosive point has been found to be under 350° Fahrenheit. Without expressing any gratification on such an occasion, they cannot help feeling some satisfaction that such a quantity of gun-cotton, when not closely confined, could be exploded without even the fracture of a pane of glass in any of the buildings only ten yards distant, forming on a grand scale an illustration that it is only when subject to a considerable degree of resistance or confinement that the destructive nature of gun-cotton is fully developed. The sporting cartridges made of "safety gun-cotton paper" (of which there were none in this part of the works) may be safely stored in any closet or cupboard, the particular mode of preparation rendering them still less liable to ignition, and harmless unless confined, as in the barrel of a gun.

Sulphate of Alumina.—*Potash alum* is composed of

$$KO,SO_3,Al_2O_3,3SO_3+24HO=474.5.$$

Ammoniacal alum, the most generally used in Paris,

$$NH_4O,SO_3,Al_2O_3,3SO_3+24HO=381,4.$$

Simple Sulphate of Alumina,

$$Al_2O_3,3SO_3+24HO=315,4.$$

Every 100 kilogr. of these three products contains the following proportion of sesquioxide of aluminium.

For 100 kilo.	potash alum...........	10·820	kilo.	of Al_2O_3
" "	Ammoniacal ditto.......	13·460	"	"
" "	Sulphate Alumina......	16 270	"	"

Thus, it is easy to find the real value of the simple sulphate alumina, the more so as that which constitutes the real value of alum, is not the potash or the ammonia, but only the alumina. It is the alumina that the dyer wants to fix his colours in a state of lacker; the tanner wants the alumina to preserve his hides, and make them fit for gloves and shoes; again, the paper-maker requires the alumina to make his pulp fit for writing on, that is to say, that paper which contains a resinate of alumina obtained from the double decomposition of sulphate alumina with a solution of resin in caustic soda,—this paper, we say, can take writing without fear of the ink running. If the alumina in these salts is the only part useful in commerce, one ought to look for a product which to a give weight shall contain the largest quantity of this alumina. Preference should be given to sulphate alumina, which contains 16 per cent. useful product, whilst potash alum only contains 10·1 per cent.—*Moniteur Scientifique*, vol. ix., p. 574.

The Atmosphere of the Metropolitan Railway.—An inquest was held on Friday last on the body of Elizabeth Stainsby, who died very suddenly while travelling on the Underground Railway. This is the third sudden death on the line within a few weeks. Previous to entering the Bishop's-road station she complained of a pain in her chest. Upon reaching the platform she remarked, "It a very nice station, but it feels very hot." As soon as the train started she exclaimed, "Dear me, what a dreadful smell there is," and these were the last words she uttered. When they had proceeded some distance into the tunnel she seemed to suffer great inconvenience, and fell sideways, with her head upon the shoulder of her companion. After leaving Gower-street station she struggled and gasped a great deal, but upon reaching King's cross she sank apparently fainting. Although Farringdon-street was her destination, she was at once removed to the waiting-room at King's-cross, and a medical man was sent for, but it was found she was quite dead. Dr. Popham, who made a *post-mortem* examination, when pressed to state whether, in his opinion, the atmosphere of the Underground Railway had hastened death, declined to give a decided answer. He had no doubt, however, that air containing a large quantity of sulphurous acid gas and carbonic acid gas would hasten the death of a diseased person. The companion of the deceased stated that the atmosphere of the tunnel between Portland-road and King's-cross was particularly oppressive. In accordance with the strong opinion of the jury, the coroner adjourned the inquiry for the purpose of obtaining the result of a chemical analysis of the atmosphere of the tunnel. We since learn that the directors of the Metropolitan Railway being anxious that the facts should be inquired into by competent and impartial persons, so as at once to remove all possible cause for anxiety, have requested Dr. Letheby, the medical officer of health for the city of London, in conjunction with Dr. Whitmore, the medical officer of health of Marylebone, and Dr. Bachhoffner, to report to them, after a thorough investigation upon the subject, and those gentlemen are now engaged upon the inquiry.

New Mode of Anatomical Research.—Professor Braune, of the University, Leipsic, has just published a method of making accurate drawings of the human system, which is at once novel and startling. He first freezes the subject to a metallic hardness by exposing it to a temperature many degrees below zero for a sufficient period of time, then with a fine saw he severs the frozen body in any direction as may be desired. If proper saws are used, these cuts will be perfectly clean and smooth; over these cuts a stream of water is poured, which instantly freezes, as the whole operation is carried on in a room at a low temperature, and the ice forms a sort of transparent coating to the severed surface, revealing very distinctly every part and outline.

Popular Scientific Information.—Tin Assays.—In the *Mining Journal* of Aug. 17, in a report upon the Missouri tin discoveries, we find the following:—"Professor H. M. Beauregard, a graduate of the Paris School of Mines, writes as follows." "I have three comparative assays with specimens obtained from the surface of the code. First, from a light to a dark green colour, showing in an unmistakable manner the presence of black tin, exhibiting the same characteristics, as specimens from the tin mines of Saxony. Second, from some specimens of yellow and gray yellow streaks, containing a small quantity of tin; and if we take into consideration their position of the surface, they present very good indications. Third, the brown specimens contain no metal; however, the covering of 'putty' which is found very abundant, is of a rich quality, and if we consider that these assays were made in open air, that tin is the most *oxydable* of metals, and that it is necessary to obtain a temperature of heat equal to 442° Fahrenheit in order to smelt it into ingots, the object in view, to establish the fact of the presence of tin, is reached"!!

Silicium-mercaptan.—C. Friedel and A. Ladenburg. Pierre's silicic chlorosulphide, which is prepared by passing a current of hydric sulphide, charged with the vapour of silicic chloride, through a red-hot porcelain tube, the authors find to be a mixture of silicic chloride, and a body of the composition $SiCl_3SH$. They may be separated from each other by repeated fractional distillations, the new compound distilling between 95° and 97°, silicic chloride at 59°–66°. The reaction by which the silicium-chlorsulphhydrate is formed may be represented by the following equation:

$$SiCl_2+H_2S=SiCl_3SH+HCl$$

Bromine gives rise to the formation of silicic chlorobromide according to the equation:

$$SiCl_3HS+3Br=SiCl_3Br+BrS+BrH.$$

This bromide resembles silicic chloride closely; it boils at 80°, and gives off fumes when exposed to air. Its density was found 7·25.

Temperature required for forming Fusible Combinations, and for melting the same.—C. Schinz. Schinz finds by application of a thermo-electric pyrometer, that silicates are formed and melted at the same temperature, and that the formation of the silicates depends more on time than on temperature, *i.e.*, it depends, in fact, on the conducting power of heat, which the materials composing the silicates possess. He also finds the temperature required for melting metals and metallurgical products to be lower, as formerly has been stated by Plattner. The latter states that a temperature of 1,789°—1,876°C. was required for forming silicates of iron, and of 1,431—1,445° for melting the same. Schinz now finds that a temperature of 1,000—1,156°C. is sufficient for both purposes. He also finds that a temperature of a glass-furnace in operation is only 1,100—1,250°; that crystal glass is worked at 833°, and becomes completely liquid at 929°. A Bohemian green glass tube softens at 769°, and becomes liquid at 1,052°. Pure limestone loses its carbonic acid by heating for several hours at a temperature of 617—675°C. An increase of the temperature will shorten the time.—(*Dingl. J.* bd. 182, p. 206.)

Rapid Reporting.—We would draw attention to the promptness, hitherto unknown in the scientific press, with which the proceedings of the British Association were reported last week in these columns. It should be remembered that Dundee is fifteen hours' railway journey from London; but the Chemical News, which was in the hands of the public on Friday morning last, contained a full account of the proceedings in Section B of the previous day. Not only was Dr. Anderson's introductory address given in full, but the two most important papers were also reported, and the discussion given. All the important papers have been reported for us verbatim, and the discussions and papers of minor importance are reported in abstract; but our available space, although extended by the issue of a supplement, is still insufficient, and we are reluctantly compelled to defer the concluding part of the proceedings till next week.—Chemical News (*Eng. Ed.*) *Sept* 13, 1867.

Origin of Gypsum and Dolomite.—In a memoir read before the *Académie des Sciences*, 22nd April, this year —"Sur la formation des gypses et des dolomies" (*Compt. Rend.*, tome lxiv., p. 815), Dr. Sterry Hunt gives the results of an interesting experimental investigation on the origin of these rocks. After alluding to a previous communication to the Academy (23rd May, 1859), in which he demonstrated that the mutual reactions of bicarbonate of lime and sulphate of magnesia would give rise to the formation of gypsum and hydrated carbonate of magnesia, he proceeds to account for the origin of the carbonate of magnesia, which, under the form of dolomite, is found so abundantly in nature, unaccompanied by gypsum. He regards in the first place the carbonates of soda, lime, and magnesia, to have been formed from the decomposition of primitive silicates by atmospheric carbonic acid, and that the carbonate of soda so formed precipitated first the lime and then the magnesia (more or less mixed with lime) from the primeval ocean; in cases of isolated basins of water previously deprived of its lime, carbonate of magnesia would be alone precipitated. The most important point in this investigation, however, is the discovery announced by Dr. Sterry Hunt of the mode in which the chemical reactions concerned in the production of dolomite and gypsum are modified under the influence of an atmosphere of carbonic acid, from which he infers that the ancient period was much more favourable to the development of these rocks, since the atmosphere of the primitive epoch must doubtless have contained much more carbonic acid than at present; and concludes by ascribing to this cause the production of the great masses of gypsum associated with dolomites which are found in the most ancient formations up to those of the tertiary period.

M.P. for London University. Candidature of Sir John Lubbock.—An influential meeting was held, in the Committee Room of Section D., of gentlemen anxious to assist the Committee of Graduates formed to secure the election of Sir John Lubbock as representative of the University of London. Among those present were—Sir W. Thomson, President of Mathematical and Physical Section; Dr. Sharpey, President of the Section of Anatomy and Physiology; Prof. Busk, President of the Section of Zoology and Botany; Prof. Wheatstone; Prof. Sylvester; Prof. Tyndall; Prof. Allen Thomson; Prof. Ansted; Dr. Williamson; Mr. Gassiott; Prof. Hirst; Dr. Odling; Dr. Turner; Prof. M. Foster; Prof. G. C. Foster; Dr. A. C. Brown, etc. Professor Tyndall was in the chair; and it was proposed by Sir William Thomson, and seconded by Professor Williamson, and carried unanimously:—"That Sir John Lubbock, Bart., having been brought forward by an influential party among the graduates of the University of London, and an opportunity being thereby afforded of obtaining for science a representative in the House of Commons, it is highly desirable that those who are interested in science should do all in their power to secure his election."

In Memoriam.—Faraday.—In his introductory address to Section A of the British Association, the President, Sir William Thomson, said: It was my intention not to detain you from the interesting subjects and abundant matter for discussion which will so fully occupy our time during the meeting, by an introductory address; but I must ask you to bear with me if I modify somewhat this resolution, in consequence of a recent event, which, I am sure, must touch very nearly the hearts of all present, and of very many in all parts of the world, to whom the name of Faraday has become a household word for all that is admirable in scientific genius. I wish I could put in words something of the image which the name of Faraday always suggests to my mind. Kindliness and unselfishness of disposition; clearness and singleness of purpose; brevity, simplicity, and directness; sympathy with his audience or his friend; perfect natural tact and good taste; thorough cultivation—all these he had, each to a rare degree; and their influence pervaded his language and manner, whether in conversation or lecture. But all these combined made only a part of Faraday's charm. He had an indescribable quality of quickness and life. Something of the light of his genius irradiated all with a certain bright intelligence, and gave a singular charm to his manner, which was felt by every one, surely, from the deepest philosopher to the simplest child who ever had the privilege of seeing him in his home—the Royal Institution. That light is now gone from us. While thankful for having seen and felt it, we cannot but mourn our loss, and feel that whatever good things, whatever brightness may be yet in store for us, *that* light we can never see again.

An American View of English Patent Law.—We quote the following from our talented contemporary, *The American Journal of Mining*. From the amusing illustration quoted in the latter part, it would appear that the American patent laws are at least as elastic as our own. "The Chemical News describes, with well-merited ridicule, an apparatus patented Oct., 1866, in England, for the generation of 'vital force' by the contact of 'an azote and a carbonated body.' The extract will be found in another column. The News justly says, 'a more powerful satire on the present state of the patent laws we have never seen. The patent laws of England are perhaps more absurd than that of any other country. They date back to a period when it was desired to transplant into England the secrets of European manufacturers; and they were primarily intended to reward those enterprising individuals who should courageously spy out the discoveries which others had made, and then, with sudden virtue, wish to be protected in their rights to the stolen property. If we mistake not, it is still a feature of the English system, that the patentee need not claim to be the original inventor, and that foreign inventors have no rights which patentees are bound to respect. A natural consequence of this fundamental injustice is an extreme looseness in the administration of the patent laws. It is proverbial that anything can be patented in England. The governmental examination amounts to little or nothing. The records show many cases of the same inventions repeatedly patented by different parties; of patents covering only elementary principles, which have long been public property; and of preposterous bits of quackery like this 'battery of vital force,' receiving the sanction of parchment and the royal seal. We pride ourselves on our superior system; but is it free from similar faults in administration? It is certain that our citizens would be saved much perplexity, and our courts much vexatious business, by a stricter examination of patents in the beginning by the government officials. As an offset to the amusing instance presented by the *News*, we might mention an American patent, obtained a year or two ago, for preserving the bodies of deceased relatives, by subjecting them to hydraulic pressure, for the purpose of expressing all moisture! We should like to submit a body, prepared by *our* patent, to the inventor of 'vital force,' and see whether, by the application of azote and carbon, he could 'bring it to!'"

The Magnesium Light.—We understand that this light is likely to play an important part in the Abyssinian expedition. Mr. Mellor, the manager of the Magnesium Metal

Company, is prepared to supply several hundred-weights of the powdered metal; and the authorities at Chatham have been for some time experimenting on the subject.

Liquid Carbonic Acid.—The well-known apparatus employed for so long a time by Mr. Robert Addams for liquefying carbonic acid, has been purchased by Mr. Stewart from funds supplied by the Royal Society, and Mr. Addams has kindly undertaken to make a preliminary experiment with his apparatus, as well as to give specific instructions regarding it. As the exact thermometric value of the freezing-point of mercury has been priviously determined by Mr. Stewart, it is expected that the apparatus will furnish the means of verifying thermometers at very low temperatures.

Iodine soluble in certain Organic Compounds.—Hlasiwetz. Iodine dissolves to a considerable extent in aqueous solutions of resorein, orein, or phloroglucin, without imparting to them any colour. The solutions may be boiled without iodine being volatilised; they have almost neutral reaction, and starch, or carbonic bisulphide does not indicate free iodine. A solution of the latter in alcohol or carbonic bisulphide is decolorised by adding one of the organic bodies mentioned, which may therefore be used in place of sulphurous acid in volumetric determinations by means of iodine. Other organic substances have been observed to behave in a similar, but less decided manner.—(*Akad. Wien*, 131, 1867.)

CONTEMPORARY SCIENTIFIC PRESS.

[Under this heading it is intended to give the titles of all the chemical papers which are published in the principal scientific periodicals of the Continent. Articles which are merely reprints or abstracts of papers already noticed will be omitted. Abstracts of the more important papers here announced will appear in future numbers of the Chemical News.]

Bulletin de l'Académie de Belgique. March 2, 1867.

A. Kekule: "*Reports on E. Dubois' Memoir on Monochlorinated Phenic Acid.*"—Van Beneden: "*Report on F. Terby's Memoir on the Method pursued by Spiders for connecting Distant Points by a Thread.*"—A. Quetelet: "*On the Dermination of the Hour at which Falls of Aerolites generally take place.*"—A. Kekule: "*On some Sulphuretted Derivatives of Phenol.*"—"*On the Sulphophenic Acids.*"—E. Dubois: "*On Monochlorinated Phenic Acid.*"—F. Terby: "*On the Method pursued by Spiders for connecting Distant Points by a Thread.*"

April 6.

Sitzungsberichte der königlich-Bayerischen Akademie der Wissenschaften. (Mathematisch-physikalische Classe.) March 10.

"*List of Subjects for Prize Essays foa the Year 1868.*"

Schonbein: "*Contributions to the Knowledge of Binoxide of Hydrogen.*"—Von Kobell: "*On Pectolite and Osmelite.*"—A. Vogel, Junr.: "*On the Variations in the Composition of Water at Different Depths.*"—"*On the Estimation of Ammonia.*"—Bauernfeind: "*On Atmospheric Refraction.*"—"*Experiments on Capillary Action at a Low Atmospheric Pressure.*

April 21—May 5.

Nageli: "*Experiments on Capillary Action at a low Atmospheric Pressure.*" "*On the Theory of Capillarity.*"

June 2.

A. Vogel, Junr.: "*On the Manufacture of Peat Charcoal.*"—Schonbein: "*On the Formation of Binoxide of Hydrogen during the Slow Oxidation of Organic Substances.*"

July 14.

A. Vogel, Junr.: "*On the Estimation of the Chemical Action of Light by its Influence on Prussian Blue.*" "*On the Volatile Acids contained in Peat, and on the Variation in Quality of Peat from Different Portions of the same Bed.*"

November 10.

Von Pettenkofer and Voit: "*On the Quantity of Carbonic Acid exhaled and Oxygen consumed by the Human Subject daring Waking and Sleeping, in Health and Disease.*"—A. Vogel, Junr.: "*On the Assimilation of Silica by Plants.*"—Von Gorup-Besanez: "*Researches on Creosote.*"—Rechnagel: "*On the Expansion of Alcohol by Heat.*"

December 15.

Steinheil: "*On a Portable Photographic Apparatus.*"—E. Voit: "*Researches on the Laws of Diffusion of Liquids.*"—Schonbein: "*On the Accelerative Action of Fluid Hydrocarbons and other Substances rich in Carburetted Hydrogen on the Oxidation of Absolute Alcohol, and on the Formation of Peroxide of Hydrogen which accompanies such Oxidation.*"

Sitzungsberichte der Wiener Akademie (Mathematisch-naturwissenschaftliche Classe). October—November, 1866.

A. Schrauf: "*On the Optical Properties of Crystals and of Allotropic Modifications.*"—"*On the Relations between Refractive Equivalents and Specific Volumes.*"—T. Peckolt: "*On the Chemical Composition, Preparation, and Exportation of Guarana or Uarana.*"—L. Ditscheiner: "*On the Theory of Diffraction in Double Refracting Media.*"—T. Hein: "*Analysis of a Meteorite from Dacca, Bengal.*"—R. L. Maly: "*On some Derivatives of Thiosinamine.*"—H. Hlasiwetz and A. Grabowski: "*On Carminic Acid.*"—G. Malin: "*On a Derivative of Rufigallic Acid.*"—F. Rochleder: "*On the Tannin of the Horse Chestnut.*"—L. Barth: "*On Paraoxybenzoic Acid.*"—J. Loschmidt: "*On the Theory of Gases.*"

January, 1867.

H. Hlasiwetz: "*On some Tannic Acids.*" "*On the Constituents of Tea.*" *On the Basicity of Gallic Acid.*"—L. Barth: "*On Protocatechuic Acid.*" "*On the Brominated Derivatives of Gallic Acid, Pyrogallic Acid, and Oxyphenic Acid.*"—E. Schwarz: "*Analysis of the Mineral Waters of Medling, near Vienna.*"—M. Vingtschau: "*On the Action of Physostigmine on the Amphibia.*"—A. Siersch: "*On the Action of Common Salt on Zinc and Oxide of Zinc.*"—A. Brio: "*On the Optical Properties of Crystals of Hyposulphite of Baryta.*"—A. Lielegg: "*On the Spectrum of the Flame from Bessemer Converters.*"

Kunst and Gewerbeblatt. April, 1867.

Schafhault: "*On the Cause of the Brittleness of Brass Wirwhen used for Lightning Conductors.*" "*On the History and Progress of Tanning in Germany.*"—E. Dietrich: "*On Fusel Oil and its Applications.*"—K. Schlee: "*On the Production of Artificial Meerschaum and Horn from Potatoes, Turnips, and Wood.*" "*On Plastic Wood.*" "*On the Composition and Utilisation of the Wash Waters of the Wheat Starch Manufacture.*" "*On Iron Minium as a Paint for Wood and Metal.*" "*Price List of Philosophical Apparatus manufactured by P. Carl, at Munich.*"

May.

W. Venuleth: "*A Press for forming Spent Tan into Cakes for Use as Fuel*"—E. Langen: "*On an Arrangement of Apparatus for mechanically emptying the Cooling Cylinders of Retorts for revivifying Animal Charcoal.*"—E. Ziegler: "*On the Presence of Carbonate of Lime in Brick Clay, on its Influence on the Ware, and on the Methods of preventing its injurious Action.*"—R. Linner: "*On the Use of Moveable Tubs as Receptacles for Excrementitious Matters at Gratz.*"

Kunst and Gewerbeblatt. May.

"*On the Crystallisation of Glycerine and on the Action of Impure Glycerine on the Skin.*"—R. Wagner: "*On the Quantitative Estimation of Essence of Mirbane (Nitrobenzol) in Oil of Bitter Almonds.*"

Journal für Praktische Chemie. April 25, 1867.

H. Kolbe: "*Remarks on Heintzel's Memoir on Triomidophenol.*"—Bottger: "*On the Use of Antimony in Voltaic Batteries.*"—F. Hoppe-Seyler: "*On the Presence of Indium in Wolfram.*"—P. Schottlander: "*On Hyposulphite of Platinum and Soda.*"—F. Baumstark: "*On the Action of Oxychloride of Sulphuric Acid on certain Organic Compounds.*"—A. Lielegg: "*On the Spectrum of the Flame from Bessemer Converters.*"

May 9.

R. Hermann: "*On the Atomic Weight of Tantalum, and on the Composition of the Compounds of that Metal.*" "*On a Double Fluoride of Antimony and Arsenic.*"—T. Petersen: "*Contributions to the Theory of Leblanc's Process.*"—A. C. Oudemans: "*Experiments on some East Indian Fats and Oils.*"—"*Experiments on Palm Oil from Surinum.*"—W. F. Gintl: "*A new Pinch-cock for Stopping India Rubber Tubes.*"—H. Hlasiwetz: "*On Hydrocaffeic Acid.*"—G. Tschermak: "*On Glaudocote, Danaite, and Arsenical Pyrites.*"

May 24.

J. G. Gentele: "*On C. Friedel and J. Crafts' Paper, or a new Alcohol in which Carbon is partly replaced by Silicium, published in the* Comptes Rendus, *vol. 61, p. 792.*"—"*On the Boiling Points of Ethers and Alcohols, and of the corresponding Sulphides and Sulphhydrates.*"—"*On the supposed Identity of Benzylamine and Toludine.*" "*On the Similarity of the Behaviour of Carbonic Oxide and Nitrous Oxide in Chemical Compounds, where they replace a Base or an Acid.*"—C. F. Schonbein: "*On the Accelerative Action of Liquid Hydrocarbons and other Bodies rich in Carburetted Hydrogen on the Oxidation of Absolute Alcohol, and on the Formation of Peroxide of Hydrogen which accompanies such Oxidation.*"

Monatsbericht der königlich-Preussischen Akademie. Feb., 1867.

J. Bernstein: "*On the Duration of the Negative Variation of Nervous Currents.*"—R. Weber: "*On some Compounds of the Chlorides of Platinum and Gold.*"—Dove: "*On the Combination of Prismatic Colours to White.*" "*On the Production of Accidental*

Colours by the Electric Light." "*On the Inversions which occur on looking at Drawings in Perspective and Transparent Objects with one or both Eyes.*" *On the Polarisation of Light by Repeated Reflection.*"—Poggendorff: "*Observations on Holtz's Electrical Machine.*" "*On a new Electrical Machine invented by Holtz.*"

Journal des Fabricants de Papier. May 15, 1867.

E. Bourdilliat: "*On Testing the Chemical Products used in Paper Making.*"—J. Nickles: "*On E. Porion's Method of Utilising Spent Lyes and other Waste Products of Paper Mills.*"

Archives des Sciences. May 25, 1867.

M. Micheli: "*On the Colouring Matter of Chlorophyll.*"—L. Dufour: "*On A. Fick and J. Wislicenus' Paper on the Production of Muscular Force.*" "*On Franklin's Paper on the same Subject.*"

Comptes Rendus. June 10, 1867.

Boussingault: "*On the Decomposing Action of a High Temperature on some Sulphates.*"—Payen: "*On the Structure and Constitution of Ligneous Fibres, followed by an Account of the Methods of manufacturing Paper from Wood.*"—A. De la Rive: "*On the Electrical Condition of the Earth.*"—L. Simonin: "*On the Bituminous Schists of Vagnas, in the Department of Ardeche, Fance.*"—Chacornac: "*On the Periodicity of Sun Spots.*"—P. Volpicelli: "*On the Determination of the Poles of Bar Magnets.*"—S. du Luca and J. Ubaldini: "*Researches on the reciprocal Action of Sulphurous Acid and Sulphuretted Hydrogen.*"—C. Forthomme: "*Remarks on M. Perret's Improved Wine Fermenting Vat.*"

Annalen der Chemie und Pharmacie. June, 1867.

A. Naumann: "*On the Specific Heat of Gases for equal Volumes under Constant Pressure.*" "*On the Velocity of the Movements of Atoms.*"—E. Rubien: "*On Œnanthylidene and Caprylidene.*"—P. Janasch: "*On Trichlorodracylic Acid.*" "*On Trixylylamine.*"—O. Pieper: "*On a Chlorinated Derivative of Toluol.*"—A. Baeyer: "*On Neurine.*" "*A Lecture Experiment demonstrating the Action of Nitric Acid on an alcoholic Solution of Propargylic Ether.*"—C. Graebe: "*On a new Method of Forming Methylsalicylic Acid.*"—C. Graebe and O. Born: "*On Hydrophthalic Acid.*"—C. Graeve and O. Schultzen, "*On the Behaviour of the Aromatic Acids in their Passage through the Body.*"—"*On Methoxybenzoic Acid.*" H. Hlasiwetz: "*On the Hydrocaffeic and Hydroparacoumoric Acids.*"

Dingler's Polytechnisches Journal. May 2, 1867.

C. Schintz: "*On J. Lundin's Improved Regenerative Gas Furnace.*" "*On the Calorific Value of Austrian Coals.*"—R. Weber: "*Contributions to the Knowledge of the Manufacture of Sulphuric Acid in Lead Chambers.*"—P. Buchner: "*On the Estimation of Tannic Acid in Oak Bark.*"—J. Malmedie: "*Apparatus for Bleaching Flax Yarns.*"—A. Lielegg: "*On the Spectrum of the Flame from Bessemer Converters.*"

May 18.

G. Schnitzer: "*Analyses of Bauxite from Austria.*" P. Buchner: "*On the Estimation of Tannic Acid in Oak Bark.*" J. C. Lermer: "*On the Destruction of Wooden Brewing Utensils by Fungi.*" H. Dufrene: "*On Rousseau's Improvements in Treating Beet Juice with Lime.*"—C. Kurtz: "*On the use of Canada Oil as a Substitute for Bisulphide of Carbon in the Extraction of Oil from Seeds.*"—J. Stinde: "*On the Preparation of Spirits of Nitre.*"—O. Reinch: "*On Colouring Thin Sheets of Metal, and rendering Membranes, Fabrics and Glass iridescent.*"—Cordure: "*A method of obtaining Silver from Argentiferous Lead by means of Zinc.*"—Clemandot: "*A new Silica Glaze for Pottery Ware.*"—B. Hoffmann: "*Some remarks on Ozokerite.*"

Comptes Rendus. June 17, 1867.

Becquerel: "*On some new-discovered Chemical Effects of Capillary Action.*"—A. De la Rive: "*Note on a Photometer for Measuring the Brightness of Distant Objects, and on the increased Transparency of the Atmosphere due to the Presence of Moisture.*"—Chevreul: "*On the same Subject.*"—J. Lefort: "*Researches on the Chemical History of Humus.*"—Pool: "*Note on an explosive Compound obtained by treating Glue with Chlorate and Nitrate of Potash.*"—Silbermann: "*On some Peculiar Phenomena observed in a Shooting Star on June 11, 1867.*"—Reboul and Truchot: "*On Ethylate of Amylene, an Isomer of Ethylamylic Ether; followed by some Remarks on the Production of Mixed Ethers.*"—F. C. Calvert: "*On Oxidation by means of Oxygen condensed in Charcoal.*"—Lecoq de Boisbaudran: "*On some Experiments on Supersaturation.*"—C. Girard and P. Chapoteaut: "*Contributions to the Knowledge of Ethers.*"—E. Maumené: "*Answer to Forthomme's Remarks on the Author's Paper on an Improved Fermenting Vat.*"—Dubrueil and Legros: "*Researches on the Physiological Action of Sulpho-cyanide of Potassium.*"

June 24, 1867.

Artur: "*On Becquerel's Memoir on the Chemical Effects of Capillary Action.*"—Zaliwski-Mikorski: "*On the Effect of increasing the Height of the Elements of a Voltaic Battery, the Base remaining unchanged.*" "*An improvement in the Bunsen Battery.*"—A. Baudrimont: "*On the Estimation of Organic Matter, Phosphoric Acid, and Nitrogen in Peruvian Guano and other Manures.*"—Eulenburg and Guttmann: "*On the Physiological Action of Bromide of Potassium.*"—J. Pernod: "*On the Preparation of Madder for Calico Printing in the Topical Style.*"—Chevreul: "*On the same Subject.*"—C. Friedel and A. Ladenburg: "*On a Silicic Mercaptan.*"—R. D. Silva: "*On Compound Ammonias with an Amyle Base.*"—Janssen: "*On the Composition of the Gases emitted from the Volcano at Santorin.*

Vol. 65. No. 1. July.

A. Gaudin: "*On the Special Function of Hydrogen in Acids, and particularly in Polybasic Acids.*"

Monatsbericht der königlich-Preussischen Akademie. March, 1867.

W. Kuhne: "*On the Digestion of Albuminous Substances by the Pancreatic Juice.*"—Poggendorff: "*On the Use of Paper Pyroxylin as an Electroscope.*"—G. Rose: "*On the Formation of Crystals in Beads of Borax and other Blowpipe Re-agents, regarded as the Cause of the Opacity of such Beads when allowed to cool.*" "*On the Preparation of Anatase and the other Allotropic Forms of Titanic Acid.*"

Annales de Chemie et de Physique. May, 1867.

G. A. Hirn: "*Memoir on Thermodynamics.*"—E. P. Berard: "*Letter to Dumas on the Invention of a Furnace for the Continuous Combustion of Sulphur in Sulphuric Acid Chambers.*"—S. M. Joergensen: "*On the Periodides of the Alkaloids.*"

June.

B. Renault: "*An Experimental Verification of the Reciprocal of Faraday's Law of the Decomposition of Electrolytes.*"—A. Scheurer-Kestner: "*Some new Researches on the Theory of Leblanc's Process for the Manufacture of Soda*"

Annales du Génie Civil. June, 1867.

E. Ferrand: "*On the History, Preparation, Properties, Action on the Workmen employed in the Manufacture and Applications of Coal Tar Dyes.*"—Griess: "*On Chlorochromate of Diazobenzide, a new Explosive Compound.*"—Fortier and Philippe: "*On the Use of Carbonate of Ammonia for Washing Wool and Cloth.*" "*On a Method of Renovating Files by Etching with Sulphuric Acid.*" "*On a new Fuel for Steam Engines, consisting of Peat saturated with Petroleum.*"

Le Technologiste. June, 1867.

H. Wagner: "*On a new Method of Treating poor Copper Ores in the Wet Way.*"—P. Le Guen: "*On a Method of Alloying Bessemer Steel with Tungsten.*"—L. Joulin: "*On the Deposits of Potash and Soda Salts at Stassfurt.*"—A. Scheurer-Kestner: "*Some new Researches on the Theory of Leblanc's Process.*"—F. Stolba: "*On the Preparation of Sulphurous Acid.*"—E. Gostmann: "*On the use of Paraffine for checking the Violent Ebullition of Syrup in Evaporating and Vacuum Pans.*"—H. A. Archereau: "*A Method of Manufacturing Oxygen by decomposing Sulphuric Acid, and on using the Gas produced, in Combination with Hydrogen, for Illuminating Purposes.*"—C. Puscher: "*On the Manufacture of Artificial Meerschaum and Horn.*"—A. Paraf: "*On the use of a Glyceric Ether in Dyeing.*"—De Laire, C. Girard, and Chapoteaut: "*On Mauvaniline, a new Coal Tar Dye.*" "*On a new Method of Dyeing Sewing Silk Black.*"—C. Jacobensen: "*On a new Marking Ink prepared from Aniline.*"—C. Liebermann: "*On a Method of Distinguishing Wool and Cotton in Fabrics and Threads.*"—C. Winkler: "*A Process for Purifying Graphite.*"

PATENTS.

Communicated by Mr. Vaughan, F.C.S., Patent Agent, 54, Chancery Lane, W. C.

GRANTS OF PROVISIONAL PROTECTION FOR SIX MONTHS.

2102. C. King, Regent Street, W., "An improvement in the preparation of chocolate and cocoa." Petition recorded.—July 17, 1867.

2410. J. G. Marshall, Leeds, "Improvements in solvent or detergent processes."—August 22, 1867.—Invention protected by the deposit of a complete specification.

2453. J. Storey and W. E. Bickerdike, Lancaster, and W. V. Wilson, Jubilee Street, Mile End, Middlesex, "A new method of bronzing metallic and other surfaces." Petition recorded.—August 28, 1867.

NOTICES TO PROCEED.

1190. J. H. Johnson, Lincoln's Inn Fields, Middlesex, "Improvements in the treatment of peat, and in the manufacture of peat charcoal, and in the machinery or apparatus employed therein."—A communication from A. Frigge, Hanover.—Petition recorded.—April 24, 1867.

1212. E. Guenin, Henrietta Street, Covent Garden, Middlesex, "Improvements in the preparation and application of mustard for curative purposes."—A communication from P. Rigollot, Paris.—April 26, 1867.

1512. J. Stenhouse, Rodney Street, Pentonville, Middlesex, and J. Duncan, West Ham, Essex, "Improvements in the treatment of animal charcoal, and in the apparatus employed therein."—May 21, 1867.

[English Edition, Vol. XVI., No. 405, page 129; No. 407, page 158; No. 408, pages 172, 173; No. 407, page 158.]

2420. W. R. Lake, Southampton Buildings, Chancery Lane, "Improvements in the manufacture of iron and steel, and apparatus employed in the said manufacture."—A communication from W. W. Blanchard, Bridport, Vermont, U. S. A.—August 23, 1867.

NOTES AND QUERIES.

Carriage Grease.—Sir,—Can any of your readers kindly supply me with the composition of carriage grease, dry and wet wheel grease.—Grease.

Pyroligneous Acid.—Sir,—Will any of your readers inform me of a good method of converting crude pyroligneous acid into pure acetic acid.—W. A. (Mass.)

A New Gas.—Sir,—In the *Transactions* of the Aëronautical Society of Great Britain, page 49, Mr. F. Brearey, speaking of some desiderata in aërial locomotion, said that "if it were possible to discover a non-inflammable gas, the object would be attainable without danger. He mentioned this not without hope, as he had received a letter from a gentleman stating thnt he had succeeded in manufacturing such a gas, which, although at present expensive, he thought might be considerably reduced in cost, and that it was nearly of the specific gravity of hydrogen." Can any of your readers tell me what gas this can be? I am utterly at a loss to imagine.—Sceptic.

Naphtha.—Sir,—On referring to the 1st chapter of the second book of Maccabees, verses 19 to 36, the origin of the word naphtha will, I submit, be plainly seen. In a letter of the Jews from Jerusalem to those of Egypt, they state that when their fathers were led into Persia, the priests "took the fire of the altar privately, and hid it in a hollow place of a pit without water, where they kept it sure, so that the place was unknown to all men." After many years Neemias sent on the matter, when it was told "they found no fire but thick water." This thick water was then drawn up and laid on the sacrifice, and when the time came "that the sun shone, there was a great fire kindled." "When the sacrifice was consumed Neemias commanded the water that was left to be poured on the great stones; when this was done there was kindled a flame." "So when the matter was known it was told the King of Persia that in the place where the priests that were led away had hid the fire there appeared water, and that Neemias had purified the sacrifices therewith; then the King enclosing the place made it holy, after he had tried the matter," "and Neemias called this thing naphthar, which is as much as to say, a cleansing."—W. C.

Manufacture of Sulphurous Acid.—Sir,—Any readers of the Chemical News will greatly oblige the writer if he or they could inform him when using the coke tower, or condenser, at the end of sulphuric acid chambers if—1st. The gas that is passing out of last chamber should be dark. 2nd. The acidulated water can be used in chambers. 3d. (If possible) the quantity of water required to condense the residual gases. An early answer will greatly oblige—"A Supervisor of Chambers."

Plumbic Chloride.—Sir,—In my paper on the "Solubility of Plumbic Chloride" I find I have made a slight mistake in the specific gravity of HCl, it ought to be 1·16 not 1·116. I should have noticed it before, only I was away from home at the time it appeared.—J. Carter Bell.

Prevention of Dry Rot.—Sir,—The enquiry of "Mr. T. H. L." in No. 403 (American Reprint Chemical News, October, 1867, p. 212.) has not reached me until now, owing to my absence from home. The timber should be in *contact* with the waste. The latter may not be very easily obtainable in London; at least I am not aware of the existence of any alkali works there. If required in large quantities, the waste might be shipped from Newcastle or Liverpool.—G. Lunge.

Platinising Metals.—Sir,—I find great inconvenience from the brass-work of my balance and other instruments being attacked by acid vapours in the laboratory. Some years ago I remember seeing beams and pans of balances which had been coated with a brilliant and coherent layer of platinum, I believe by electro-deposition. Can any of your courteous readers kindly give me directions to prepare a platinising solution, and tell me what strength of battery is required?—Olinthus.

The Word Aneroid.—Sir,—Can any of your correspondents tell me the origin of the word *aneroid* as applied to a barometer? What I want is not a conjectural derivation, or an "I have always taken it to mean," or "to come from," but who was the originator of the name, and what was the sense that he put upon the word? The history of the invention might, perhaps, supply some portion of an answer. A Frenchman, somewhere about 30 years ago, is said to have been the inventor.—K.

The Word Aneroid.—Aneroid is derived from the Greek 'α privative—*without* νηρος—wet, damp, or fluid, *i.e.*, barometer without mercury, or fluid.—A.

Phthalic Acid.—Sir,—Can any of your readers give me the details of the manufacture of phthalic acid?—Naphthaline.

Preservation of Crystals.—Sir,—In reply to "Cassio's query last week (Chemical News, American Reprint, Oct., 1867, p. 211), I beg to suggest that benzol is an excellent medium in which to preserve fine crystals. Most aqueous salts are insoluble in benzol. They can be removed for examination, and after a few minutes' exposure to the air the smell of benzol will have disappeared. If immersion in a liquid is objected to they may be oiled. Many crystals which change and become dull by exposure to air, as alum, sulphate of copper, sulphate of iron, ferro-cyanide of potassium, etc., if slightly oiled, do not then alter in a long time, and many efflorescent substances are prevented from changing by the same means. Even crystals of sulphate of soda may be exposed to the air for weeks together without efflorescing if well oiled. The plan is to soak the crystals in fine olive oil for a few hours, then to wipe them on soft cambric and put them in bottles.—A. Thompson.

Platinising Metals.—Sir,—I can recommend my fellow-reader who applies for information on this subject to adopt the following process, given by Professor Church, in the *Intellectual Observer*, some time back:—"Dissolve in one ounce of distilled water sixty grains of bichloride of platinum and sixty grains of pure honey. Add to the above solution three quarters of an ounce of spirits of wine, and one-quarter of an ounce of ether. The mixed liquids, if not quite clear, must be filtered through a piece of white blotting paper. The objects to be platinised, which may be of iron, steel, copper, bronze, or brass, are to be thoroughly cleansed by washing them in soda, then in water. When they have been dried, they require heating over a lamp, to a heat below redness. For this purpose they may be suspended, by means of a fine wire, over a spirit or an oil lamp, in such a way as not to touch the flame. Suddenly, before they have had time to cool, the objects are to be completely plunged beneath the surface of the platinising liquid. One immersion for a single minute generally suffices; but the process may be repeated if necessary, care being taken to wash and dry the pieces operated upon before re-heating them. The composition of the solution may vary considerably, and yet good results be obtained. Sometimes the addition of more honey improves it; sometimes the proportion of bichloride of platinum may be increased or diminished with advantage. Indeed, it will be found that the appearance of the platinum film deposited upon the objects may be altered by changing the proportion of the bichloride present. The solution may be used several times; gradually, however, it loses all its platinum, the place of this element being taken by the iron or copper dissolved off the immersed objects." I have tried the plan and found it very successful. I am very happy to contribute my mite towards a column which has frequently given me more information than any other in your valuable journal.—E. Bearley.

Electro-magnet.—Sir,—Can any fellow-reader of the Chemical News tell me if there is any great disadvantage in using cast-iron instead of soft wrought iron for the core of an electro-magnet? For the purpose to which I wish to apply it a little residual magnetism will do no harm.—C. Linley.

German Yeast.—Sir,—Can any one put me in the way of obtaining any information respecting the particulars of the manufacture of German yeast? The supply, originally obtained from the Schiedam Vats, has failed to keep pace with the demand, and I believe it is now specially prepared for the market.—F. Ireland.

Another Specific Gravity Problem.—Sir,—Perhaps one of your obliging correspondents would point out the shortest way to solve the following:—"How much of a liquid whose s. g. is 1,000 must be added to 1,000 grain measures of a liquid whose s. g. is 1,314, to reduce its s. g. to 1,286, supposing no change to take place on mixture which would vitiate the result of the calculation, that is to say, supposing the bulk of the mixture to be the sum of the bulk of the two liquids mixed."—Henri du Chemin-creux.

Quinine.—Sir,—Could you, or any of the readers of your paper, oblige mé by informing me "how to re-dissolve quinia, which has been precipitated in the course of manufacturing ferri citr. c. quinæ, by an excess of liq. ammon. fort., in a warm solution?" An addition of more citric acid has no effect upon it.—E. S.

Aneroid.—The word *aneroid* is a contraction of *anaëroid*, derived from *αηρ*—air—and the 'α privativum—meaning a barometer founded on the action of the pressure of air on a tube, from which the air has been exhausted. It was first constructed by Vidi, and improved by Bourdon, whose well-known steam-gauges are on the same principle.—G. L.

Table of Densities.—Sir,—Could you inform me where I can purchase a table giving the decimal weight per cubic inch of sulphuric acid at the different specific gravities?—George K. Bowntiff.

Testing Cognac.—Sir,—Can you tell me whether it be possible by chemical analysis to distinguish real Cognac from spirits of wine made into so-called Cognac by the addition of "flaveur de Cognac,' or "essence de Cognac?"—C. Campbell.

Table of Densities.—If your correspondent, G. K. Bowntiff, meets with any difficulty in finding a table giving the weight per cubic inch of sulphuric acid at the different specific gravities, he may very easily prepare one by multiplying each specific gravity by 16·358 (the weight in grammes of one cubic inch of water at 16°·7 C), and each result will be the weight in grammes of 1 cubic inch of acid for that specific gravity. Of course, if the result is required in grains, the multiplier will have to be 252·45.—F. J. R. C.

Testing Cognac.—In answer to your correspondent, C. Campbell, I beg to state that the aroma left on slow evaporation of genuine spirits when gently evaporated in the hollow of the hand is so very characteristic that it is used as a criterion in the South of France to distinguish between pure *esprit de vin*, *esprit de marc de raisin*, and the spirituous fluids obtained from grain and beet-root. It is impossible to entirely eliminate from the latter the fusil-oil, but this is never present in spirits made from wine, which, on the contrary, always contain small quantities of œnanthic and acetic ethers. The smell left on evaporation of spirits not made from wine is so peculiar that it may be even recognised in the ether made from this spirit. Since the ravages occasioned by the grape disease it will be difficult to procure from France or Spain really genuine spirits, unless specially ordered. The largest distillery in the United Kingdom is almost entirely employed making whiskey for exportation to France. I should say that the ripeness of the wine, its age, the grapes it was obtained from, and the whole process of fermentation, leave an indelible impression on the quality of the spirits obtained. From my own experience, I think it is hardly likely that Mr. Campbell will be able to find a chemical test for the purpose alluded to.—Dr. Adriani.

ANSWERS TO CORRESPONDENTS.

Hugo H.—Pure glycerine is not affected by boiling with nitrate of silver solution; common glycerine frequently contains compounds which possess a rancid odour, and which reduce nitrate of silver.

Puzzled.—Alumina prevents the precipitation of the ammonio-magnesian phosphate under some circumstances. This was pointed out by Herr Knop in our last volume, page 207 (Chemical News, English edition).

J. Bignol.—Ascertain if the phosphoric acid contains phosphorous acid by adding to a solution of it an aqueous solution of sulphurous acid, and gently heating. If phosphorous acid be present, sulphur will be precipitated; if arsenic be also present, a yellow precipitate of sulphide of arsenic will be formed.

Tyro asks if we can oblige him with the name of a poison which will kill in about three days' time, and with all the symptoms of some known disease. The poison, moreover, must be one which an analytical chemist could not detect. Will our correspondent kindly forward his name and address?

Beak.—The explanation would prove too lengthy for this column. You had better refer to Wurtz's "Introduction to Chemical Philosophy," where the question is fully treated.

Percolator.—It is the ammonia which chiefly acts. The sulphur combines with hydrogen and is evolved.

A. Payne.—They are given in full in the *Journal of Gas Lighting*.

R. C. C. L.—The alterations are not important enough to be worth calling attention to.

G. J. de Winton.—The Mille Gazo-lamp is to be obtained of MM. Leplay, Noel & Co., Paris.

Specific Gravity Problem.—Several correspondents are thanked for their communications on this subject. "C. H. P.," and another correspondent who gives no signature, have each given the right solution, taking Sideros's corrected figures, viz. sp. gr. = 7·587. "H. Catheldron" uses the original data, and brings out the sp. gr. = 15·4104. "Y. O. U.," "O. Jennings," "John Fordyce," and "J. B. T." give erroneous formulæ. "Hugo Schmidt," "Ed. Moyes," "J. Hazlewood," "W. H. A.," and "E. Robinson," send elaborate criticisms on all the published formulæ, and each give correct results, with general methods for solving all such problems. To print these letters would occupy several pages, and as they only give in different language the general formulæ already published, we think little good would be gained by prolonging the discussion.

A. B.—Apply to Mr. Griffin, Garrick Street.

A. Dyer.—Use peach or Brazil wood for the colouring agent.

C. R.—You can so easily obtain the information by consulting any elementary work on chemistry, that we really must decline to occupy so much of our space as answers to your seventeen queries would require.

F. H.—Böttger's method of preparing chloride of platinum will be found in our 11th volume, p. 168 (Chemical News, English edition).

J. Mandella.—Mix white of egg with the solution; boil and strain; the precipitate will contain what you want.

G. M. A.—Not an article of commerce yet.

N. A.—Picric acid frequently contains a little nitric acid as an impurity. This has attacked the paper. Pure picric acid will not affect paper,

Pharm.—Balm of Gilead is the produce of the *Balsamodendron Gileadense*. The genuine balm is very scarce.

Edward D.—You will find a muffle very convenient for incinerating animal matters. Put the body whose ash you wish to obtain in a platinum dish, and this in a muffle heated to bright redness.

An Old Reader.—By this time you will have received an answer by post.

M. Fleming.—Soak the agate in warm oil of vitriol for some days. This will frequently bring out the bands and markings with great distinctness.

O. Dragon.—If you send the price in stamps to our publisher the book will be forwarded by post.

Inquirer.—The colouring matter is peroxide of iron. Our correspondent will perceive that it is unreasonable to expect us to perform a quantitative analysis simply to oblige an anonymous writer. The Editor is always willing to assist correspondents in difficulties, and never objects to try laboratory experiments, or even perform simple analyses in cases where it would appear that the information so obtained would be of real value; but the carrying out of a research which would occupy several days is more than a correspondent should fairly ask. By referring to our advertising columns the names of several gentlemen may be seen who will be willing to undertake the analysis professionally.

J. P., Shire.—Any wholesale chemist will supply you with bisulphite of soda. Be careful to get the bisulphite, not the bisulph*ate*.

E. Ellis.—It is not our province to give the information you ask. Many so-called depilatories are to be met with in commerce, but there is risk attending their employment.

F. C. S.—1 "Watts's Dictionary," under the heading Phenol (a synonym), gives a very good account of the preparation and purification of carbolic acid. As for its properties you cannot have a better account than that given in our own "Cattle Plague Report," published at the Chemical News Office. 2. Consult Bowditch's Analysis, etc., of Coal Gas, or Sugg's "Gas Manipulation."

C. Campbell.—To make permanganate of silver the two solutions must be quite saturated; the equivalents are easily calculated.

J. C. B.—The alteration is unimportant.

Tyro—The increased temperature of the soapy water is evidently due to the friction in the process of washing, and the warmth communicated to it by the hands.

H. K. Bamber, F.C.S.—Your communication has been carefully considered, and steps will be taken to diminish the abuse likely to arise.

F. J. R. C.—Received with thanks. We shall always be glad to hear from this correspondent.

J. H. Mann, Polytechnic Institute, Troy, New York.—Communication received. The offer came to us through an unimpeachable channel, but, owing to the sudden decease of the gentleman entrusted with the negotiation, the matter is in abeyance. We will therefore hold the communication at our correspondent's disposal, to be either published or returned to him, unless in the meantime we can forward it to the proper quarters.

Ignoramus.—Nothing whatever is known about the cause of the transparency of metallic particles in solution. Your theory is certainly wrong. Take our advice; avoid theories for the present. Hear what Sir Humphry Davy said:—"When I consider the variety of theories which may be formed on the slender foundation of one or two facts, I am convinced that it is the business of the true philosopher to avoid them altogether. It is more laborious to accumulate facts than to reason concerning them; but one good experiment is of more value than the ingenuity of a brain like Newton's."

Thomas MacFarlane.—We doubt if a minute description of your system would be admitted to competition if it is only a trifling modification of the process in ordinary use. We imagine that an entirely new process is wanted; there is, however, no harm in trying.

V. Cruse.—We regret we cannot give the information required.

D. J. O.—You can get the journals at Asher's, Bedford Street, Covent Garden, or Baillière's, Regent Street.

Rusticus.—1. There is no special memoir that we know of on the subject of nitro-glycerine as an explosive agent. 2. Larkin's magnesium powder lamp is the best.

F. J. Booth.—The campher weather-glass is only a toy. In your question about the density of the air you put cause for effect.

E. Smith.—The article is received with thanks.

J. Fordine.—An article on the subject will soon appear. We are waiting for the report of the scientific examiners of the foul atmosphere of the railway.

Ellen G.—Tea is not adulterated to the extent you suppose. Your suspicions in the present instance are quite unfounded, as the sample sent us is qurom colouring matter.

Communications have been received from D. Forbes, F.R.S.; W. Ainsworth (with enclosure); J. Spiller; C. R. C. Wright, B. Sc.; R. C. C. Lippincott; Dr. Parkes, F.R.S.; G. J. de Winton; W. Huggins, F.R.S.; A. Payne; S. Reeve; A. Pritchard (with parcel); H. Woodward; Dr. Adriani; E. Anderson; P. Jestram; J. Foreman; G. Griffith; F. Price (with enclosure); Dr. S. Smith; Edward Burn; Edward Beanes; W. Armstrong; J. Cubitt (with enclosure); Howards and Sons; S. Bew; Prof. Pepper (with enclosure); Page and Tibbs (with enclosure); R. Alison; J. J. Buchanan; C. Tennant (with enclosure); H. Catheldron; O. Jennings; J. Fordyce; Hugo Schmidt; E. Moyes; J. Hazlewood; E. Robinson; W. A. Townsend (with enclosure); Victor Cruse; Dr. Phipson; Rev. R. C. Douglas; Charles Tomlinson (with enclosure); M. A. Baines; George Hopwood; John Heywood (with enclosure); J. C. Wilson (with enclosure); Edwin Smith (with enclosure); G. A. Keyworth; J. C. Wilson; John Bray, F.C.S.; E. Ellis; W. Simmons; P. J. Worsley (with enclosure); W. Ladd; F. C. Calvert and Co.; J. Horsley (with enclosure); J. Harry Taylor; A. Scott; Watson Smith; D. J. O.; Edwin Smith (with enclosure); D. J. O.; Abbé Moigno; Thomas Anderson; A. Scott (with enclosure); W. Hartley (with enclosure); J. Heywood (with enclosure); F. C. Calvert, F.R.S.; F. Muspratt; Harry Taylor; — Warrington; J. Turner (with enclosure); Peter Squire; G. Worsley; Jabez Hughes (with enclosure); A. Brown (with enclosure); H. Gillman (with enclosure); Edward Beanes (with enclosure); F. J. Booth; J. Mercer (with enclosure); W. Herapath, F.C.S. (with enclosure); A. Gow (with enclosure); J. C. Braithwaite (with enclosure); "Rusticus" (with enclosure); F. Ireland (with enclosure); W. B. Giles (with enclosure); J. T. Bouck (with enclosure); B. W. Gibsone (with enclosure); John Cliff (with enclosure); W. L. Lindsay; M. A. Baines (with enclosure); A. Dalziell; W. Bywater; W. Ladd; C. A. Wright; W. J. Morgan (with enclosure); Nicholson and Maull; E. A. Parnell (with enclosure). Dr. Andersen; Charles A. Wright; J. M'Kenny and Co.; A. M'Leod; E. Ellis; J. Carter Bell; W. Skey (with two enclosures); H. K. Bamber; T. Macfarlane; W. Herapath, senr.; Dr. Day; F. J. Booth; H. Morton (with enclosure); W. Hooper (with enclosure); Peter Spence (with enclosure); M. Khanikof; C. Tomlinson, F.R.S. (with enclosure); Professor A. H. Church (with enclosure); T. Hill; J. Thorley; F. J. R. Carulla; W. Procter, junr.; T. G. Wormley, M.D. (with enclosure); W. J. Russell; G. F. Rodwell; J. Attfield; Rev. R. Harley, F.R.S.; A. E. Sansom, M.D.; J. Cliff; J. P. O'Brien; T. J. Barker (with enclosure); W. Lawrence; H. Bedford (with enclosure); R. Ward; C. J. Ellam (with enclosure; J. E. Wright; John Brown; Dr. Adriani (with enclosure); H. Armstrong (with enclosure); Henry Denny; J. Spiller; Lewis and Son; Robert Harley; C. L. Lee (with enclosure); P. W. Hofmann (with enclosure); E. Corbett, junr. (with enclosure); W. E. Bickerdike, F.C.S.; John Heywood; Peter Squire; E. Kienmann; John W. Burton (with enclosure)

Books Received.—"Analysis of a Biliary Concretion; and on a new method of Preparing Biliverdin." By Dr. Phipson, F.C.S. "The Relief of Pain by the use of Metallic Tractors." "Catalogue of the Library of the late Dr. Richardson." "A Dictionary of Chemistry." Part xii. "Intellectual Observer." "Experiments on the Removal of Organic and Inorganic Substances in Water." "Micro-chemistry of Poisons, including their Physiological, Pathological, and Legal Relations." By Theo. G. Wormley, M.D., with "Atlas" for the same, containing 78 illustrations upon steel. Baillière Brothers, New York. "Thorley's Illustrated Almanack."

Erratum.—In the Report of Mr. Stanford's paper, given in this number (Amer. Reprint for Nov., 1867, page 248), *for chloride of iodine* read *chloride of sodium*.

[English Edition, Vol. XVI., No. 404, page 114; No. 405, page 129; No. 407, page 158; No. 408, page 173.]

THE CHEMICAL NEWS.
Vol. I, No. 6. American Reprint.

ON SOME POINTS IN CHEMICAL GEOLOGY.

BY DAVID FORBES, F.R.S., ETC.

It must be admitted that the study of geological chemistry has more particularly of late years met with but little attention from British chemists; this apparent neglect, however, cannot be attributed to any want of appreciation of the importance of this branch of the science, but is rather due to the greater attractions possessed by the more novel and extensive field of exploration now opened up by the rapid strides of organic research, which appears to have all but absorbed the supply of labourers in the domain of chemical science.

The appearance in a late number of this periodical of a somewhat lengthy communication on chemical geology, was hailed by the author of these remarks with much pleasure, and as he has already long devoted himself to similar inquiries, and believes that a little discussion on the subject might prove useful in exciting afresh the interest of those familiar with this branch of the science, and might assist in arousing the study of chemical geology from its present semi-torpid state in England, does not consider further apology necessary for laying before the chemical public some observations upon the theoretical views in geological chemistry recently propounded by Dr. Sterry Hunt,[*] which are at considerable variance with those hitherto generally accepted by those chemists who have more specially studied this branch of the science.

In the *Geological Magazine* for this month the author has treated of the same subject more from a physical and geological point of view, and therefore will in the present communication as much as possible confine himself to a more purely chemical examination of the arguments and data brought forward by Dr. Hunt in support of his propositions.

In explaining the origin of this globe, Dr. Hunt adopts the nebulous hypothesis, imagining the whole of the chemical elements now constituting its mass, to have been originally present as "dissociated" gases, in a state of chemical "indifference" to one another, due to the intensely high temperature to which he supposes them to have originally been exposed. A lowering in temperature is then assumed to have brought about the chemical combinations of these elements, and the subsequent condensation of their compounds into the form of a sphere of igneous fluid matter surrounded by a dense gaseous atmosphere.

The old hypothesis of Davy assumed a similar result as being due to the heat eliminated by the combination of the elements themselves; but in Dr. Hunt's lecture he does not explain how he imagines the intense heat which originally caused the elements to become "dissociated and indifferent" to have arisen.

According to Dr. Hunt, this igneous sphere when cooling commenced to solidify at its centre, and extended outwards towards its exterior so as to produce a globe solid to the core, his words being, "The cooling in a mass like this would be just like the cooling of a great bath of metal or sulphur—*i. e.*, in other words, the condensation or congelation would commence at the centre and extend outwards towards the surface."[*] It is almost superfluous to observe that everyone knows that this is not the case with either sulphur or metals when cooling under ordinary circumstances; for chemists and others are accustomed in preparing crystals of sulphur or metals to allow such a melted bath to cool until its exterior has alone become solidified, and then, after making an orifice through the crust, to pour out the still fluid central mass, which consequently has not solidified first.

Dr. Hunt, however, explains that in the case of a cooling globe the central part would solidify first, owing to its melting point being much more elevated by the pressure to which it was subjected, and appeals to experiments of the late Mr. Hopkins[†] as conclusive proof that the melting points of bodies do become (*ad infinitum*) elevated in proportion to the applied pressure.

Without disputing that the fusing points of many bodies may be elevated by pressure, a reference to the experiments appealed to by Dr. Hunt will show that (in the present instance at least) they are not at all conclusive. Setting aside the experiments made on spermaceti, stearine, and wax, which being organic substances decomposed at comparatively low temperatures, and possess no analogy whatever to the mineral compounds here under consideration, the other experiments of Mr. Hopkins were made on sulphur and metallic alloys, and consequently their results should be particularly applicable to the case of a "bath of metal or sulphur," alluded to as a simile by Dr. Hunt. On reference to the report in question, however, it will be at once perceived that the result of these experiments cannot at all warrant deductions so conclusive as Dr. Hunt has drawn from them; for Mr. Hopkins expressly states that in the case of the metallic alloys experimented upon, "*he has not detected any elevation of fusing temperature acquired by increasing the pressure.*" Again, the experiments made with sulphur afforded the following results:—

Pressure in atmospheres.	Pressure in lb. per square inch.	Fusing point in Centigrade.
1	15	107·2
520	7,790	135·2
793	11,990	140·5

In other words expressed, these figures show that the melting-point of sulphur under a pressure varying from 1 to 520 atmospheres, becomes elevated in temperature at the rate of 0·394° Centigrade per atmosphere of applied pressure, but that subsequently up to 793 atmospheres, the highest pressure employed in these experiments, this rate diminished greatly, becoming little more than one quarter of the previous rate of increase, or only 0·0101° Centigrade for each atmosphere. It may not unreasonably be supposed, therefore, that greater pressure would still further lower this ratio, and eventually reduce its fusing-point once more to the temperature at which it melts when not subjected to any pressure, and it may even be imagined that subsequently, as in the case of water, a still higher pressure might actually cause depression instead of elevation of its fusing-points.

It is admitted now that there is a limit to the

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[*] Dr. Hunt's *resumé* of his lecture, "On the Chemistry of the Primeval Earth," Chemical News, vol. xv. pp. 315-317 *Eng. Ed.* (*Amer. Reprint, Aug.* 1867, p. 32:) short-hand verbatim report of same in *Geological Magazine*, vol. iv. pp. 357-369, containing more details than the above abstract; also Dr. Hunt's communications to the *Academie des Sciences*, 22nd April, 1867, noticed in the Chemical News, vol. xvi. p. 148, *Eng. Ed.*—(*Amer. Reprint, Nov.* 1867, p. 276.)

[*] *Geological Magazine*, vol. iv. p. 361.

[†] British Association Report, 1854, page 57.

increase in density of bodies when subjected to pressure, or rather that after a certain point is reached that the increase of density of a body bears a less and less ratio to the actual compressing force employed; and it may be imagined that the same result would be also found to take place in the relations of the increase in temperature of the fusing-points of bodies to the pressure applied. It seems not improbable even that substances when once brought to the condition of maximum density might not then have the temperature of their fusing-points further elevated by increase of pressure.

Whether this be or be not the case, however, Dr. Hunt's argument would not be valid unless he at the same time brought forward proof that the substance of the earth is homogeneous throughout, or made up of substances possessing the same or nearly the same fusing-points as that of the original external crust or layer. Now, there seems no ground for believing that such can be the case; for since the mean specific gravity of the earth is about double that of the substances composing its known exterior crust, it would appear all but certain that the interior mass must be composed of substances different in composition, and much more dense than those known to form the superficial parts of the globe;[*] and this would indicate the great probability of there being in the interior of the earth an immense accumulation of metallic bodies of great density; and as the fusing points of such substances are acknowledged to be immensely lower than that of those composing the known crust of the earth, it might be advanced in opposition to Dr. Hunt's views that this difference would more than counterbalance the tendency to solidify at the *centre* in the case of the fusing points being really even considerably elevated by the effects of pressure.

For these and many other reasons, some of which will be afterwards noticed in the course of this discussion, the author cannot agree with Dr. Hunt that the earth is solid to the core, but believes that there is still some vast reservoir or reservoirs of molten matter in its interior.

In entering into the consideration of the chemical history of the earth from the moment of solidification Dr. Hunt now bases the whole of his views of the reactions and the entire exposition of the chemical changes which took place in this newly-created globe upon the constitution of the atmosphere with which it then was surrounded; it consequently becomes of the highest importance to ascertain by careful scrutiny as to whether his views upon this subject are sound and likely to meet with acceptance in the chemical world.

This atmosphere according to Dr. Hunt was intensely acid and of great density, and contained all the carbon, sulphur, and chlorine in combination respectively, as carbonic, sulphurous and hydrochloric acids along with the nitrogen, steam, and "a probable excess of oxygen."

That the nitrogen, steam, and carbonic acid would be there is really admitted, but it may fairly be inquired whether any chemist can believe, that after the grand scene of general combination of the elements occurring under the circumstances assumed by Dr. Hunt, that (even if possible?) it could be at all *probable* that *an excess of oxygen* could exist along with the vast amount of sulphurous acid which was present, if that gentleman's premises were correct. Further, the improbability of such an atmosphere containing a mixture of heated hydrochloric and sulphurous acid gases, may be inferred from Dumas' researches;[*] that chemist having long ago shown that these gases when mixed together, react and mutually decompose one another with the formation of water, chlorine, and sulphur.

The strong affinity which sulphur has for the metals is well known, as also the fact that sulphurous acid is itself readily decomposed by many metals, with the formation of metallic sulphides and oxides; and the inference which the author would deduct therefrom is that so far from all the sulphur having at this crisis of chemical combination gone into the atmosphere as sulphurous acid, it in reality united itself to the metals, and thus formed dense sulphides which at once sunk through the external and lighter fluid layer of the still liquid igneous sphere, and there remained in its interior protected from oxidising action.

The existence in Dr. Hunt's imaginary atmosphere of all the chlorine in the form of hydrochloric acid is also protested against. Independently of the probability of in such event the oxygen and hydrochloric acid gas reacting upon one another, and reproducing chlorine with the vapour of water; it is contended, that hydrochloric acid was not even likely to have been formed at all under the circumstances here alluded to. Amongst the most stable, if not the most stable of all the compounds of chlorine are the alkaline chlorides—for example, chloride of sodium or salt, and it may be gathered from Dr. Hunt's lecture that he cannot but admit that there must have been abundance of metallic sodium present at the moment of the general combination of the chemical elements. Chemists therefore will require of Dr. Hunt an explanation as to why he in such a case, supposes the chlorine to have united itself with the hydrogen to form hydrochloric acid, instead of at once combining with the equally accessible sodium, for which chlorine is known to possess a stronger affinity than for hydrogen; chlorine unites even at ordinary temperatures and without compulsion directly with sodium, and still more energetically at more elevated temperatures; and that its affinity for sodium is far stronger than for hydrogen is shown by the fact that hydrogen does not decompose chloride of sodium even with the assistance of heat, whilst, on the contrary, the chloride of hydrogen or hydrochloric acid is decomposed and yields up its chlorine to the sodium and various other metals, even in the cold.

When summing up the arguments advanced on both sides, as to the constitution of the atmosphere which enveloped the earth at this early period of its existence, the writer confidently believes that chemists will agree with him in disputing the probability and even possibility of such a chlorhydric and sulphurous atmosphere as Dr. Hunt has attempted to realize, and thinks with him that the main differences between the air of that period and the present age would be in the large quantity of carbonic acid and water, along with probably a much less amount of oxygen.

To elucidate the chemical reactions which character-

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[*] Knowing that there is a limit beyond which substances increase but little in density when subjected to additional pressure, it may be fairly assumed that the materials of the known external crust would have attained their maximum density long before the conditions here required would have been arrived at; a simple calculation will show that if we regard the mean density of the earth as 5·3, and that of the surface crust as one half this, or 2·65, and further imagine the earth to be composed of three concentric layers of equal thickness, and of densities increasing respectively in arithmetical progression, there would be respectively an outer crust of specific gravity, 2·65, an intermediate zone of specific gravity 10·7, and a central kernel specific gravity 18·8. If instead of three such zones, more than this number are imagined, then the calculation will show that the specific gravity of the central kernel will come out still higher than 18·8.

[*] *Traite de Chimie*, t. i. p. 146.

ised this stage of the earth's history, Dr. Hunt states that they were "just what would now result if the solid land, sea and air were made to react upon each other under the influence of intense heat"; it is well known that temperature may greatly modify chemical action, but as Dr. Hunt's ultra-igneous theory[*] deals with the effects of heat so immensely intense as to "dissociate" and vaporize even the most refractory bodies, it must be admitted that his comparison is quite correct, but at the same time it is not admitted that the results of such reactions would be such as Dr. Hunt represents them to be when he states, "to the chemist it is at once evident that from this would result the conversion of all carbonates, chlorides, and sulphates into silicates, and the separation of the carbon, chlorine, and sulphur in the form of acid gases"; on the contrary the author believes that the chemist who knew anything about geology would remember the vast stores of carbonaceous matter locked up in the earth's bowels, and deduce therefrom that the carbon of these would react upon the sulphates, converting them into sulphides without their evolving all the sulphur they contain in the form of acid gas, as Dr. Hunt would have us to believe—nor would he admit that all the chlorides were converted into silicates, or that as Dr. Hunt elsewhere[†] tells us that the chlorine of the sea-salt would be expelled into the atmosphere in the form of hydrochloric acid gas; for although he would be fully aware that silica, sea-salt, and water, if exposed to heat under forced circumstances, as for example when heated in confinement or when the vapour of water and salt be passed over highly heated silica, that in such case silicate of soda would be formed and hydrochloric acid evolved; this, however, would not be the case in nature in the event alluded to by Dr. Hunt, for the water in the sea would be all evaporated at the first approach of the heat, leaving the anhydrous salt, which, being also volatile, would be next sublimed as soon as the heat became more intense; the unaltered quartz remaining behind, before it had even attained a temperature sufficient to have effected the supposed reaction.

The sea, which would cover the earth's surface as soon as this had cooled down sufficiently to allow of the condensation of the vast accumulation of aqueous vapour in the atmosphere, would, the author believes, become salt the moment it appeared upon the surface of the globe, since the water would at once dissolve the chlorides formed by the direct combination of the metals with chlorine, as previously alluded to, and thus produce a solution of the chlorides of sodium, potassium, calcium, magnesium, etc., along with iodides and bromides which owed their origin to similar reactions. As Dr. Hunt does not attempt to explain why the chloride of sodium should so preponderate over that of potassium as the other alkalies, this question may be reserved for future research and speculation.

Dr. Hunt however accounts for the saltness of the sea by an explanation totally different from the above, and after stating that "the depressed portions of half cooled crusts would be flooded with a highly heated solution of hydrochloric acid," proceeds to inform his audience that this acid deluge would extract the soda along with some other bases from the silicates of the crust, and thus form the salt sea.

Should chemists, however, adopt the author's opinion that the chlorine really had at once united with the sodium to form salt and other chlorides, then it naturally follows that Dr. Hunt's views of this stage in the earth's history are untenable.

For the sake of argument, however, let it be supposed for a moment that Dr. Hunt is correct in insisting upon that all the chlorine and sulphur had ascended into the atmosphere as acid vapours; then it must be asked, What became of the sulphur? As Dr. Hunt did not inform his audience in his lecture, we must inquire ourselves. The sulphurous acid would naturally convert itself sooner or later into sulphuric acid, and would be condensed and fall down on to the globe, and be carried into the sea.

As now, sulphuric acid is more powerful than hydrochloric acid, it would at once turn out the hydrochloric acid, and convert the chlorides into sulphates, so that, instead of the ocean formed by Dr. Hunt's theory being a salt sea in the ordinary acceptation of this term, it would really be a solution of glauber-salt or sulphate of soda.

There are many reasons for estimating the probable quantity of sulphur contained in the globe as fully as large, if not larger, than that of chlorine; but as the equivalent of sulphur is only 16, whilst that of chlorine is 35·5, it would not require as much sulphur as even one-half the amount of the chlorine present in the sea to convert the entire amount of salt contained in the ocean into sulphate of soda.

Dr. Hunt next makes the surprising assertion that all true limestones are the result of the precipitation of carbonate of lime thrown down from a solution of the chloride of calcium by the action of solutions of carbonate of soda. As Dr. Hunt does not in his lecture advance any evidence whatsoever in support of this statement, it is considered to be purely hypothetical; and it is believed that chemists will still adhere to the opinion that limestones have not been so found, but that they are essentially the result of organic action, as has been very satisfactorily demonstrated by the careful study already made of these rocks by geologists, palæontologists, and microscopists.

It is not probable that either chemists or zoologists will agree with Dr. Hunt's further assertion that "animals can only appropriate the carbonate of lime which they find ready formed," but that they will consider these animals capable of utilising the other lime salts in the sea until, at least, Dr. Hunt brings forth convincing evidence to the contrary.

Sorby's admirable microscopical investigations have clearly demonstrated that the magnesian limestones and dolomites in reality only represent ordinary limestone beds, altered *in situ* by the infiltration of magnesian solutions. Dr. Hunt, on the contrary, claims to have discovered that magnesian limestones, dolomites, and gypseous beds have originated through chemical "reactions hitherto unsuspected," and that his experimental researches have proved them to have been formed at a time when the surface of the earth was covered by a dense atmosphere of carbonic acid. In reply to Dr. Hunt, the author would, in plain words, declare his firm belief that geologists, palæontologists, or zoologists will be as little disposed to consider his conclusions even likely to be true, as chemists on the other hand will admit his reactions and experiments to be new.

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[*] The idea of igneous action propounded by Hutton and his followers the Plutonists, is but a milk-warm theory when compared to that of Dr. Hunt, who, whilst protesting against the earth having been formed "entirely by fire," discourses eloquently on its creation from a state of ultra incandescence at temperature so elevated as would have been far beyond the conception of Hutton himself.

[†] *Geological Magazine*, vol. iv. p. 362.

The former will content themselves with informing Dr. Hunt that every geologist should be aware that the great development of such beds took place at an epoch in the world's history, when air-breathing animals (both vertebrate and invertebrate) lived upon the face of the earth, and with expressing their surprise that Dr. Hunt could imagine these animals living in an atmosphere of carbonic acid; whilst chemists, on their part, would not be disposed to regard the mutual reactions of the sulphate or chloride of magnesium with carbonate of lime, as possessing novelty, and would further inform Dr. Hunt that, notwithstanding he has considered the results of his experiments on magnesian compounds under an artificial atmosphere of carbonic acid as worthy of being laid before the *Academie des Sciences* of Paris,[*] for nearly, if not more than a quarter of a century these very processes have been in general application on the large scale in the manufactories of preparations of magnesia both in England and Ireland.

In concluding these remarks, the author can only but record his protest against the soundness of the arguments propounded by Dr. Hunt in his explanation of the origin of granite and the formation of the metamorphic and eruptive rocks, both ancient and modern, as in this present communication the space at disposal will not allow of more extended discussion. On some future occasion, however, an attempt will be made to take the chemistry of the formation and alterations undergone by these rock-masses also into consideration.

ON THE COMMERCIAL ANALYSIS OF SOME OF THE PRODUCTS AND MATERIALS OF THE ALKALI MANUFACTURE, Etc.

BY C. R. A. WRIGHT, B.SC., F.C.S.

(Continued from page 228, Amer. Reprint, Nov. 1867.)

Where the hypochlorite contained in a sample of bleaching powder, which may also contain chlorate, is to be determined, the only safe and convenient method is that of Penot, *i.e.*, by the use of an alkaline solution of As_2O_3. When the chlorate likewise is to be determined, it may be expeditiously done by heating the sample with a known quantity of the same arsenite solution, and addition of HCl; from the difference between the quantities of arsenite peroxidised in the two instances the chlorate is readily known. The writer has found bleaching powder of commerce to contain several per cents. of calcium chlorate, even when newly made; in older samples the chlorate has been occasionally found to represent as much as 10 per cent. of available chlorine, or fully one-fourth of the amount originally present; thus indicating overheating either in the process of manufacture or subsequently.

(V.)—**Manganese Ore.**—The mode of analysis of manganese ores usually adopted is that of Fresenius and Will, viz., estimation of the CO_2 evolved by acting on an oxalate in presence of SO_4H_2. Although capable of yielding the most accurate results, this process is usually misapplied in such a way as to indicate 2, 3, and more per cents. of "available binoxide" over and above that really present. In order to shorten the time requisite for analysis the apparatus, weighed cold previously to the expulsion of CO_2, is usually weighed whilst quite hot the instant the reaction is complete; thus errors of varying amount are introduced, the apparatus always appearing to weigh less while hot than when cold on account of the effect of the ascending current of warm air buoying up the scale-pan, etc.; the writer has found differences of from 1 to 3 per cent. between the results obtained by weighing the apparatus while quite hot, and those got by allowing it to cool completely before weighing. Again, many kinds of manganese ore contain perceptible quantities of carbonate in the gangue, and frequently the commercial analyst does not take the trouble to estimate and subtract the CO_2 evolved from this source. Through haste, also, the CO_2 may be liberated too rapidly to get perfectly dried before escaping from the apparatus. Lastly, the CO_2 evolved is considered to represent the available MnO_2 present, whereas it represents $\frac{88}{87}$ of that amount, 55 being the generally admitted equivalent of manganese. From one or all of these reasons it is by no means unfrequent to find the percentage reported by a commercial analyst 4 or 5 per cent. above what is really present; a matter of considerable importance to the purchaser who pays according to the certificate of analysis. Practically speaking, therefore, volumetric methods requiring less time or attention on the part of the analyst, are more likely to give correct results in cases where accuracy must be sacrificed to speed, which is too often the case when low fees are demanded for analytical work.

In order to compare the results obtainable by the better known processes for the valuation of manganese ores, the following methods were tried with the same sample of uniformly mixed finely powdered ore.

(1.) Fresenius and Wills' process: oxidation of oxalate and estimation of CO_2 produced by loss of weight.

(2.) Bunsen's process: distillation with strong hydrochloric acid and reception of chlorine evolved in potassium iodide solution, the liberated iodine being determined by hyposulphite of soda and standard iodine solutions.

(3.) Mohr's process: distillation with hydrochloric acid, reception of chlorine evolved in an alkaline arsenite solution of known strength, and estimation of unoxidised arsenite by standard iodine solution.

(4). Price's process: boiling the ore with hydrochloric acid and a known amount of As_2O_3, in a flask to which a bulb tube is attached to prevent the loss of $AsCl_3$, estimation of unoxidised As_2O_3 by a solution of permanganate.

(5.) Price's process modified: SO_4H_2 used instead of HCl, and accordingly an ordinary flask being used instead of the flask and bulb apparatus.

(6.) Otto's process: boiling with a known amount of a ferrous salt, the excess of iron being determined by a standard solution of potassium di-chromate, or permanganate.

These methods gave the following results:—

Name of method.	Percentage of available binoxide found.	Difference from mean.
(1.) Fresenius and Wills'	65·49	+ 0·04
" 2nd experiment	65·43	− 0·02
(2.) Bunsen's	65·41	− 0·04
" 2nd experiment	65·63	+ 0·18
(3.) Mohr's	65·46	+ 0·01
(4.) Price's	65·49	+ 0·04
(5.) " modified	65·60	+ 0·15
" 2nd experiment	65·35	− 0·10
(6.) Otto's	65·30	− 0·15
" 2nd experiment	65·36	− 0·09
Mean result	65·45	

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[*] Comptes Rendus, April 22, 1867, lxiv., p. 815.

In no case is there so great an error as ± 0·2 from the mean result. In point of speed Fresenius and Wills' is very good, but, as usually employed, is open to the objections previously stated; and when any carbonate is contained in the ore, requires a double estimation. Bunsen's and Mohr's are both speedy, but are troublesome in a commercial laboratory, and require accurate weighings on account of the small amount of substance taken. The latter objection also applies to Otto's process. Price's process and its modification are both open to the objection that permanganate does not act absolutely uniformly on As_2O_3, and that a reddish manganic salt is produced by the reaction; for technical purposes, however, this error is rendered negligible by not taking a very large excess of As_2O_3, and standardising the permanganate by an arsenious solution of known strength. In performing the process with SO_4H_2, the weighed As_2O_3 should be placed in a flask, and boiled with sufficient pure sulphuric acid diluted with twice its bulk of water to dissolve it; the weighed manganese ore is then dropped in, and the whole boiled until no black specks of MnO_2 are visible. For every gramme of manganese ore of 70 per cent. available peroxide 0·85 grammes of As_2O_3 is sufficient, leaving thus only a small portion of unoxidised As_2O_3 to be determined by the permanganate, which should be standardised by a solution of a known weight of As_2O_3 in sulphuric acid. If any considerable excess of As_2O_3 have been used it will be more convenient to dilute the acid fluid obtained after boiling with the manganese ore to a known volume—say 300 c.c.—and filter off an aliquot portion for titration by permanganate.

Occasionally, manganese ores contain admixtures of magnetic oxide of iron, ferrous carbonate, or other iron compounds not fully oxidised to the ferric state. Accordingly, when treated with hydrochloric acid, the chlorine given off will be a measure, not of the total MnO_2 present, but of that MnO_2 over and above what is requisite to peroxidise the ferrous compounds. In order to see how the presence of ferrous compounds affects Fresenius and Wills' process, known weights of pure $FeSO_4 + (NH_4)_2SO_4 + 6H_2O$ were treated along with weighed portions of the manganese ore previously experimented on in the CO_2 apparatus with the following results:—

	A.	B.	C.
Percentage of MnO_2 corresponding to the CO_2 evolved	62·61	61·38	45·46
" " Ferrous salt used	2·71	4·14	20·58
Total	65·33	65·52	66·40

It therefore appears that even when a considerable amount of ferrous compound is present the CO_2 evolved corresponds, as in the processes depending on the evolution of chlorine, not to the whole MnO_2 present, but to that left after the ferrous compound is oxidised. In the case of Price and Otto's processes the same will evidently be the case.

ON METEORS.*

BY PROFESSOR ALEXANDER HERSCHEL.

A QUESTION which at present agitates the minds of physical astronomers is, to ascertain whether a slight acceleration of the moon's apparent motion can be attributed to a lurking error in the calculations of its place, or whether the earth, in the course of ages, has lost a small portion of its speed of revolution round its axis. The latter alternative would appear to explain the fact that the lunar tables, which exactly represent the moon's apparent motion at the present time, do not absolutely give the hour of the day of an eclipse which happened when the sun was setting at Babylon some hundred years before the Christian era. The eclipse began, according to the tables, when the sun was already below the horizon, and it would be invisible at Babylon. But if the earth's rotation, instead of being uniform, were a little more rapid in former times than it is at present, the sun, instead of being set below the horizon of Babylon, would appear eclipsed above it, as the phenomenon was in reality observed. To account for a slower rotation of the earth about its axis at the present time than that which it possessed formerly, the friction of the tides has been supposed to play an important part in checking its velocity. A slow accumulation of meteorites upon the earth's surface, although not appreciably altering the figure and dimensions of the globe, must yet, in the course of many ages, produce an average effect of diminishing its velocity of revolution. The change of a hundredth part of a second in the length of the day, since the time of the earliest observations, would explain the small error which astronomers have discovered, and the cause of which still eludes their search.

Damages to life and property by the fall of meteorites are, from the generally small size of aërolites, among the rarest catastrophes on record. Yet a Franciscan monk was struck and killed by an aërolite, at Padua, in the year 1660, and the Italian philosopher Zerzago, wonderfully concerned at the event, inquired if the stone could not have been projected from a volcano on the moon. At a later period of discovery with regard to meteorites, this conjecture received considerable support, but it was finally rejected as insufficient when it was found that aërolites move with velocities much greater than that of satellites of the earth.

In the year A.D. 1719, a meteor of unusual size appeared in England, to which trigonometrical calculation assigned a diameter of at least a mile, a velocity of three miles per second, and a height in the atmosphere of sixty geographical miles. A detonation like thunder shook the houses as it passed. Dr. Edmund Halley, who was then Professor of Astronomy at Oxford, described the appearance of this meteor. He held the opinion of Aristotle that the meteor was caused by the kindling of a tract of inflammable gas, collected in a long train at the top of the atmosphere, and there exploding. Aristotle's opinion cannot be entertained, on account of the rarity of the atmosphere at great heights being insufficient to support the vivid illumination of large meteors by simple inflammation and combustion of a gaseous mixture. It was, according to the opinion of Dr. Wallis, who described an equally large meteor that passed over England at twilight on the 20th September, 1676, thought to be more probable that the fire-ball was a near view of a comet which was seen near the sun about a fortnight later. It deserves to be mentioned that Dr. Wallis occupied the same chair in the University of Oxford in which Dr. Baden Powell, who alone instituted and began the present series of reports of the British Association on luminous meteors, afterwards proved his illustrious successor, and that the views which Dr. Wallis first introduced on the subject of observations of luminous meteors to English readers have been singularly verified in the

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* From a lecture delivered before the British Association, at Dundee.

events of the past year. In the years 1758 and 1783 Dr. Pringle and Dr. Blagden, at that time the Secretaries of the Royal Society, described two of the largest meteors that appeared in the last century in England. Their calculated height in the atmosphere was about 50 miles, and they were accompanied, like that described by Halley, by very loud explosions. The discovery of atmospheric electricity had hardly been made, when these two writers attributed the appearance of large meteors to the same cause as that which gives rise to lightning in the lower regions of the atmosphere. The character of the discharge of electricity in exceedingly rare gases was, however, beginning at the time to be studied, and Lichtenberg's experiments at Gottingen convinced Chladni at Wittemberg that the real explanation of fire-balls had not yet been discovered. In the Mineralogical Museum of St. Petersburg a large mass of metallic iron, weighing about seven hundred weights, had been brought by Pallas, the geologist and explorer, from the summit of the hill of Krasnojarsk, in Siberia, where it was found. The origin of the mass was a vexed question with geologists when, in the year 1794, Chladni published his work on "The Iron Mass of Pallas, and on Other Masses of Iron and Stone Reputed to have Fallen from the Air." In this work Chladni supposes that all the accounts hitherto received of the falls of aërolites were correct, and he presents a catalogue of them, together with all the accounts of large fire-balls which he was able to collect. Chladni conceived that a class of cosmical bodies exists in all parts of the solar system, each forming by itself a peculiar concourse of atoms, and that the earth from time to time encounters them, moving with a velocity as great as its own, and doubtless in orbits of very various eccentricity round the sun.

Chladni further assumed that a certain property of compressed air, which can be readily exhibited by an instrument called a match-syringe, produces the vivid light and heat of combustion which these bodies exhibit when they are first brought into collision with the outer strata of the atmosphere. When air is confined by a piston in a tube, and the piston, carrying a piece of tinder or other light substance at the end, is suddenly forced into the tube, the heat developed by the compression of the air is so great as to ignite the tinder. The passage of a celestial body through the atmosphere must be intensely rapid, so that before the air can make its escape from the front of such a projectile, it must necessarily undergo a violent compression of the kind exemplified in the match-syringe—the heat developed on its surface must, doubtless, far surpass what can be produced by mechanical means. A series of accurate experiments was made by Dr. Joule, from which it may safely be concluded that a velocity of transit through the air, which is not uncommonly observed in meteors, of thirty miles in a second, would produce upon the surface of the meteoric body a heat sufficient to fuse, and probably also to volatilise, the most refractory substances. Not only the thin glaced surface or crust with which aërolites are invariably covered, but also the appearance of fire-balls and shooting-stars can be satisfactorily explained on these assumptions. Astronomical observations are only required to determine what is the real course of the meteoric particles in space; what is their law of distribution; what is the class of orbits which they pursue; and finally, what is their history, either as independent bodies or as emissaries from the train of some other well-known bodies of the visible universe. The first astronomical observations of the kind necessary to confirm the theory of Chladni were those conducted by Brandes and Berzenberg, at Gottingen, in the year 1798, on the heights and velocities of shooting-stars. It was found that shooting-stars appear at a surprising height in the atmosphere, and move with the extravagant velocity which large aërolitic fire-balls were already known to have. The first indication was thus gained that shooting-stars are, in fact, pigmy aërolites, and that aërolites are a gigantic kind of shooting-stars. Observations of luminous meteors have now divided themselves into three classes, for each of which a separate investigation leads to the uniform result that the hypothesis of Chladni is the only one which bears upon its face the stamp of truth. In the principal division of the subject (to which Professor Maskelyne has given the name of aërolites), it was shown by Edward Howard, in the beginning of the present century, that meteoric stones differ essentially from terrestrial rocks, by abounding with metallic iron. But they agree among themselves, by having, in every case which he examined, the rarer metal nickel for an ingredient. Chromium was afterwards shown by Laugier to be an even more constant companion of iron in meteorites than nickel. Copper, tin, and lead, soluble chorides of sodium and potassium, carbon, in the form of graphite, and once occurring as a carbonaceous peat-like mass, and in one other case as a volatile substance—have been found in meteorites; but no new element has been discovered which is not already known to exist upon the earth. Quite recently, the Master of the Royal Mint, Prof. Graham, has found an abundance of hydrogen gas occluded, or stored up, in the mass of a meteoric iron. The similarity of composition of all the members of the solar system receives from these discoveries an argument of credibility quite as strong, and, indeed, much stronger than that which can be drawn from an examination of the sun's light in the spectroscope; because, in the case of meteorites, bodies evidently belonging to the celestial spaces can be handled, and their materials have been freely analyzed. Among the largest aërolite falls of modern times, two celebrated examples have occurred in France, and two took place in Austria and Hungary. A violent explosion was heard at L'Aigle, in Normandy, and at a distance of eighty miles round L'Aigle at one o'clock in the afternoon of the 26th of April, 1803, a few minutes before the explosion was heard, a luminous meteor with a very rapid motion appeared in the air, and the explosion heard at L'Aigle was caused by the bursting of the meteor. Two thousand stones fell at L'Aigle, upon trees, pavements, and the roofs of houses, so hot as to burn the hands when touched, and one person was wounded by a stone upon the arm. The shower extended over an oval area nine miles long and six miles wide, close to one extremity of which the largest of the stones was found; but the only description I have seen at all approaching graphic was, that some thought their chimneys were on fire, and rushed out for a pail of water. A very similar shower of stones fell at Stannem, between Vienna and Prague, on the 22nd of May, 1812, when 200 stones fell upon an oval area eight miles long by four miles wide. The largest stones, in this case, were found, as before, near the northern extremity of the ellipse. The third stone-fall occurred at Orgueil, in the south of France, on the evening of the 14th of May, 1864. The area in which the stones were scattered was eighteen miles long by five miles wide, and the largest stone was picked up at the eastern extremity of the area. Lastly, at Kuyahinza, in Hungary, on the 9th of June last year, an aërolite, weighing six hundred-weights, was deposited, with nearly one thousand lesser stones, on an area measuring ten miles in length by four miles wide. The large mass was found, as in the other cases, at one extremity of the oval area, and a luminous meteor, followed by a loud explosion, accompanied the stone-fall, which left a smoky streak, visible in the sky for nearly half-an-hour. A considerable aërolite fell upon the same date in Algiers, at Tadjera, in the present year, and two days later, on the 11th of June, a fire-ball, leaving a streak, visible at least one hour, was seen in full daylight, at sunset, in the north of France, in Switzerland, and in Belgium; and those who were in Paris at the Exposition had doubtless seen many accounts of it in the papers at that time. Although it was accompanied by a detonation, it discharged no stones, but the coincidence of two stone-falls happening in two successive years on the 9th of June makes it probable that this large fire-ball belonged to the same aërolite date. The largest meteors are obviously divided into two classes, one of which, the bolides, or silent fire-balls, appear to have a looser texture, or to consist of more easily inflammable substance than the rest. They burn very brightly, but without producing an audible concussion of the air. Several true bolides accompanied the last November star-shower. Aërolitic fire-balls, as their name implies, frequently precipitate solid stones upon the ground. Fire-balls of this class are accompanied by a detonation. Four such fire-balls have happened within the last few years, on or about the 20th of November. The list of fire-balls observed hitherto numbers some thousands, and as far as their appearance in comparison with certain shooting-stars is concerned, the latter present a dwarfed resemblance to the former, so that it is probable that no break exists, but that fire-balls of every kind are shooting-stars of a larger stature.

The progress of knowledge regarding shooting-stars may also be identified with the history of the November star-shower. That great apparition which took Humboldt and Olmsted by surprise in 1799 and 1833, has met the gaze of thousands unable or unwilling to speculate upon its nature. Yet how aptly an Arabian chronicler of the last display describes the host of meteoric atoms invading the earth's atmosphere, as "the mighty armies of the sky joined in a fierce strife." He adds that the earth's atmosphere proved a perfect safeguard to ward off the skirmishers from the sphere of human habitations, for "the fire and sparks (he writes) were harmless, not touching the earth, or injuring our safety, as if night's daring horsemen, who continued till morning beating each other in single combat, gave us protection and peace." Thousands, again, who never saw the display of Humboldt, nor the much greater spectacle of Olmsted, have thought for themselves to penetrate its meaning. Humboldt, in his description of the star-shower at Cumana, states that the oldest inhabitants at Cumana remembered that a similar phenomenon preceded the great earthquakes of 1766. But no suspicion of its periodicity could then have crossed his mind for want of a statement of the month and day. On the 13th of November, 1832, and again on the 13th of November, 1833, the shower reappeared, at first in Europe, and the second time in full magnificence in America. No doubt of its periodical character could, after that time, exist. A point of capital importance was also discovered on that occasion, which distinguished the great November star-shower from all other exhibitions of meteors that had been previously observed. Instead of clashing together, as too many old accounts of their appearance might, perhaps, lead us to imagine, the November meteors, in 1833, shot outwards in smoothly-flowing lines from a single centre of emanation in some part of the constellation Leo. Olmsted himself describes the radiant point of the star-shower as the vanishing point of nearly parallel straight lines seen in perspective. The position of the radiant point in Leo was by no means unanimously fixed by different observers. Olmsted thought it near the star γ Leonis, but Professor Twining placed it in the centre of Leo's sickle, close to the small star χ Leonis—the identical spot where observers agreed in placing it during the November star-shower in 1866. The fixity of this point among the stars was, in the opinion of Professor Twining, sufficiently distinct to enable him to recognise that at this juncture the earth passed through a vastly extended system of meteoric bodies entirely independent of every terrestrial agency, and yet moving in entire harmony and concert,—in short, that each November meteor had an orbit, and that in their orbits they were all revolving together round the sun. Humboldt described them a little later as pocket planets, and as such they continue to be considered until their expected return, after a lapse of about thirty-three years, should enable observers, with better means at their command, and with full preparation for their reappearance, to come to a more exact conclusion.

It was soon after this that Mr. Quetelet, of Brussels, formed a catalogue of all the ancient records of star-showers that he was able to collect, in order to discover in them any signs of a periodical character that might exist. He succeeded in predicting the return of the meteors of St. Lawrence on the 10th of August, 1837, which have ever since been the most constantly observed of star-showers. The radiant point of this shower is not far from the sword-handle of Perseus. The result showed that other periodical star-showers might probably be looked for with success, and one which took place at Richmond, U.S., on the 20th of April, 1803, was watched for by Hewick in America, and was found to resemble that which from time to time appears on the 2nd of January, by great uncertainty in its returns. Its radiant point he found to be near Vega Lyræ. The other date when meteors are supposed to be most plentiful is the 2nd of January, when the meteors have a radiant point near the right knee in the figure of the constellation Hercules. A moderate shower of meteors is seen every year on the night of the 12th of December, radiating from the neighbourhood of Castor and Pollux. Finally, on or about the 19th of October, a tolerably well-marked shower of meteors has been seen during the last two years radiating in a very definite manner from Orion.

It is an interesting discovery in the familiar phenomenon of shooting-stars, perhaps too long neglected, as Mr. Quetelet remarks, by astronomers, that if their number seen on any night, by one person, much exceeds fifteen per hour, the appearance generally indicates a special shower; and a very moderate amount of attention to their apparent tracks among the stars in general suffices to determine the fixed centre of radiation from which they diverge. A small degree of diligence and perseverance may thus often be profitably bestowed in rendering with very little trouble an essential service to astronomy, which would doubtless be more heeded if the beautifully striking property of shower-meteors to radiate from a fixed point among the constellations were more generally known. From the records of scattered observations, extending over more than twenty years, the Luminous Meteor Committee of the British Association believe that they have traced the existence of at least fifty periods of such occurrences during the twelve months of the year, with the positions of their connected radiant points. But the exact date of maximum of many of the showers is still undetermined, and this is what an opportunity might frequently present itself to observers of shower-meteors to supply.

A study of ancient appearances of the November meteors led Professor Newton, of Yale College, U.S.A., to anticipate their reappearance on the morning of the 14th of November last. The interest of astronomers was awakened by the seasonable appeal in good time for preparations to be made in almost every quarter of the globe to note the reappearance of the shower. The area of its visibility extended from the British Isles to India in the east: and from Europe in the northern to the Cape of Good Hope in the southern hemisphere. This was exactly the district occupied by the same shower at its appearance in the year 1832, and it may be expected that this great shower, like that of 1833, will this year be again visible in America on the morning of the 14th November next. In that case, it will be only partially visible in Europe; but it may include some of the most brilliant parts of the shower, so as not to allow of losing the opportunity of seeing what there is. The position of the radiant point, as well as the moment of the maximum abundance, was distinguished with great precision at the Royal Observatory, Greenwich, and, compared with observations at other places, leave nothing to be desired in respect of philosophical exactness. The moment of maximum frequency, observed at the Cape of Good Hope Observatory, shows that South Africa, on account of its high southern latitude, entered the densest portion of the shower about fifteen minutes earlier than the same phase of shower was witnessed in the British Isles; while the total duration of the shower, at all the stations, shows that the greatest thickness of the stream of meteoric bodies through which the earth passed in two hours was about thirty thousand miles. The inclination of the stream to the earth's orbit was also determined, whilst, by a new consent of eminent master-minds—M. Le Verrier and Professor Adams—at the end of elaborate calculations, both agreed that the true orbit of the meteoric stream is a long ellipse extending from the earth's orbit at its least, to that of Uranus at its greatest distance from the sun. The periodic time of the meteors in their orbit is thirty-three years and a quarter. The inclination of the orbit to the plane of the elliptic is about seventeen degrees, and the meteors revolve round the sun in the opposite direction to the earth. It appears that the densest portion of the group has not yet been passed through, as it occupies such an extent of length along the elliptic orbit as to require two or three years to make its passage round the sun.

A most curious incident connected with these discoveries is, that a comet detected by Tempel shortly after the first outposts of the November meteors made their appearance in 1865, to which an elliptic orbit, with a period of thirty-three years and a quarter, was assigned by Oppolzer, before the recent display of the November meteors was observed, is found to move in exactly the same orbit with the meteoric bodies, throughout their entire revolution round the sun. A coincidence so unexpected, and against which the probabilities are *a priori* so enormous, must alone make the physical connection between Tempel's comet and the group of meteoric bodies little less than certain. But the astronomer of the Brera College of Milan, Signor Schiaparelli, had already published the announcement in a letter written previously to this discovery to the Padre Secchi, that the orbits of the St. Lawrence's meteors of the 10th of August, which he supposes to be nearly parabolic, must, in that case, coincide almost exactly with the long elliptic orbit of a very conspicuous comet, known as Swift's or Tuttle's comet, which appeared in August and September, 1862. A similar inquiry has since been made by Dr. Weisse regarding the orbit of the group of April shower-meteors, supposed, like the former, to be nearly parabolic; and this is found to coincide almost exactly with the long elliptic orbit of a bright comet which was visible for some weeks in the month of June of the year 1861.

Shower meteors thus continue to engage great attention, and by the light which their new-found relation to those most mysterious messengers from distant space, may end by throwing light upon the obscurity of the phenomena of comets.

The spectroscope has been turned with some success to analyse their light; and it was found by Mr. Huggins that the nucleus of Tempel's comet was self-luminous, shining with a single ray of bluish light; while the pale light of the envelope consisted of the sun's reflected rays. Spectroscopes of the best form that could be devised were turned towards the streaks of the November meteors; and in some of those was also recognised a single ray of a lavender blue, or of a greyish colour.

It is not impossible that the meteoric particles are portions of the comet's tails, shreds of a dismembered mist, torn by the sun's disturbing action from the nucleus of the comet, and left upon its path like embers or smoke-flakes in the track of an expiring flame. But is the heat of their collision with the atmosphere sufficient to restore a portion of the luminous appearance with which they shone in the nucleus of the comet? or are the November meteors and Tempel's comet perfect nebulæ undergoing condensation, of which the meteoric bodies are the quite-faded stars, and the cometary nucleus is the still gaseous and self-luminous portion of the nebula? When the bright and persistent character of the cometic portions of the November meteor streaks is borne in mind, the telescope armed with the spectroscope may still enter the field on the eventful morning of the 14th of November next. An answer will then, if possible, be given to questions which as yet hardly admit of being rightly framed, so unexpected are the revelations, and so novel are the conceptions which a few short months have introduced into the rapidly advancing theatre of meteoric astronomy. These observations are thus given, and it is to be hoped they may prove of some value in ascertaining the character of meteors and comets. When, on the next starry shower or appearance of November meteors, the telescope or stereoscope are presented to these bright streaks, we may thus hope that answers may be given to the questions which can as yet hardly be framed—for sudden are the conceptions which have in the few past months dawned upon us in this history of meteoric phenomena.

ON FLUO-SILICATE OF BARIUM.

BY M. FR. STOLBA.

To obtain this salt free from sulphate of baryta and silica, first add to the hydro-fluosilicic acid a little baryta, and separate the precipitate obtained; the filtered acid then contains neither sulphuric acid nor silica, and gives a pure salt by saturation with hydrate of baryta.

The pure fluo-silicate of barium is in the form of small microscopic prisms when obtained by boiling with diluted liquids; these prisms are united in clusters or disposed in stars.

Its density at 21° = 4·2772 (mean). It dissolves at 21° in from 3262 to 3319 parts of water, and in 3731 parts of water at 17°; boiling water dissolves about three times as much.

Acids dissolve it more easily, and some salts also increase its solubility. It dissolves at 22° in 272 parts of nitric acid (containing 8 per cent of $N_2\Theta_5$), and in 448 parts of hydrochloric acid containing 4·25 per cent. of HCl.

To dissolve it requires 563 parts of a concentrated and boiling solution of sea-salt; 349 parts of a boiling solution of sea-salt; containing 10 per cent. of this salt; 2185 parts of a solution at 10 per cent of sea-salt at 20°; 1140 parts of a solution at 5 per cent. at 20°; 306 parts of a solution of saturated sal-ammoniac at 22°; 361 parts of a solution of sal-ammoniac at 15 per cent. at 22°.

Dilute sulphuric acid slowly decomposes the fluo-silicate of barium when cold, rapidly with heat. This property may be utilised in the preparation of pure hydro-fluosilicic acid; to do this, digest with heat the fluo-silicate of barium, well divided, with nine-tenths of the sulphuric acid necessary wholly to decompose it, until the filtered liquid contains no traces of sulphuric acid.

Sulphates also rapidly decompose the fluo-silicate of barium, but incompletely. Boiled with the alkaline carbonates, it decomposes, leaving a mixture of silica and carbonate of barium; but some silica which adheres strongly to the sides of the vessel is produced at the same time. Calcined with sal-ammoniac, a large portion is changed to chloride of barium, but a complete transformation is very difficult.

The aqueous solution of fluoride of barium is one of the best tests to ascertain the presence of sulphuric acid in the solution of hydro-fluosilicic acid, or of fluosilicates, it is preferable to sulphate of strontium.—*Journal für Praktische Chemie*, xcvi. 22.

VOLATILITY OF THE COMPOUND OF IRON WITH SULPHOCYANOGEN.

BY WILLIAM SKEY,

Analyst to the Geological Survey, New Zealand.

When a solution of sesquichloride of iron and an alkaline sulphocyanide is treated with a large excess of hydrochloric acid, there is evolved, even at common temperatures, a notable quantity of a red coloured compound which is best arrested and collected by porous bodies or rough surfaces. It gives the reactions of iron and sulphocyanogen.

The production of this volatile iron compound is easiest observed by placing a solution of the above substances in a shallow vessel resting upon white paper, over which a slightly larger vessel is inverted; in a short time a red coloured ring appears upon the paper. The penetrability of this vapour is such that in a short time it traversed through five thicknesses of thick writing paper.

As thus attached to paper, it did not re-volatilize or change in a neutral atmosphere at a temperature of 200° F.; but on moistening the paper with water, its colour immediately went. In ether, however, it is soluble without change of colour.

The composition of this volatile iron compound I hope to be able to furnish by the next homeward mail. In the meantime I cannot avoid remarking how its volatility at low temperature, its colorific properties, and its production only in presence of a more powerful acid than the hydrosulphocyanic, gives force to the supposition that the iron present in it is united with the element of sulphocyanogen to form a salt radical of the same molecular constitution, probably, as ferrocyanogen, thus:

$$\mathrm{Fe}+\begin{matrix}\mathrm{Cy}\\ \mathrm{S}\\ \mathrm{S}\end{matrix} \qquad\qquad \mathrm{Fe}+\begin{matrix}\mathrm{Cy}\\ \mathrm{Cy}\\ \mathrm{Cy}\end{matrix}$$

the sulphur occupying the place of as many equivalents as cyanogen, at all events with the exception of the compound of sulphocyanogen with tungsten. I should be inclined to view it as differently constituted to the sulphocyanides just described.

ON THE PROPERTY OF

TUNGSTIC AND SILICIC ACIDS TO COMBINE WITH PHOSPHORIC ACID,

AND THE PRESENCE OF THIS ACID IN OPAL, FLINT, QUARTZ, ETC.

BY WILLIAM SKEY,

Analyst to the Geological Survey, New Zealand.

In considering the behaviour of molybdic with phosphoric acid, it occurred to me whether other analgous phospho-compounds with the more insoluble mineral acids might not be made, or even have a present existence, but unrevealed, owing to the want of those decided colorific and physical changes which are involved in the production of phospho-molybdic acid from its individual compounds, and which in all probability led to the discovery of their mutual chemical affinities for each other. Many reasons for the possible existence of such compound presenting themselves, I began a course of experiments with tungstic acid, when it was soon ascertained most conclusively that this acid was able to effect a combination with phosphoric acid, in acid solutions. The compound so formed is void of colour even when boiled, whereas recently precipitated tungstic acid soon contracts a persistent yellow colour; under similar circumstances, the colour proper to the ignited acid.

Following up the subject, silica was next selected for examination, and the results thereof appearing to have some degree of interest, I would beg to state them here.

In the first place, a portion of quartz rock was pounded and fused with a mixture of carbonate and phosphate of soda, and subsequently treated in exactly the same manner as that described by Fresenius for the separation of silica. The insoluble residue from the hydrochloric acid was then thrown on a filter and washed until not a trace of phosphoric acid could be detected in the washings therefrom. A solution of pure ammonia was then passed through the residue on the filter, and to the filtrate ammonio-chloride of magnesium was added, when a copious gelatinous precipitate appeared, which partly dissolved in acetic acid, and the filtered solution gave a considerable amount of a crystalline adhesive precipitate when rendered alkaline by ammonia: the part insoluble in the acid was silicate of magnesia. A portion of the crystalline precipitate above mentioned furnished yellow crystals when placed in contact with a solution of molybdic acid in nitric acid.

The capacity of silica to retain phosphoric acid in presence of water or hydrochloric acid being thus demonstrated, the possibility of the presence of phosphoric acid in certain of the more siliceous minerals naturally suggested itself; and forthwith I proceeded to examine some of these minerals, when I found unmistakeable indications of phosphoric acid. I also detected phosphoric acid in a specimen of flint, and in a portion of the quartz rock already treated of, but in quantity not nearly so large as when phosphate of soda was fused with it in the first experiment. I should state that in this instance, to get a fair comparison, the quartz was first fused with carbonate of soda, so that the whole of the phosphoric acid could be eliminated; but in the case of the other silicas, for the removal of their phosphoric acid for detection, I merely passed ammonia through their very finely ground powders. Subsequently, however, I obtained phosphoric acid from quartz by the same means.

As bearing upon the complete separation of phosphoric acid from granites, basalts, etc., I may further state that, as far as I have gone, I have invariably found phosphoric acid as a constituent of their silicas as separated for estimation. In one case I obtained ·16 per cent. of phosphate of magnesia admixed with a small proportion of silicate of magnesia; but in the course of the laboratory work I hope to be able soon to furnish the absolute amount of phosphoric acid thus retained in those *silica residues* which come under my notice.

In conclusion, it would seem that the facts here recited tend to show—First—That the present mode of extracting phosphoric acid from siliceous minerals for estimation is radically wrong, or at least very imperfect, much of the phosphoric acid these minerals may contain being determined to their silica when recently precipitated, if not already in combination therewith. Secondly—That in the great majority of the analyses of silicates, etc., the silica therein is given a trifle too high; and, lastly—That phosphoric acid exists in larger quantity, and is even more widely distributed through the mineral kingdom than has hitherto been suspected—circumstances possessing some degree of interest in connection with both mineralogy and agriculture.

NOTICE OF

NATROBOROCALCITE IN NEW LOCALITIES,

AND OF OTHER BORATES IN HANTS COUNTY, NOVA SCOTIA.

BY PROFESSOR HOW, WINDSOR, N.S.

In this journal, April 19th, 1867 (*Eng. Ed.*), I mentioned incidentally some facts respecting the occurrence of natroborocalcite here, and having lately found this mineral in the same matrix in new localities in this county, and also in its neighbourhood at one of these places, some other borates not before observed here, and (one at least) probably new, I offer a short note on the subject as possibly not uninteresting.

The revival of the trade in gypsum with the United States, which had declined very much during the late war, has caused great activity in the quarries of Nova Scotia, especially in this vicinity, which affords the greatest supply to that market. Not less than 70,000 tons of "plaster" have been cleared from this port during the 18 months ending June 30 of this year, the larger part of which has been shipped from the wharves of Windsor, having been brought from several quarries close at hand, or within some six miles. The rock exported consists principally of gypsum, the rest being anhydrite; both are called in the official returns gypsum, and locally, plaster, hard and soft respectively. An excellent opportunity is thus afforded here of studying the varieties of these rocks and the minerals they contain. In one of the heaps of "stones" brought from a quarry which had not been worked for twenty years until last season, I saw a specimen which I at once recognised as natroborocalcite, and, on examination, I found a considerable number of fine specimens, varying in size from that of a pea to that of a small hen's egg, embedded in gypsum, chiefly in a soft grey or "blue" variety, the colour of which rendered the snowy white borate very obvious. The rock was from near the surface of a quarry called Brookville, situated about three miles south of the original locality (mentioned in the paper in the journal above referred to), where the rocks dip gently to the south: low hills and a broad marsh lie between the two quarries. I afterwards observed the same mineral in gypsum from a quarry about six miles north-east of the original locality, but not in such abundance as at Brookville.

The Brookville quarry affords two other borates, one of which is a hydrated silicated borate of lime, apparently a new species. The observation of combined silica in more than traces in these beds for the first time is interesting. The other borate seems to be quite different from any before observed here, but I have not yet subjected it to analysis.

NOTE ON THE

PREPARATION OF CRYSTALLISED PHENIC ACID.

BY W. E. BICKERDIKE, F.C.S.

It is seldom that crystallised phenic acid can be obtained by the common process described for its preparation in the text books; and even when the crude liquid does crystallise, the still fluid portion contains the largest amount of the pure acid.

The following process I find always to give good results:—

The impure liquid separated from tar oils in the usual manner by means of soda solution, is first distilled alone, so as to get rid of most of the water and H_2S. It is then re-distilled in a perfectly dry retort with 1 to 2 per cent. of anhydrous cupric sulphate, collecting the distillate in 5 or 6 dry flasks. Most of the distillate will crystallise at 16° C., though it is generally necessary to drop in a fragment of the solid.

If *much* H_2S is present, it should be removed by boiling, or by leaving the liquid in an open vessel, over night, previous to distilling with the sulphur.

Dalton Square, Lancaster, September 25th.

ON ULTRAMARINE.

BY DR. ERNST ROHRIG.

The manufacture of artificial ultramarine is one of great interest, and has been the object of careful investigations by many eminent chemists during the last fifty years. But none of these investigations have shown the real cause of the colour of ultramarine, nor have they given a sufficient explanation of the chemical changes which the components undergo in its formation. This explanation would be the more interesting as it would supply some hints with regard to the nature of the colouring matter of many minerals, besides many chemical and metallurgical products.

It seems surprising to the writer that this manufacture has not been introduced into England, as the materials used in it are partially English, and the others could be obtained cheaper here than in Germany and France, where the manufacture is carried on to a great extent; and as furthermore the greatest trade and consumption of ultramarine takes place in England.

These facts will, perhaps, render the following communication acceptable to the readers of the Chemical News:

In former times ultramarine was produced from *lapis lazuli*, and being very rare had double the value of gold. The accidental formation of blue ultramarine-like masses in certain chemical processes (manufacture of soda) induced trials for the production of artificial ultramarine. Such trials by Guimet, in Toulouse, and Gmelin, in Tübingen, were crowned with success, almost simultaneously, though both investigations were carried on independently of each other.

Guimet has always kept his discovery a secret, but Gmelin[*] published his in 1828, and may, therefore, be considered the founder of the present ultramarine manufacture.

Both investigators have, no doubt, taken as the basis of their experiments the composition of the natural ultramarine from *lapis lazuli*. The composition of that ultramarine, according to Desormes and Clement[*], is as follows:—

Silica	35·8
Alumina	34·8
Soda	23·2
Sulphur	3·1
Carbonate of lime	3·1

Some inferior sorts also contain iron.

Gmelin's mode of operation is the following:

Solution of caustic soda is saturated with hydrated silica, and to this hydrated alumina, containing 90 per cent. water, is added in such quantity that 35 parts of dry silica, will correspond to 30 parts of dry alumina. This mixture is evaporated to dryness. It is then mixed with some sulphur, pulverised and thoroughly mixed with an addition of dry carbonate of soda and flowers of sulphur. A quantity of each of the latter two substances is added equal in weight to the above dried mixture. This compound is pressed into a crucible, heated as quickly as possible, and kept at a red-heat for two hours. The resulting green-yellow mass is then heated in contact with the air till it becomes blue. After this it is boiled out with water and well washed.

A number of different modes of producing ultramarine were afterwards published by chemical investigators.

Robiquet[*] recommended a mixture of 2 parts china-clay, 3 sulphur, and 3 carbonate of soda.

Tiremon,[†] 100 parts alumina or hydrated alumina, 1075 crystallised (or 400 dry) carbonate of soda, 221 sulphur, 3 sulphide of arsenic.

Prückner,[‡] 200 parts sulphide of sodium (prepared by heating sulphate of soda with carbon and some sulphur), 50 clay as pure as possible, and 1 green copperas.

Brunner[§] heated a mixture of 70 parts sand, 240 alum (the weight calculated for anhydrous alum), 48 carbon, 144 sulphur, and 240 carbonate of soda, in a closed crucible, at a low red-heat for about 1½ hours. The resulting mass, partly of a greenish hue, and partly of a reddish yellow hue (and which was reduced to ⅜ of its original volume) was washed. He obtained a light ash-grey powder, which he mixed with an equal weight of sulphur, and with 1½ times its weight of dry carbonate of soda and heated as before. This caused it to take a blueish-green hue. He then spread flowers of sulphur, until about $\frac{1}{12}$ of an inch in depth, upon an iron plate, and on this a similar quantity of the last product, after having pulverised it. By heating the iron plate, he ignited the sulphur, taking care to keep the temperature as low as possible, avoiding heating the ultramarine to redness. After repeating such heating with sulphur 3—4 times, the ultramarine became of a fine dark blue colour. In this process it is necessary to remove the ultramarine from the plate, and to pulverise it afresh each time.

By roasting this ultramarine with sulphur it increases in weight from 5—10 per cent. and it is possible for it to increase still more if the heating is oftener repeated; however, the colour will not improve any more. If the ultramarine be heated without addition of sulphur, a decrease of weight takes place (probably as then sulphur becomes exchanged for oxygen), the ultramarine becomes paler and the powder more compact and granular.

According to *Gentele's* published method ultramarine is produced in manufactures from a mixture of clay with sulphate of soda and carbon; or with carbonate of soda, sulphur, and less carbon; and also with sulphate of soda, carbon, carbonate of soda, and sulphur. By heating these mixtures a green ultramarine would result, which by roasting with sulphur, in contact with air, would become blue.

Ritter[‖] used for the production of ultramarine, a mixture of clay, sulphate of soda, and carbon.

He used china-clay from Cornwall, of the following composition.

Silica	47·06
Alumina	36·47
Peroxide of iron	1·10
Potash	3·16
Water	12·05
	99·84

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[*] *Annales de Chemie* lxvii. 317.

[†] *Naturwissenshaft. Abhdlngen. Wurtemberg*, ii. 191.

[*] *Annali de Pharm.*, x. 91.

[†] *Comptes Rendus*, xiv. 761.

[‡] *Dingl. Polyt. Journ.*, xciv. 388.

[§] *Poggend. Annalen*, lxvii. 541.

[‖] *Rutter über des Ultram, Göttingen*, 1860.

This mixture was roasted at a strong red-heat (900–950°); the result was raw ultramarine, white in colour, similar to that obtained by Brunner. This he called white ultramarine, as it is the first transformation of the alumina by the action of sulphide of sodium. This white compound, when exposed to atmospheric air at common temperatures, does not change, but if heated for some time at 100°, it slightly increases in weight, and if heated to 400° in free contact with air, it will take a dark yellow colour which changes to green on cooling. If the heating is continued for a longer period the Ultramarine will become blue. All the colours produced in this way are very pale, but they may be made much darker; the change takes place in a shorter time if sulphurous acid or chlorine is made to act at the same time, upon white ultramarine.

The components of the white ultramarine, as furnished by analysis, are the following:—

Al_2O_3	31·17
Na	17·75
K	1·33
Fe	0·07
a S	4·78
b S	1·42

a S is that which (according to Elsner and Breunlin) is evolved as sulphuretted hydrogen gas, and *b* S that sulphur which is retained in the residue.

The composition of the white ultramarine may be calculated as follows:—

SiO_2	39·06
Al_2O_3	31·17
Na O	14·75
K O	1·60
Na S	3·09
$Na\,S_2$	4·88
Fe S	0·11
	99·66

When this white compound is heated it becomes green and then blue, as one atom of S of the NaS_2 burns to sulphurous acid, causing the transformation of the colour.

As before stated, the addition of sulphur hastens the process of conversion. In one of Ritter's experiments, the quantity of extracted sulphur amounted to 1·3 per cent. and exactly the same quantity was contained in the NaO,SO_3 (1·32) formed. From this and from the proportion of SO_2 to the increase of weight it may be calculated that two equivalents, SO_2 combine with one equivalent, NaS forming NaO,SO_3+2S. It is singular that the sulphur of the Na[illegible] is not extracted, and that it combines with another portion of NaS_2 of the ultramarine forming a higher sulphuret of sodium, while none of the sulphur existing as SO_2 is absorbed.

By calculating one equivalent oxygen for one equivalent of the sulphuric acid formed according to the first mentioned process:—

$$(Na_2S_2+S_2O_4—NaS_2+NaO,SO_3+S).$$

It will be found that the number calculated will express almost exactly the increase of weight found:—

Increase of weight.	1·89	4·49	4·38	per cent.
SO_2+O	13·95	4·44	4·20	“

It is evident that sulphurous acid does not enter into the constitution of ultramarine, and acts only by abstracting some sodium from it.

According to Gentele's observation, chlorine will answer the same purpose; Ritter used it, and found it succeed perfectly well. As sulphurous acid only forms SO_2NaO, and Cl forms NaCl, omitting mention of a trace of ClS. From this it may be concluded, with perfect safety, that the sulphur is exclusively combined with sodium. If it were combined with Al and Si, MCl and SiO_2 would be found in the aqueous extract besides NaCl, which is not the case if dry chlorine free from hydrochloric acid is employed, and if the heat is not excessively high and of long duration. This fact also proves that the NaS in ultramarine exists in real chemical combination with the silicate; otherwise it would be completely decomposed by the action of chlorine. The silicate prevents the Na Cl from undergoing decomposition, and is able to combine with NaS.

The quantity of oxygen which is absorbed may be calculated from the proportion of the sulphuretted hydrogen gas evolved by acids from the blue ultramarine. By deducting the quantity of S, which is equivalent to the absorbed chlorine from the *a* S of the white ultramarine, the remainder will correspond to that quantity of sulphur which should be evolved by acids. And, as for each equivalent of O afterwards absorbed, one equivalent less will appear as sulphuretted hydrogen, the quantity of S in question may be calculated from the difference between that number and that which expresses the *a* S of the blue ultramarine. The real quantity will be equal to half the difference, as the equivalent of oxygen is half as large as that of sulphur.

Ritter found, by analysis, blue ultramarine to be composed of:—

	Silica	40·40	per cent.
	Alumina	31·88	“
	Soda	15·18	“
	Potash	1·65	“
Na 2·39 S 5·55	Na S	7·94	“
	Hyposulphite of Soda	1·84	“
		98·89	

The amount of sulphide of sodium and hyposulphite of soda is, of course, variable, and depends on the greater or less action of sulphurous acid or chlorine.

Several other formulæ for producing ultramarine have been published. Only those already referred to have been mentioned, as the investigators have at the same time started some theory explaining the blue colouration.

Having been engaged in ultramarine manufacture for a number of years, I may also state that all the published modes of producing ultramarine are incapable of application to the manufacture on a large and lucrative scale, though they are all very well for analytical researches in the laboratory. I may be allowed to state here also, that the mixture for manufacturing ultramarine is not the only secret connected with it; there are many others besides, which manufacturers mostly obtain by many very expensive experiments.

The supposed colouring principle in ultramarine has been variously stated by different investigators.

Before the composition of lapis lazuli was known, it was believed that copper was the cause of the blue colour.

Marggraf* first confuted that opinion, and he attributed the blue colour to iron. Klaproth† was of the same opinion.

Guyton Morveau‡ also attributed the colouring matter to iron. He had observed that sulphate of lime containing iron became blue when heated with carbon, and that the blue colour disappeared again when it was acted upon with acids.

Varrentrap§ inferred from the analysis of nosean, sapphirine, lapis lazuli, and artificial ultramarine, that the intensity of the blue colour increased with the amount of sulphur and iron, and he considered it possible that the blue colour was attributable to sulphide of iron.

This analysis of artificial ultramarine gives the following numbers:—

Silica	45·604
Alumina	23·304
Soda	21·476
Potash	1·752
Lime	0·021
Sulphur	1·685
Sulphuric Acid	2·830
Peroxide of Iron	1·063
Chlorine	trace
	98·735

Pruckner‖ stated that he could not produce ultramarine from clay which was free from iron; he, therefore, recommended an addition of green copperas, and was of opinion that iron was essential for the blue colouring.

Elsner¶ arrived, by a number of experiments, at the conclusion that the colouring principle was a combination of sulphide of iron and sulphide of sodium. He found that Gmelin's ultramarine base free from iron heated with sulphur and soda, also free from iron, produced a yellowish-white mass, whilst when using materials containing iron, a green mass was obtained.

Now, the supposition of iron being the blue colouring matter has a very weak foundation, since, on the contrary, it is proved by Gmelin, Desormes, Clement, Bounner, and Ritter, that ultramarine may be produced from materials free from iron.

Iron, and likewise lime, magnesia, etc., substances which are often to be met with in ultramarine, are only accidental components, and have no relation whatever to its blue or green colour.

Desormes and Clement were the first who found, by analysing the natural mineral, that it contained no iron. This metal, therefore, could not be the colouring principle; but they expressed no further opinion on the subject.

Gmelin considered the sulphur as the cause of the blue colour. He thought he had observed that in decomposing ultramarine by hydrochloric acid, sulphuretted hydrogen gas and sulphuric acid were formed, and he concluded from this that either a metallic sulphide, together with a sulphate, were contained in ultramarine or hyposulphurous acid, which was decomposed into sulphuric acid and sulphuretted hydrogen, a decomposition of water taking place at the same time.

Gmelin seems to have given the preference to the latter supposition, which is yet clearly erroneous, as hyposulphurous acid, when separated from dilute solutions of its salts by means of an acid, becomes decomposed into equal equivalents of sulphurous acid and sulphur.

Schweigger Seidel* states that the colouring matter of ultramarine is Vogel's blue sulphuric acid (solution of sulphur in anhydrous sulphuric acid); but he does not prove how sulphuretted hydrogen gas could possibly be evolved from that acid; and, besides, there is no sulphuric acid contained in ultramarine.

Elsner called attention to the important fact that by decomposing ultramarine by means of acids, sulphuretted hydrogen gas was evolved, and that, at the same time, sulphur became separated. He found the proportion of the evolved sulphuretted hydrogen gas to that of sulphur to be—

In green ultramarine at	3·6 : 1 and
" blue do "	1 : 7

and he supposed that the blue ultramarine contained a higher degree of sulphuration of sodium than the green ultramarine.

Breunlin† was of the same opinion. A great number of analyses of the blue and green ultramarine induced him to consider that both kinds of ultramarine consisted of a combination of a nephelin-like silicate and sulphide of sodium, the latter in blue ultramarine having the formula NaS_6, and in green ultramarine, NaS_2.

He made the following formula to represent the composition of the ultramarine:—

$$2\left([(2NaO)SiO_3]+2[Al_2O_3,\ SiO_3]\right)+NaS_6\Big)$$

and for green ultramarine—

$$2\left([(2NaO)SiO_3]+2[Al_2O_3,\ SiO_3]\right)+NaS_2\Big)$$

Gentel‡ found also by analysing different yellowish-green sorts of ultramarine, a proportion of evolved sulphuretted hydrogen gas and separated sulphur corresponding to NaS_2, but he is of opinion that the green ultramarine has a very different composition, and he assumes that it contained a mixture of different combinations of sulphur with sodium. He found in one sample of blue ultramarine a proportion corresponding to NaS_{10}, and he thinks it not improbable that ultramarine consists of a silicate of alumina, and a combination of silicate with sulphide of sodium. But he considers the proportions not sufficiently ascertained to justify forming a formula.

Wilkens§ concluded from this mode of producing ultramarine that the blue colouring principle was hyposulphurous acid in combination with soda and NaS.

Stolzel‖ considered sulphite or hyposulphite of soda to be the cause of the blue colouration.

Brunner¶ supposed that blue ultramarine contained oxidised sulphur; but he made this supposition from an erroneous observation (influence of burning sulphur upon green ultramarine).

Ritter has drawn from his investigations on the subject the following conclusions:—

1. The combination formed by heating NaS together

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* *Marggraf's Chem. Schriften, Berlin*, 1768.
† *Klaproth's Beitrage z. Chem. Kennts. der Mineralien* i. 189.
‡ *Poggend. Annalen* xlix. 521, 550.
§ *Ann. de Chimie*, xxxiv. 54.
‖ *Journal f. Pract. Chemie*, 1844, *bd.* 3.
¶ *Journal f. Pract. Chemie*, xxiv. 285, xxvi. 106.

* *Schweigger's Journ.* liv. 280.
† *Ann. de Cheme u. Pharm.*, xcvii. 295.
‡ *Dingl. polyt. Journ.* cxli. 116.
§ *Ann. de Chem. und Pharm.*, xcix. 28.
‖ *Ann. de Chem. und Pharm.* xcvii. 35.
¶ *Poggend. Annalen*, lxvii. 541.

with silicate of alumina is colourless, and consists of silicate of alumina and soda and NaS, and a higher sulphide of sodium; this compound contains no combination of oxygen with sulphur.

2. By extracting some sodium from the white ultramarine (by the action of Cl or SO_2) a corresponding quantity of S will combine with the remaining NaS, and form a higher sulphuret of sodium.

3. The white ultramarine converted in such way absorbs oxygen (one part of the NaS being transformed into an oxygen compound), and takes the blue colour. The blue ultramarine, therefore, forms a combination of a silicate of alumina and soda, polysulphide of sodium, and sodium salts with one of the oxides of sulphur.

4. It is probable that the combination of O with S contained in blue ultramarine is either as sulphite or hyposulphite of soda; the latter is the more likely.

5. KS, when heated with silicate of alumina, does not form an ultramarine-like combination; the result is a silicate of alumina and potash free from sulphur.

We know that sulphur may take a blue colour in three cases:—

1. When sulphur is in combination with anhydrous sulphuric acid;

2. By melting Rhodanide of potassium at a temperature approaching a red heat;

3. The sulphur which gets separated in mixing sulphuretted hydrogen with chloride of iron.

Considering these facts, it may be possible that a blue modification of sulphur may exist in ultramarine.

ON A

NEW APPARATUS FOR TECHNICAL ANALYSIS OF PETROLEUM AND KINDRED SUBSTANCES.

BY S. F. PECKHAM.

In the Chemical News for August 31st, 1866 (*Eng. Ed.*) I noticed a paper in which was described a process with apparatus, for the assay of coals and other substances yielding illuminating and paraffin oils.* After stating the fact, that no process had hitherto been described by which technical analyses of bituminous and pyro-bituminous substances could be made to yield analogous and satisfactory results, the author proceeds to describe what I should suppose to be a very valuable process for the primary distillation in the technical analysis of coals and shales. I do not repeat the description here, as it would require considerable space, and it can only be applied to the treatment of solid substances, which do not melt at a temperature below that required for their distillation. As the original paper is easy of access, I would recommend its perusal to all who wish to make technical analyses of either coals or shales. The apparatus is simple and inexpensive, and I am aware of no reason why the results furnished by it should not prove highly satisfactory, especially as its operation bears a striking resemblance to the most improved processes of manufacture on the large scale.

But beyond the primary distillation of the coal or shale, I do not consider that our author has added anything to processes long in use. When he arrives at the second distillation, or that which corresponds to the primary distillation of petroleum, he is forced to return to the old process of fractional distillation from a common tubulated glass retort. This process is not only very unsatisfactory in its results, but it is quite expensive, and is attended with considerable danger from fire. It is unsatisfactory, because the separation of fluids of different densities and different boiling points, is much less complete than by Warren's process, for any temperature below the boiling point of mercury; in fact, for any temperature necessary to ensure the complete separation of the light oils usually called naphtha, and the "photogen" or illuminating oil.* It is expensive, for the reason that if the distillation is conducted to dryness the retort is sacrificed, as it is rarely possible to remove the coke with safety. It is attended with danger from fire, because the best retorts are liable to fracture from the high heat to which they are exposed, even when the greatest care is exercised in conducting the operation.

I was about to commence a technical examination of several specimens of California petroleum, when the above mentioned paper arrested my attention, and I was unpleasantly conscious when I had finished its perusal that in respect to apparatus for this department of research my want was just as far from being supplied as it was six years since, when I commenced the study of petroleums. I had but a small quantity of each specimen, and besides subjecting them simply to fractional distillation, I wished to test them by Young's process of distillation, under pressure. To conduct the latter process in glass was an impossibility.

To answer my purpose, therefore, my apparatus must fulfil the following conditions. It should be capable of working not more than one and one-half litres, and admit of being heated by an ordinary gas furnace. The joints should sustain a pressure of forty pounds per square inch, and it should be so constructed as to admit of the ready extraction of the coke. I could find no description of any such apparatus, but after numerous failures and corrections, in an apparatus of my own invention I found my want so well and fully supplied, that I am led to offer a description, for the benefit of those who, like myself, have felt the need of such an instrument.

Upon each extremity of a piece of a wrought iron gas-pipe, three inches in diameter and twenty inches in length, a cap is securely screwed. The caps should be heated nearly to redness and screwed on to the cold pipe in order that by their contraction they may be more firmly secured. The pipe is then put in a lathe and the caps turned off in such a manner as to leave a band upon each end of the pipe, about three-fourths of an inch in width, and two circular discs of iron, each about four inches in diameter, and one-fourth of an inch in thickness, having a projection upon one of their surfaces to which a wrench may be applied. The edges of each extremity of the pipe with the bands are now carefully turned off, presenting smooth surfaces slightly bevelled inwardly. The plane surface of each of the discs is then so turned off upon its circumference, that it will exactly fit the bevelled edge of the pipe. This completes the retort.

A stout parallelogram is then made half an inch longer and wider than the retort, one of the shorter sides of which should contain in the middle a stout set-screw, and the other an orifice made to fit the projection upon the disc. This may be called the frame.

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* On the Assay of Coal, etc., for Crude Paraffin Oil, and of Crude Oil and Petroleum for Spirit, Photogen Lubricating Oil and Paraffin, by John Attfield, Ph. D., F.C.S., Director of the Laboratory of the Pharmaceutical Society of Great Britain, Chem. News, vol. xiv. p. 98. *Eng. Ed.*

* For details of this process, see Chem. News, vol. xii. p 85.—*Eng. Ed.*

Two holes are then drilled a short distance from either extremity of the retort, and in a line parallel to the axis of the retort. One of these should admit a half inch, and the other an inch gas-pipe. With this arrangement the retort may be used either for pressure distillation, or for distillation by the ordinary process. It also admits of being connected with an apparatus for furnishing superheated steam or carbonic acid gas, either of which are sometimes used to assist the distillation of hydrocarbons. Both the goose-neck and valve should be connected with the retort by a short piece of gas-pipe and a brass "union" or coupling, as the difference in the expansion of brass and iron would cause a joint of the two metals to leak very badly when subjected to a high temperature. The goose-neck may be made of the ordinary form, tapering from one inch to one-quarter inch, and about ten inches in length. The material should be copper brazed. The valve will be described hereafter.

In order to use the retort, one of the discs is luted with a very thin paste of plaster of Paris and firmly pressed into its seat. The retort is then slipped into the frame and left a moment for the luting to set, the open end being uppermost. The oil is next poured in and the other disc luted into its seat, the frame adjusted and the set-screw firmly set up, so as to securely fasten both discs in their places. The goose-neck or valve is then adjusted, and the connections made with the worm and receiver. It will be observed that all the expansion that takes place in this retort only brings the different portions of the apparatus more firmly together, instead of causing them to crack apart and leak with every slight variation of temperature, as is usually the case. With this arrangement I was able to distil fifteen hundred cubic centimeters of petroleum to dryness, the last portions coming over at a red heat. The distillation was commenced with two ordinary Bunsen's gas lamps, increased as required to four, and towards the end of the operation to six—the latter number being sufficient to bring the side of the retort in contact with the flame to a bright cherry-red heat.

Any one who has attempted the distillation of small quantities of petroleum in either iron or copper stills, or retorts of whatever form, imbedded in coal fires or suspended over them, must be aware of the difficulty of so regulating the fire as to secure a constantly increasing heat from beginning to the end of this operation. No such difficulty is experienced with this apparatus. In it the lightest oils may be distilled by means of a sand-bath, and the heaviest by applying the flame of a sufficient number of lamps directly to the retort. The joints of this apparatus when luted with the smallest possible quantity of finely pulverized calcined sulphate of lime, admit of the least loss by leakage of any metallic retort that I have ever used. With the exercise of proper care the amount of distillate from California petroleum averaged above ninety per cent. by measure, and with a pressure of thirty pounds per square inch the average was eighty-seven and one-half per cent. In the latter instance the loss was increased by the formation of gas and vapours that passed through the worm uncondensed at 8° C. The largest amount of distillate that I have seen recorded, as yielded by any material of undoubted natural origin, is ninety-five and one-half per cent. by measure.* The distillation of which this was the product was performed wholly in glass, without pressure, the crude material being a California petroleum of medium density, yielding no permanent gases and no naphtha. In this case the loss may be estimated at zero. I think it will be readily conceded, that any apparatus which admits of the ready extraction of the coke, and at the same time yields an average of ninety-two and one-half per cent. of distillate, furnishes results far more satisfactory than any hitherto in use for operating upon so small a quantity as fifteen hundred cubic centimeters.

A thermometer may be inserted in the smaller orifice, for noting the temperature at which light oils distil. A piece of gas-pipe of the requisite size and about two inches in length may be used for making the connection, the thermometer being luted into one end of it. When but one of the openings in the retort is in use, the other may be closed with an iron plug.

In making my experiments upon Young's process of distillation under pressure, I experienced much difficulty in contriving an apparatus that would enable me to register the amount of pressure, and at the same time prevent any loss of vapour. I first attempted to register the pressure by means of a U tube, the arms of which were of unequal length. The tube was filled with mercury to a level with the shorter arm, and the long arm sealed with a column of air above the mercury. The pressure was indicated by the rise of mercury in the longer arm and consequent compression of the air, the shorter arm being in communication with the retort. The escape was badly regulated by an ordinary stop-cock. The very unequal expansion of glass and iron prevented me from making a tight joint between the retort and U tube.

I next tried a small valve constructed like an ordinary safety-valve. I found it impossible with this valve to prevent a large amount of loss from escape of vapour around the spindle.

I next tried a loaded valve, the load of which was placed directly upon the spindle, the whole contained in a chamber resembling a miniature steam-chest, from which the vapours could only escape into the worm. It was found upon trial with the safety-valve that an orifice three-eighths of an inch in diameter was too large in proportion to the size of the retort, the vapours escaping in too large volume to admit of a continued flow from the worm. The vapour escaped in intermittent puffs, thereby causing an undulatory movement from the requisite amount of pressure to no pressure at all. As a consequence, the results rendered were very imperfect. To obviate this difficulty, I made the orifice beneath the valve only one-sixteenth of an inch in diameter, the surface of the orifice being to that of the retort as one to sixty thousand. This arrangement enabled me to secure a constant flow of vapour from the retort, to maintain a constant pressure, and to preserve a constant degree of temperature. I found by computation that a pressure of two ounces avoirdupois upon an orifice one-sixteenth of an inch in diameter was equivalent to a pressure of forty pounds to the square inch, yet when I placed a weight of two ounces upon the spindle, which of itself weighed half an ounce, the steam-gauge registered only ten pounds, and the oils passed through it unchanged in density. Although I employed one of the most skilful workers of brass in this city to grind the valve, I am satisfied that the fault was in the mechanical execution of the work, and that the bearing of the valve was upon the side of the

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* Report of C. M. Warren, Esq., contained in an article on Petroleum in California, by Prof. B. Silliman, National Intelligencer, Feb. 7th, 1866.

cone instead of at its apex, leaving a minute cavity beneath the valve. This fault could only be remedied by increased pressure. The chamber being too small to admit of placing the requisite weight upon the spindle, I made use of a spiral spring, the force of which was adjusted by an ordinary steam-gauge. By this means I was enabled to obtain the required pressure and to estimate its amount, with but one source of error, viz., the diminution in the elasticity of the spring incident to the high temperature of the vapours of the oil. I am convinced that the amount of this diminution is considerable; I have estimated it at one-fourth. The original elasticity returns, however, as soon as the spring is cold.

The following is a description of the valve as finally arranged. A piece of wrought iron gas-pipe one inch in diameter and three inches in length is bored out true, and an orifice drilled in its side one and one-fourth inches from the upper end, into which is brazed a piece of quarter inch gas-pipe about three inches in length. Both ends are now turned off and threads cut upon them, to which are carefully fitted strong brass caps. The upper caps should be about three-quarters of an inch in thickness, perforated two-thirds through from the inside with an eighth-inch drill, the orifice to serve as a guide to the upper end of the spindle. There should be a nipple three-fourths of an inch in length upon the lower cap, to connect it with the retort. The cap should be about one-half an inch in thickness, and with the nipple, should be perforated with a sixteenth inch drill. The seat of the valve should be excavated in the inside of the lower cap. A diaphragm should be placed within the iron tube, one inch from its lower end to serve as a guide for the spindle, through the centre of which the spindle should pass, while around it should be numerous small openings to allow for the free passage of the vapour. The valve itself should be turned upon the end of a spindle three-sixteenths of an inch in diameter, and carefully ground into its seat. The length of the spindle should be one-fourth of an inch less than the distance from the seat of the valve to the bottom of the orifice upon the inside of the upper cap, when both caps are in position. This allows the spindle to lift well, with sufficient room for the passage of the vapours. The diameter of the spindle should be reduced to one-eighth inch above the diaphragm. A spiral spring, of a diameter nearly equal to the interior of the pipe, made of brass wire about one-sixteenth of an inch in thickness, is so adjusted that the valve would be raised against the elastic force of the spring. This is effected by gradually reducing the diameter of the coils of the lower end of the spring to one-eighth inch, when it will just rest upon the shoulder upon the spindle. The upper coil of the spring should just touch the inside of the upper cap, when it is firmly screwed up. It will thus be seen that a force sufficient to cause the spring to contract one quarter of an inch is equal to a direct pressure upon the valve of two ounces. This depending for the same length of spring and size of wire upon the *number* of coils employed.

With this apparatus and the one described by Mr. Attfield, small quantities of every variety of bituminous and pyro-bituminous substances, may be subjected to treatment analogous to the most improved processes now in use upon the large scale. The results are reliable and admit of ready comparison. The cost of the retort with goose-neck and valve, made by the most skilful workmen, is about twenty-five dollars.

ON THE

PRODUCTION OF SOME NEW METALLIC SULPHOCYANIDES,

AND THE SEPARATION OF CERTAIN BASES FROM EACH OTHER BY THE METHOD THEREIN EMPLOYED.

BY WILLIAM SKEY,

Analyst to the Geological Survey, New Zealand.

The principle employed in the production of the following sulphocyanides is their great solubility in ether, by which not only can they be readily removed from their aqueous solution, but even their production in some instances determined.

Sulphocyanide of Cobalt.—If an alkaline sulphocyanide is added to an aqueous solution of a salt of cobalt, the colour thereof is merely browned, or, if ether is shook up with the cobalt salt, it remains colourless; but if ether is shook up with a mixture of the two salts, a blue colouration instantly results, which, on the subsidence of the water, is found to be confined to the ether. The etherial solution left to evaporate spontaneously, affords beautiful slender crystals of a dark blue colour, containing sulphocyanogen and cobalt.

If alcohol is substituted for ether, the solution of the mixed salt is also coloured blue. The blue colour is destroyed by acetate of soda, chloride of mercury, and hyposulphite of soda.

Sulphocyanide of Uranium.—If ether is agitated with a solution of chloride of uranium, it remains colourless, but the addition of sulphocyanide of potassium thereto determines the uranium to the ether on further agitation, after first markedly increasing the colour of the uranium solution. On examination, the whole of the metal is found in the ether united with the sulphocyanogen. If iron is present, the etherial solution will have a more or less red colour. In this case a deoxidizing agent is necessary to remove the iron, and thus manifest the colour proper to uranium.

Sulphocyanide of Molybdenum.—A solution of molybdic acid in hydrochloric acid is coloured a deep yellow by sulphocyanide of potassium, which colour is permanent in the air at common temperatures, while the addition of alcohol does not affect it; but instantly on contact with ether the colour rapidly darkens, and continues darkening till a deep red colour is attained. Also, if zinc is placed in a molybdic solution, to which a sulphocyanide has been added, together with excess of acid, the solution acquires a deep clear red colour, and ether removes a rich magenta coloured sulphocyanide of molybdenum, having a depth approaching to that of the iron compound with the same acid. Acetate of soda instantly discharges the colour.

Sulphocyanide of Tungsten.—This salt is best prepared by first treating a solution of tungstate of ammonia with the sulphocyanide. The granular precipitate thus produced is placed in contact with hydrochloric acid and ether. In a short time the ether acquires a yellow colour, and affords to the proper test good indication of both sulphocyanic acid and tungsten.

Sulphocyanide of Gold.—This sulphocyanide is very soluble in ether or alcohol, but scarcely dissolved by water. After taking great precaution to ensure the absence of iron, the colour of the etherial solution was red.

Sulphocyanide of Copper is also soluble in ether if an excess of hydrosulphocyanic acid is present, communicating to it a dark brown colour. It can be completely removed from water by this solvent.

The metals of the following oxides or chlorides I have not been able to combine with sulphocyanogen to form compounds soluble in ether:—Oxides of manganese, sesquioxide of aluminium, sesquioxide of chromium, bichloride of platinum.

In respect to the iron compound with sulphocyanogen, its solubility in ether has been already pointed out in a former paper; but since then it has been ascertained that its affinity for ether is so great that it can be entirely removed from water by this solvent, and its presence revealed where the eye fails to detect it. And, further, that ether determines in a remarkable manner the instant and abundant formation of this coloured compound from protochloride of iron and sulphocyanide of potassium even in presence of a soluble hyposulphite—a circumstance in no ways wholly accounted for by the presence of ozone in the ether employed, since but the faintest indication of this body could be obtained.

Separation of certain Bases from each other by the method here employed.—It is I think highly probable that the principle of the abstraction of certain of the metals from water by ether in presence of hydrosulphocyanic acid might be advantageously adopted for the separation of the following metals.

1. Iron from the alkaline earths—alumina, sesquioxide of chromium, oxides of manganese, also from uranium, platinum, and nickel.
2. Cobalt from nickel.
3. Gold from platinum.

It should be mentioned that for the separation of iron it is necessary to have the solution somewhat dilute, and for reasons which will presently appear, the solution must not be very acid.

The separation of cobalt from nickel has just been satisfactorily accomplished upon this principle.

ON THE
CONSTITUTION AND PROPERTIES
OF THE
HÆMATITE IRONS OF WEST CUMBERLAND.

BY EDMUND G. TOSH, PH.D.

The composition of five specimens of Cumberland Hæmatite pig iron, as determined by chemical analysis, is given below.

	I.	II.	III.	IV.	V.
Iron	93·552	93·100	92·850	92·798	92·802
Graphite	3·082	2·952	2·997	1·902	1·879
Combined carbon.	1·265	1·235	1·134	2·186	1·892
Silicium	1·389	2·286	2·706	2·714	2·753
Sulphur	0·068	0·075	0·068	0·065	0·164
Phosphorus	0·027	0·055	0·028	0·030	0·055
Manganese	0·216	0·288	0·140	0·140	0·288
Titanium	0·006	0·006	0·007	0·007	0·005
Nitrogen	0·056	0·041	0·051	0·051	0·049
Arsenic	trace	trace	trace	trace	trace
	99·661	100·038	99 981	99·893	99·887

I. II. and III. are from Cleator, Harrington, and Workington respectively, and were all manufactured specially for conversion into the best varieties of Bessemer steel. In appearance they were much alike: the fracture presented a bold crystalline structure, due to the graphite disseminated through the mass in large scales. IV. is a pig iron likewise intended for the production of steel, but on account of its containing too little free carbon or graphite, could not be successfully employed. Like the first three specimens, its fracture was highly graphitic, but in an inferior degree. V. is the analysis of ordinary grey forge iron made at Harrington.

Before examining the parts played by the various elements of pig iron in its conversion into steel, it is necessary to give a short outline of the Bessemer process.

Highly graphitic pig iron is melted and run into a large egg-shaped vessel of iron lined with some fire-resisting material, known technically as the "converter." In this vessel air is blown through the metal from beneath, the carbon and silicium are oxidised, and by their combustion produce a heat so intense, that in from 14 to 30 minutes, at the end of the process, the resulting almost pure *iron* at a dazzling white heat, may be poured out of the converting vessel in a stream almost as liquid as water. If *steel* be required, the necessary quantity of spiegeleisen (containing a known proportion of carbon) in a molten state, is added to the decarburized iron in the converter, previous to running the metal out into ingots.

If now, white, instead of grey iron be introduced into "the converter," and air blown through it, the heat does not increase, no carbon or silicium is burned. I have this upon authority of several influential steelmakers who have made practical experiments on the subject. This different bearing of white and grey iron is somewhat remarkable, as, up till the present time, it has been the opinion of metallurgists, if there was a definite one on the point, that the internal arrangement of the various kinds of cast iron was very similar when in the molten state. The phenomena here presented would seem to point directly to the conclusion that the two varieties of iron, grey and white, retain in the molten state, at least to a certain extent, the constitution they possessed when solid. Karsten states that graphite in fused graphitic iron, may exist either in chemical combination or in mechanical solution. The different deportment of grey, as distinguished from white iron, inclines us to the latter opinion or an approximation to it, for besides simple solution of graphite in the mass of metal, we may admit the existence of a high carbide of iron, necessarily a much more unstable compound than that existing in white iron, and the carbon of which, being in a somewhat weak state of combination, may combine more readily with the oxygen of the air at a high temperature. Further, either the graphite or this high carbide of iron may be dissolved in a second lower carbide, nearly resembling white iron in composition. Of course any premises as to the state in which carbon occurs in iron must be in a great measure hypothetical, on account of the extremely limited and imperfect nature of our knowledge of the carbides of iron. One conclusion we may draw with perfect safety I think, that the carbon at a temperature not far above the melting point, is very differently distributed in grey and white iron respectively; and if in the latter, the 4 or 5 per cent. of carbon is combined with about 95 per cent. of the metal, we are justified I think in inferring that a portion of the carbon in grey iron exists in one of the conditions I have mentioned as probable.

I here give an extract verbatim from a paper read by Bessemer before the Mechanical Engineers, in which the various changes that take place during the conversion of pig iron into steel are very clearly and fully described. "The silicium always present in greater or less quantities in pig iron is first attacked, and unites readily with the oxygen of the air, producing silicic acid; at the same time a small portion of the iron undergoes oxidation, and hence a fluid silicate of iron is produced, a little carbon being simultaneously burnt off. The heat is thus gradually increased until nearly the whole of the silicium is oxidised, which generally takes place in about 12 minutes from the commencement of the process. The carbon of the pig iron now begins to unite more freely with the oxygen of the air, producing at first a small flame which rapidly increases, and in about three minutes from its appearance, a most intense combustion is going on; the metal rises higher and higher in the vessel, sometimes occupying more than double its former space, and in this frothy fluid state it presents an enormous surface to the action of the air, which unites rapidly with the carbon contained in the crude iron, and produces a most intense combustion, the whole mass being in fact a perfect mixture of metal and fire. The carbon is now burnt off so rapidly as to produce a series of harmless explosions, throwing out the fluid slag in great quantities, while the combustion of the gases is so perfect that a voluminous white flame rushes from the mouth of the vessel, illuminating the whole of the building, and indicating to the practised eye the precise condition of the metal inside. The blowing may thus be left off whenever the number of minutes from the commencement, and the appearance of the flame, indicate the required quality of the metal. This is the mode preferred in working the process in Sweden, but at the works in Sheffield it is preferred to continue blowing the metal beyond this stage, until the flame suddenly drops, which it does on the approach of the metal to the condition of malleable iron; a small measured quantity of charcoal pig iron, containing a known proportion of carbon, is then added, and thus steel is produced of any degree of carburisation, the process having occupied about 23 minutes from the commencement. The converting vessel is tipped forward, and the blast shut off for adding this small charge of pig iron, after which the blast is turned on again for a few seconds."

"In the new process the carbon and silicium of the iron itself were employed as fuel to support the heat for reducing the cast iron, and the intense heat thus obtained, together with the intimate mixing of the air blown through the metal in a fluid state, caused the reduction to be much more rapid. Instead of the silicium in the iron requiring 2 or 3 hours to be burnt out as in the ordinary puddling process, it was now burnt out in only 12 minutes, giving out a great amount of heat by its combustion; and the complete reduction of the metal occupied less than half an hour, and was accomplished with far greater certainty and completeness, while 3 to 4 tons were acted upon at once instead of as many hundred-weights."

From this it would appear that silicium is first oxidised in the process of conversion, and that its combustion produces a great elevation of temperature. If this be correct, returning to the way white iron behaves in the converting vessel, it seems that not only the carbon, but the silicium too, is differently situated in white and grey pig iron in a state of fusion. An explanation of these various conditions and phenomena must at present be a matter of pure conjecture, as we are almost without information on this very important point. When the combustion of the graphitic carbon commences, the heat rises rapidly, chemical affinity between the iron and its constituents is so to speak weakened, the oxygen of the air is at liberty to act with greater effect, the carbon which existed in the crude iron in the combined condition is burnt, the last portions of silicium are oxidised, and good malleable iron results. If in the use of white iron a temperature could once be obtained high enough to lessen to such an extent the affinity between the metal and its carbon, as to allow the oxygen to take the latter, the conversion would succeed, but unless this elevated temperature could be imparted to it previous to its treatment with air in the converting vessel, its peculiar constitution renders this otherwise impossible. Lately in Styria, and, I believe, in certain parts of England, by a lengthened treatment in "the converter" with highly pressed air, pig irons resembling IV. (in the table) in composition, containing a small quantity of graphite, have been manufactured into an inferior kind of steel.

Chemists are generally of the opinion that the quality of Bessemer steel is strongly influenced by the amount of silicium contained in the raw iron from which it was derived, and Phipson* sought to prove that in pig irons which may be successfully used in the making of steel, the silicium must, like the carbon, exist in the free state. The means by which he arrived at this conclusion were somewhat peculiar, and I was induced to make some experiments on the subject, an account of which I published. Phipson† had examined three varieties of iron, A, B and C, in which he found the silicium to exist as under:—

	A	B	C
Combined silicium or Siα...	98	1·81	2·60
Graphitoidal silicium or Siβ.	3·22	2·15	1·63
	4·20	3·96	4·23

Because A contained its silicium principally in the free state, and C for the most part combined, they yielded respectively good and bad steels. In order to detect this graphitoidal silicium, I dissolved about 20 grammes of iron I. (in the table of analyses, which ranks highest among steel makers), in dilute hydrochloric acid, collected the insoluble matter, and burned it in oxygen. The residue was evaporated to dryness twice with hydrofluoric acid to remove silica, and afterwards heated with strong hydrochloric acid to remove a small quantity of oxide of iron. After this treatment I obtained a very small quantity of a light brown substance which displayed none of the physical properties of graphitoidal silicium under the microscope, but which proved to be titanic acid. Had free silicium been present I should have detected it.

To estimate the two modifications of silicium, Dr. Phipson treated a weighed quantity of the iron with aqua regia, the resulting silica from Siα was dissolved by the acid, while that from Siβ was insoluble. It is a well established scientific fact, that any variety of silicium which has once been exposed to a red heat (as Siβ in iron must certainly have been) is quite unaffected by aqua regia—enough in itself to show how little this process is to be relied on.

To see if the amount of insoluble silica was a constant quantity I made the following three experiments.

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* *Comptes Rendus*, t. lx. p. 1030.

† Chemical News, No. 330. (*Eng. Ed.*)

I. 2·409 grms. of iron dissolved in aqua regia ($3HCl + NO_6H$) gave 0·0565 grm. SiO_2=1·094 per cent. silicium.

II. 2·39575 grms. of iron in a large excess of aqua regia gave 0·038 grm. SiO_2=0·74 per cent. silicium.

III. 2·336 grms. of iron, dissolved in aqua regia, and most of the acid carefully boiled off before filtering, gave, 0·06775 grm. SiO_2=1·353 per cent. silicium.

As might be expected, the amount of insoluble silica varies with the quantity of acid employed; thus in II., where much acid was used, the quantity of silica is only about ½ of that in III., where, previous to filtration, most of the acid was boiled off.

In what I took as a reply to my communication of these experiments, Dr. Phipson[*] stated that what he at first looked upon as free silicium in pig iron, occurred, he had more recently found, as silicic acid combined with protoxide of iron. Now if we are to consider the 3·22 of Siβ (given in Dr. Phipson's estimations), as present in the state of silicic acid saturated with protoxide of iron, we arrive at the astounding discovery that this iron contains 23·46 per cent. of slag. Few chemists would accept this statement without strong evidences of its truth. To ascertain whether this position were tenable, I heated 3 to 4 grammes of Cleator iron (I.) in a stream of perfectly dry chlorine; all iron and silicium are volatilised as chlorides, while silica, if present, remains unaffected. The carbon burnt off in oxygen. I obtained a small residue, mostly titanic acid, weighing only 1 or 2 milligrammes, equal to 0·3 per cent., instead of 6 or 7 per cent., as Dr. Phipson would show.

From his experiments, by a course of reasoning upon which we have no enlightenment, Dr. P. infers that only Siα, and not Siβ, exerts a deleterious action in the conversion of iron into steel. If, however, we have shown his assumption that Siβ exists in pig iron is groundless, no correct conclusions can be drawn from the data which he has given, and until our information is more definite, the subject must remain where it is.

The presence of graphitoidal silicium in iron, though believed possible by many metallurgists, has, as far as I know, been never clearly shown. Percy[†] thinks its existence highly probable, and Büchner[‡] makes a statement to the same effect, but neither seem to have made any special experiments on the subject. Hahn[§] prepared a silicide of iron containing as much as 20·29 per cent. silicium. By treatment of this substance with hydrofluoric acid, a small crystalline residue remained undissolved, which proved to be a definite compound $FeSi_2$. Even with this enormous percentage of silicium none of that element separated in the free state. In the preparation of this compound only pure materials of known composition were used, and it may be urged that in the presence of the numerous substances which make up the constitution of crude iron, its behaviour might be modified. Caron,[‖] a high authority in these matters, distinctly states that silicium never can exist in the uncombined state in iron, and that on account of its superior affinity it expels most other elements from combination.

Although altogether disagreeing with Dr. Phipson's conclusions, or more exactly with the way in which he arrives at them, I believe it quite possible that silicium may exist variously combined in pig iron, and as it occurs in one manner or another (as yet undefined) may exert a more or less deleterious action upon the steel made from it. I look upon it nevertheless, as a fact, that an iron tolerably rich in silicium, may be converted into good steel, although it contains neither graphitoidal silicium nor silicate of protoxide of iron.

As I mentioned in the earlier part of my paper, the quantities of sulphur and phosphorus, particularly the latter, in iron, are not very materially lessened by its conversion into steel by the Bessemer process, hence only those varieties of crude iron can be used which contain an exceedingly small proportion of these elements, otherwise the resulting steel would be of very inferior quality and possess the properties of cold and red-shortness in an objectionable degree. This is a drawback to the very general application of the Bessemer process, as only in exceptional cases are irons of sufficient purity to be met with. Numerous attempts have been made to remove these substances, and many patents have been taken out with the same object, but as yet none have been successful. Calvert recommended the use of common salt, but its volatility at the extraordinarily high temperature of the metal while undergoing decarburization was an impediment to its employment. Recently a patent has been taken out by Wintzer, a Hanoverian ironmaster, for the use of chloride of calcium for the removal of these objectionable substances. If chloride of calcium have the effect desired, a great obstacle will have been overcome, but I have not heard of any experiments having been made with it.

I have estimated sulphur, phosphorus, and silicium in a specimen of steel made from Harrington pig iron (II. in the table). The percentage amounts in the iron and steel respectively are as follows:—

	Iron	Steel
Sulphur	0·075	0·034
Phosphorus	0·055	0·046
Silicium	2·286	0·172

Red-shortness.—I have already observed that malleable bars made from hæmatite pig iron by the ordinary process of puddling, are so exceedingly red-short as to be almost useless. A piece of this iron at a dull red-heat is so brittle that it breaks into fragments after a few blows under the hammer. This most remarkable and undesirable property can not in the present instance be ascribed, as it usually is, to the presence of an excessive quantity of sulphur, for even the raw hæmatite iron contains a less proportion of that element than the refined malleable bars of the best Swedish and English makes, such as Dannemora and Lowmoor; and it is very unlikely that the amount of sulphur should increase by puddling.

The Cumberland pig irons are as a class rich in silicium, and on the other hand contain very little manganese, and I am inclined to think that these co-existing peculiarities of constitution point towards the true cause of red-shortness in the present case. Caron[*] made the very interesting observation that at a high temperature in an oxidising atmosphere, manganese possessed the power of removing silicium from iron. This property at once assigns to manganese a place of high importance in connection with many operations in the metallurgy of iron, particularly that of puddling. Until spiegeleisen began to be used in making Bessemer steel, and its beneficial influence forced itself to be

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[*] *Comptes Rendus*, t. lxii. p. 803.

[†] "Metall. Iron and Steel."

[‡] *Journ f. prakt. Chemie*, bd. 72. p. 364.

[§] *Ann. d. Chem. u. Pharm*, cxxix. p. 57.

[‖] Memoire sur les Aciers."

[*] "Memoire sur les Aciers."

[English Edition, Vol. XVI., No. 411, page 203; No. 412, pages 213, 214.]

recognised, the action of manganese was greatly ignored by many practical men, at least in Britain. Any literature we have on this neglected subject is, for the most part, of an exceedingly vague description: the determination of manganese in iron by commercial analysts is often looked upon as a matter of minor importance, hence we have no good grounds to go upon in seeking to understand the action of this substance. It is, however, highly probable that in the removal of silicium by puddling, manganese plays a very important part, and looking upon the small proportion of this metal in hæmatite pig iron, in forcible contrast with the large amount of silicium, it is very possible that a comparatively large percentage of the latter element remains combined with the iron, exerting upon it a most deleterious effect, and causing the red-shortness in question. Unfortunately I have not a specimen of the malleable iron at my disposal, otherwise I might test, by chemical analysis, the value of these remarks.

Titanium.—This substance exists in small quantities in all the hæmatite irons I have examined, very probably combined with cyanogen and nitrogen, forming cyano-nitride of titanium, the composition of which was first shown by the classical researches of Wöhler.[*] Although the effect of its presence in iron is not distinctly known, the prevailing opinion among chemists and metallurgists is, that the cyanogen compound being probably merely diffused through the metal, and not chemically combined with it, its influence is very small. In repairing the blast furnaces in the Cumberland district, very considerable quantities of this cyano-nitride of titanium are found in the vicinity of the hearths, generally in the irregularly shaped lumps which though black and dull on the outside, exhibit when broken an exceedingly beautiful copper-coloured mass of crystals, among which a little iron is diffused. All crystals I have seen were octahedral, and although I have tried several times to obtain a perfect octahedron by dissolving away the surrounding iron with dilute hydrochloric acid, I have not succeeded. The faces of the crystals are rough and deeply striated, resembling occasionally the skeleton forms presented by common salt.

I examined the blast furnace slag for titanic acid, and found that it contained a mere trace.

Nitrogen, as it occurs in iron, stands in close relation with titanium. According to Caron[†] it is to be found to a greater or less extent in all commercial iron, from which it can only with the greatest difficulty be perfectly removed. The proportions of the titanium and nitrogen in these hæmatite irons, as determined by me, are not by any means those which are found in cyano-nitride of titanium, but where quantities are so small the unavoidable errors of analysis may account for any such difference.

Although somewhat out of place, I would here make some remarks upon a specimen of slag from one of the Cumberland blast furnaces, which when analysed was shown to have the following composition:—

		Oxygen.
Silica..............	30·200	16·11
Alumina...........	12·007	21·58
Protoxide of Iron...	1·269	
Lime	50·507	
Magnesia......	2·553	
Alkalies...........	1·225	
Sulphide of calcium...	2·400	
Phosphoric acid......	trace	
Titanic "	trace	
Protoxide of manganese	trace	
	100·173	

The percentage of silica is strikingly low, and if it be calculated it is found that the bases are very considerably in excess, the relation of oxygen being about 8 in silica to 10 in the bases.

I have often heard of the loss some of the companies in this district experience from the rapid wearing out of the furnace lining; sometimes after only three months working, or even less, the fire bricks were quite burned through. If the specimen analysed represents the slags usually produced, and it is not a very exceptional case, this destruction of the furnace lining is easily explained. If unneutralised bases cannot find silicic acid in the ores to combine with, they will seek it elsewhere, and coming in contact with the firebrick lining will at once attack the silica in it, causing this disastrously rapid wear and tear of which I have heard so many complaints. In order to remedy this evil, furnace managers must either use less limestone or a larger quantity of siliceous matter.[*]

This slag was of very vesicular and open structure, and of a greyish colour, agreeing closely with another described by Percy,[*] which he looked upon as a highly basic silicate, a conjecture fully confirmed by my analysis.

The Bessemer process, as yet in its infancy, presents a most interesting subject for study and examination. The chemical reactions involved in the process are still imperfectly understood, and a careful series of observations of the phenomena which are presented during its performance, while leading to a correct knowledge of the changes which take place during the conversion, would I have no doubt give us much more correct and reliable views as to the internal constitution of raw iron than any which we hitherto possess.

A PRELIMINARY NOTICE OF

THE AKAZGA ORDEAL OF WEST AFRICA,

AND OF ITS ACTIVE PRINCIPLE.

BY THOMAS R. FRASER, M.D.

This ordeal poison is referred to in the works of Du Chaillu[†] and Winwood Reade;[‡] and several of its toxic properties have been described by MM. Pecholier et Saintpierre.[‡] A few specimens were sent to this country in 1864 by the Rev. A. Bushnell of Baraka, and these were very kindly given to the author by Mr. Thomson of Glasgow; and a further supply came from the same quarter in 1865. These gentlemen, and Dr. Nassau of Bonita, supplied valuable and interesting information regarding its employment.

The poison is known in Africa as Akazga, Boundon (or M'Boundou) Ikaja, and Quai; Akazga being probably derived from *nkazga*, which signifies pain or hurt. It is used as an ordeal for the detection of real and superstitious crimes on the West Coast of Africa, in a large district which extends north and south of the equator, and many miles inland, and also in the adjacent island of Corsica. It is believed that several

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[*] *Ann. d Chem. u. Pharm.* lxxiii. p. 33.

[†] Memoire sur les Aciers.

[*] "Metall. Iron and Steel," p. 506.

[†] Explorations and Adventures in Equatorial Africa, 1861.

[‡] Savage Africa, 1862. [§] *Comptes Rendus*, 1866, p. 809.

thousand persons are annually subjected to this method of trial, and that the fatal cases are about 50 per cent.

The Akazga arrived in bundles, which consisted of long, slender, and crooked stems, having their roots generally attached to them, but sometimes their leaf-bearing branches only, and containing also a very few complete plants, with roots, stem, and branches. The plant is usually about six feet in length; but some specimens were only four, and others as long as eight feet. The bark is yellowish orange, and in some parts light red, and it is frequently covered with a gray efflorescence. It adheres firmly to the stem, but may be readily detached, after exposure to a gentle heat for some days. Its internal surface is light brown. The space between the bark and the wood was found, in a few pieces, to be occupied by a large number of minute sparkling crystals; but it has not yet been determined whether these consist of a vegetable or mineral substance. The leaves are opposite and oval-acuminate in form; the apex frequently consisting of a linear prolongation more than an inch in length. From its general characters, the plant is supposed to belong to the Loganiaceæ, but the materials are insufficient to identify it.

By boiling the powdered bark with alcohol of 85 per cent. and distilling and evaporating the tincture, a brown shining extract is procured, weighing from 12 to 15 per cent. of the bark employed. It has a bitter, non-persistent taste, and when treated with concentrated nitric acid, produces a brownish-yellow colour, which is not materially affected by heat, nor by solution of proto-chloride of tin. It is obvious that the active principle of Akazga is contained in this extract; and to separate it the following method has been adopted, after several attempts at various processes:—The extract is treated with a very dilute solution of tartaric acid, which removes 77 per cent. and filtered. The clear, yellowish-brown acid solution is shaken with successive portions of ether, so long as any colour is removed; and by this means also a small quantity of an aromatic oil is separated from it. After decantation, a solution of carbonate of sodium is added to the liquor, so long as it causes a nearly colourless, flocculent precipitate. It is again shaken with ether, which is decanted, and agitated with three successive portions of distilled water, and finally received in a bottle containing a dilute solution of tartaric acid, and shaken with it. As soon as the etherial solution is brought in contact with the acid, it becomes opalescent, but again assumes its normal appearance on agitation. This change is of some value, as indicating the frequency with which the alkaline solution should be treated with ether, as the milkiness, on contact with tartaric acid, is not produced when the former is exhausted. On reaching this stage the tartaric solution is exposed to a gentle heat—to free it completely from ether—filtered, and again treated with carbonate of sodium, by means of which a bulky, colourless, and flocculent precipitate is obtained. This is collected in a filter, washed, and dried, by exposure to a gentle heat for a short time, and then by the action of sulphuric acid *in vacuo*.

By this means a colourless, amorphous substance is obtained, which is the active principle of the Akazga poison, and which possesses the general properties of a vegetable alkaloid. About 10 grains may be separated from 500 grains of the powdered stem-bark, or 2 per cent. *Akazgia* is proposed as its name; and it is hoped that when the plant is described, if it has been previously unknown to science, Akazga will be adopted as its specific name, and thus the usual connection of nomenclature between the vegetable alkaloid and its source will be maintained.

Akazgia is soluble in about 60 parts of cold absolute alcohol; in about 16 parts of spirit, of 85 per cent.; in about 120 parts of anhydrous sulphuric ether; and in 13,000 parts of distilled water, at a temperature of 60° F. It is freely soluble in chloroform, in bisulphide of carbon, in benzole, and in sulphuric ether of specific gravity 0·735. It crystallises with difficulty, but it may be obtained in the form of minute prisms, by the slow evaporation of a solution in rectified spirit. An analysis of its platinum-salt, and a determination of its combining proportion with dry hydrochloric acid, yielded 290 in the former, and 293 in the latter, as the equivalent of Akazgia. When heated it becomes yellow, then melts, and gives off fumes of a pungent, disagreeable odour, and finally becomes charred, but leaves almost no residue if the heat be continued for a sufficient time. Its solutions have an alkaline reaction, and neutralise acids; and the salts are freely soluble in water, and have a very bitter, non-persistent taste. Concentrated nitric, hydrochloric, and sulphuric acids change its colour to brown, but these in a diluted state, as well as many of the organic acids, form pale, yellowish solutions with Akazgia. It is precipitated from these solutions by hydrate, carbonate, and bicarbonate of sodium, and of potassium; by iodide, sulphocyanate, ferrocyanide, and chromate of potassium; by phosphate of sodium, proto-chloride of tin, trichloride of gold, dichloride of platinum, potassio-mercuric-iodide, carbazotic acid, tincture of galls, solution of iodine, and various other substances, *but these precipitates are never crystalline.* Corrosive sublimate causes an amorphous white precipitate, which is dissolved by heat, and reappears in a non-crystalline form when the solution has cooled. Chlorine produces an amorphous, colourless precipitate, which does not disappear on the addition of ammonia. With concentrated sulphuric acid, and peroxide of manganese, bichromate of potassium, or any other of the usual oxidising agents, the same succession of colours is produced, from blue to brown, which results from a similar treatment of strychnia.

The alcoholic extract of Akazga possesses physiological properties very similar to those of nux vomica; and comparative experiments were detailed, to show that the principle, Akazgia, has exactly the same actions as the extract, and a proportional activity to it.

There are several instances in which a Natural Order produces several very similar active principles. In the Longaniaceæ itself, strychnia, brucia, and igasuria already exist, and these are nearly identical in their physiological actions. In chemical properties, brucia and igasuria have much in common, and they are both readily distinguishable, in this respect, from strychnia. Akazgia conveniently completes this group, as its chemical properties are nearly allied to those of strychnia, whilst its connection with all the members is maintained by the similarity of its physiological actions.—*Proceedings of the Royal Society of Edinburgh. Session* 1866-67.

Volatility of Sesquichloride of Iron at Common Temperatures.—When sesquichloride of iron is rendered very acid by hydrochloric acid, the vapour therefrom colours a solution of sulphocyanide of potassium faintly red, when allowed to impinge upon it for a short time.—*William Skey, New Zealand.*

ON THE APPLICATION OF THE

BLOWPIPE TO THE QUANTITATIVE DETERMINATION OR ASSAY OF CERTAIN METALS.

BY DAVID FORBES, F.R.S., ETC.

(Continued from American Reprint, Sept. 1867, p. 111.)

Silver Assay—

B. Metallic Alloys incapable of direct Cupellation.

b. Containing tin: argentiferous tin, bronze, bell and gun metal, bronze coinage, etc.

Alloys of silver with other metals containing tin do not admit of being cupelled, since the oxide of tin formed by the oxidation of that metal is not absorbed by the bone ash of the cupel along with the litharge; it consequently remains upon the surface of the cupel, and if present in any quantity interferes with the operation. As tin is not volatile when heated on charcoal either in the oxidating or reducing blowpipe flame, it cannot be so dissipated, and in consequence, the entire amount of tin contained in any alloy under examination must be removed by oxidation or scorification from the silver lead, previous to its being submitted to cupellation.

For this purpose: 1 part of the stanniferous alloy is fluxed with from 5 to 15 parts granulated assay lead (according to the amount of copper suspected to be present in the alloy), 0·5 part anhydrous carbonate of soda, and 0·5 part pulverized borax glass, made up as usual in a soda paper cornet, and the whole at first gently heated in reduction flame, until the soda paper is charred and the alloy has afterwards united with the lead to form a single globule, whilst the borax and soda have combined as a glass or slag in which the soda prevents the easily oxidisable tin becoming oxidised to any extent before a perfect alloy has been formed with the lead, which then contains the whole of the silver.

As soon as this is effected, the blowpipe flame is altered to an oxidating one, and the metallic globule is kept at the point of the blue flame, which should touch it so as to cause the tin to become oxidised and be at once taken up by the glass surrounding it.

Should, however, it be seen that minute globules of metallic tin made their appearance on the outer edge of the slag or glass,* the operation must be at once discontinued, and the assay allowed to cool; after cooling the metallic globule is detached from the slag surrounding it, and being placed in a cavity on charcoal, is fused in the reducing flame along with a small piece of borax glass and afterwards treated with the oxidating flame exactly as before (and if necessary, which is seldom the case, unless when treating argentiferous block tin, this operation may again require to be repeated), until it is seen that the surface of the metallic silver lead globule does not any longer become covered with a crust or scales of oxide of tin, but presents a pure and bright metallic surface.

The silver lead globule is now quite free from tin, and can be cupelled and the amount of silver determined as usual.

c. Metallic alloys containing much antimony, tellurium, or zinc: antimonial silver and argentiferous antimony, telluric silver and argentiferous zinc.

Alloys of antimony with silver when treated on charcoal in the oxidating flame give off all their antimony, leaving the silver behind as a metallic globule having a frosted external appearance; telluric silver, on the contrary however, when treated in similar manner only evolves a part of its tellurium, and even after cupellation with lead a small amount of tellurium generally remains behind alloyed with the silver.

All these compounds may be assayed as follows:—

One part of the alloy is placed in a soda paper cornet along with 5 parts granulated assay lead, and 0·5 part pulverised borax glass, and fused in reducing flame until the globule and slag are well developed; the oxidating flame is now directed on to the globule, causing the whole of the zinc along with most of the antimony and part of the tellurium to volatilise before the lead commences oxidising. The last traces of antimony are removed with some difficulty, during which operation some portion of the lead becomes oxidised. On cooling, the globule is now separated from the slag and concentrated upon a coarse bone ash cupel as usual, and if no tellurium were present in the concentrated silver lead, this may now be cupelled as usual.

If tellurium is present, as is seen by the concentrated globule of silver lead possessing a dark coloured exterior, it must be remelted with 5 parts assay lead and again concentrated, and these operations, if necessary, must be repeated until the surface of the concentrated globule is found to be clean and bright as usual with pure silver lead, when it may be cupelled fine and the silver globule weighed.

It sometimes happens, even after all these precautions have been taken, that the silver globule after cupellation shows a crystalline, greyish white, frosted appearance from its still containing tellurium; in such cases its own weight of assay lead (in one piece) should be placed beside it on the cupel, melted together, and the globule again cupelled fine on another part of the surface of the same cupel. In assaying substances very rich in tellurium the results obtained are, however, not very satisfactory, and may be as much as one or two per cent. too low, even after employing all precautions.

d. Compounds of silver with mercury; arquerite, native and artificial amalgams and argentiferous mercury.

The assay of these compounds is very simple. A weighed quantity of the liquid or solid amalgam is placed in a small bulb tube, and heated over the lamp very gradually in order to avoid spirting and to allow the mercury to volatilise quietly*; the heat is increased by degrees as long as any mercury is driven off, and the residue is heated for some time at a red heat in order to drive off as much mercury as possible without fusing the glass or causing the residual silver to adhere to it. The mercury expelled condenses itself above the bulb on to the upper part of the tube, and by gently tapping will collect in globules, which by carefully turning the tube unite and can be poured out of the tube; after which the silver, left behind as a porous mass, may be removed from the tube, and after being fluxed with an equal weight of granulated assay lead and half its weight of borax glass, must be fused on charcoal in the reducing flame, and the button on cooling cupelled as usual. Should, however, much copper have been present in the amalgam, a propor-

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* This occurs when the flux has become so saturated with oxide of tin that it cannot take up any more.

* In the case of solid amalgams, which often spirt very violently, this may be obviated by wrapping the assay in a small piece of tissue paper, and heating it in a blow-pipe crucible, when all the mercury is given off quietly, leaving the silver behind; a useful little dodge shown the author lately by Mr. Crookes.

tionately larger amount of assay lead is required to be added.

When the argentiferous residue is extremely small, as is often the case when assaying argentiferous mercury, this may adhere firmly to the glass of the tube. On such occasions this part of the tube must be cut off with the adherent residue, and the whole fused in a strong reducing flame along with its own weight of granulated assay lead, and with half its weight of anhydrous carbonate of soda. Upon cooling, the globule of silver lead thus obtained is cupelled as usual.

(c.) Compounds chiefly consisting of iron; argentiferous-steel; cast-iron; bears from smelting furnace.

Compounds consisting principally of iron with a small percentage of silver, although occasionally produced in the arts intentionally, as, for example, the so-called silver steel, are commonly found on the blowing out of furnaces used in the smelting of silver and copper ores, and are frequently rich in silver, as is the case with the bears from the silver furnaces at Kongsberg in Norway. An alloy of iron with silver is occasionally also found appearing in small quantities on the surface of melted silver in the process of casting, and in some cases at least this may be due to the action of the melted silver on the iron rods used for stirring up the molten metal.

As iron cannot be made to alloy itself with lead before the blowpipe, it becomes necessary to extract the silver by a more indirect process than is used in the case of other alloys containing that metal. In order to remove the iron the alloy must first be converted into sulphide of iron and silver, and to effect this, the iron or steel must be reduced to powder, or fragments none greater than about a quarter of a grain in weight; for which purpose steel when hardened may require to be softened previously.

One part of the finely-divided iron or steel is now mixed with 0·75 part sulphur, eight parts granulated assay lead, and one part pulverised borax-glass; the mixture after being placed in a soda paper cornet is carefully fused in a cavity on charcoal in the reducing flame, until the whole appears as a fluid globule containing both the lead and iron in combination with the sulphur. Without removing either this globule or the glass surrounding it from the charcoal, an amount of borax glass in one or more fragments (in all about equal in weight to the original amount of iron employed), is now added (in order to combine with and slag off the whole of the iron), and fused along with the former globule, after which the whole is submitted to a strong oxidating flame until the impure lead globule shows itself protruding from the slag.

The charcoal is then inclined so that the lead is alone subjected to the action of the outer flame, in order to volatilise the sulphur, and at same time oxidise the iron which goes into the slag; this operation is continued until the globule of lead appears with a bright metallic surface; should it on cooling, however, be found to possess a black colour, and to be brittle, it must be still further oxidised as before described.

The silver lead thus obtained will now be found to contain all the silver, and at the same time to be free from both iron and sulphur, and can be cupelled as usual.

No notice is here taken of alloys of silver and gold, since these metals cannot be separated before the blowpipe by any process yet known; and in all cases where gold may be present in an alloy, treated as here directed for obtaining its contents in silver, the gold also will be found to follow along with the silver, and must be parted from that metal by the humid method, in order to enable the true amount of silver present in the substance to be ascertained.

ON A NEW TEST FOR HYPOSULPHITES.

BY M. CAREY LEA.

In an examination of the platinum metals which I published some time back in this journal, I described a very delicate test for ruthenium, by which the faintest traces of that metal could be detected through the agency of hyposulphite of soda. Recently, having occasion to test for the last-named substance, it occurred to me as probable that ruthenium might be rendered available for that purpose. This I found to be the case, and that the reaction exhibited considerable delicacy. It is true that ruthenium is at present a very rare metal, and not within the reach of all who might wish to use it, but the changes from rarity to more or less abundance are now so common and sudden that present scarcity is no reason for ignoring any useful reagent.

When a solution of ruthenium is rendered alkaline by ammonia and boiled with hyposulphite of soda, it gradually assumes a rose colour which passes into a rich carmine; with strong solution the color is so intense as to be almost black. When diluted the shade is magnificent, rivalling the aniline red in richness.

I have already stated within what limits ruthenium can be detected by hyposulphite of soda. I now subjoin the limits observed with respect to hyposulphite of soda.

A solution containing one four-thousandth of hyposulphite, gave a clear rose red.

One containing one twelve-thousandth gave a well marked pink fluid.

One containing one twenty-five-thousandth gave a salmon colour.

The experiment was not carried further because the salmon colour in the last-mentioned trial showed that the test had then reached its practical limit. I do not doubt that even with one hundred-thousandth a colouration could be obtained, but it would not have the specific distinctness given by the carmine and rose shade previously described.

A few words remain to be said as to the best mode of applying this test.

I have recognised in solutions of sesquioxide of ruthenium a strong tendency to decompose by dilution; dilute solutions have a strong tendency to gradually deposit their ruthenium as oxide. And even before the slightest sign of a precipitate appears, in fact immediately upon dilution, solutions show a tendency to change their reactions. So that I find it invariably better on diluting the ruthenium solution for use in testing, to boil it (as I have elsewhere pointed out in speaking of the dilution of ruthenium) with a few drops of hydrochloric acid, and this even although the solution is to be immediately afterwards rendered alkaline by ammonia. To ascertain with certainty that this improved the delicacy of the reaction, I made comparative experiments on two portions of the same ruthenium solution, and found that the colouration by hyposulphite was at least three times stronger in the case of the portion that had been boiled with HCl than with that that had not.

As ammonia was thereafter immediately added, it might appear that the function of the hydrochloric acid was to form hydrochlorate of ammonia. But it was found by experiment that the addition of sal-ammoniac in no way aided the reaction.

The addition of ammonia to a hot solution of sesquichloride of ruthenium immediately darkens it to a blackish olive colour, which, according to the dilution and the light that falls on it, is of a reddish or a greenish shade. By standing, the ruthenium is precipitated as oxide. As this condition is the necessary preliminary (as before explained) to the production of the characteristic carmine reaction, it is not a little singular that the delicacy of that reaction should be so greatly enhanced by taking steps to strengthen the combination with excess of acid and boiling, immediately before the affinities are to be loosened by ammonia.

In using this reaction for the detection of small quantities of hyposulphite, it is useful to remark that it succeeds best when very little ruthenium is present. After the ruthenium solution has been boiled with acid and supersaturated with ammonia and the liquid to be tested for hyposulphite added, the mixed solution should have so little ruthenium in it as to exhibit only a very pale transparent olive colouration—should in fact be almost without colour. Otherwise if the hyposulphite is present in mere traces, we get a salmon or flame colour instead of the pure carmine.—*American Journal of Science*, September, 1867.

PRACTICAL HINTS TO THE STUDENT.*

BY WILLIAM ALLEN MILLER, M.D., LL.D., V.P.R.S.

Professor of Chemistry in King's College.

I SHALL best consult the wishes of those whom I represent, and shall be doing what is most fitting upon such an occasion, if I aim at usefulness rather than novelty; and for that purpose I shall direct my remarks chiefly to those who are just commencing their career among us, and who may perhaps not unnaturally feel some degree of perplexity and apprehension at the formidable array of studies to which they are now at once introduced as a preliminary to the practice of their profession.

It will not be sufficient for any one of you, however diligent, to content himself with mere attendance upon lectures. Admirable as these may be, they can only present an outline of the subject, which the student must fill up by reading and reflection. No man can really do the work of thinking for another, if that other is to be anything more than a cypher. The most important part of every man's education is that which he gives to himself. He must learn to master his own mind, and to conquer the tendency which every one naturally has to prefer ease to persevering work. When once the habit of steady application has been acquired, all others are comparatively easy.

In preparing yourselves for the practice of your profession, your great object must be to seize upon the principles of each branch of your studies; and it is here that good lectures are of such value in directing the mind of the student. Do not suppose, however, that I wish you to undervalue the acquisition of even very minute details in certain cases. Details indeed are not to be despised or neglected, for it is upon the mastery of detail that all successful practice depends. All acquired knowledge, to be valuable, must be precise as far as it goes: but the selection of those points that must be filled up minutely, and the omission of details where the knowledge of the principle only will suffice in others, are essential conditions to success in study; and in such cases a hint obtained from the Professor may often save you many an hour of profitless labour. For example—the custom of taking notes during lectures is one which may be beneficial or injurious, according to the mode in which it is carried out. If judiciously managed, it may be of great value. Unless, however, you are a master of shorthand it would be a mistake to endeavour to take down all that is said. The great value of notes of lectures will be to guide you in your subsequent reading; but since most of the details given in systematic courses of lectures will be found in the text-books upon the subject, it would be waste of effort to do more than preserve the heads and main divisions of the discourse. If more be attempted, the attention is in danger of being distracted by the mechanical effort of writing, and the drift of the argument of being lost in consequence. It will often be useful to take down references to books, numerical details, and special information of any kind. If it is an experimental lecture, a list of the illustrations employed may be preserved; whilst a sketch of any particular piece of apparatus, or of the arrangement of an experiment, will frequently both save a long description and recall the whole more vividly to the memory. In short, good notes to a lecture are like an index-map to an intricate country, upon which a few of the leading rivers, mountains, and cities are clearly marked out.

Every student who aspires to distinguish himself—and who is there among you that does not?—must set apart methodically certain portions of the day for study. Four or five hours a day spent in real study, in addition to the time occupied in the class-room, in dissecting, and at the hospital, will be as much as will be profitable to most men. The mental food, like the food for the body, must be digested and assimilated, otherwise it will not become part of the mind, nor will it be available for use.

This systematic and orderly arrangement of your studies will be greatly facilitated by the custom of drawing up every evening a plan for your work on the following day; it will preserve you from indecision, and will save time as you pass from one pursuit to another in its due order. You will then also be in less danger of falling into the habit of procrastination, which so often ruins a promising character. If you know that a thing which must be done can be done at once, do not postpone it. The recollection of the duty will either hang uneasily over you, and rob you of your repose, or else you will become indifferent; the habit of delay will be confirmed, and your power over yourself will be weakened.

The first requisite to successful study is the concentration of the powers of the mind upon the subject in hand; and this concentration of the faculties, though a voluntary act, is difficult at first to accomplish, but it gradually becomes easier by repeated practice. A well-trained mind will find that this process will afford invaluable aid to the memory, even when not naturally retentive, whilst it will enable one endowed with a really strong memory to acquire a knowledge, at once ready and accurate, of any subject which he may select.

We often hear complaints of memory on the part of those who really ought to blame themselves for want of attention. The same man who complains that his mem-

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* Extracts from an Introductory Lecture at the opening of the Medical Session at King's College, London, October 1, 1867.

ory is so bad that he forgets what he has read as soon as he has closed the book, will, nevertheless, often give you the particulars of a boat-race, or of a game at cricket, or of football, in which he was himself personally interested, with the minute details of every incident, showing no want of memory in this case, where his bodily and mental powers were called into full activity.

The mind must be directed to the subject for a certain time with a view to remembering it, and the idea must be strengthened by repetition. Systematic repetition or review of the leading features of the subject under study should never be neglected. It is irksome, but indispensable. This habit of directing the mind intensely to whatever comes before it in reading or observation should therefore be cultivated by all means in your power, and the opposite habit of listless inactivity should be carefully guarded against, for in this lies the foundation of a sound intellectual character.

Next to attention, there is nothing that affords so important an aid to the memory as the habit of associating ideas correctly with each other. The constant practice of tracing the relation between new facts and those already acquired; the custom of referring facts to the principles which they confirm, illustrate, or extend, is of the utmost value; since it not only fixes the new facts firmly in the memory, but it refers them to their proper place in the mind, thereby enabling you to recall them in connection with the subject itself to which they relate. This mental operation is most important to prevent confusion of mind. Indeed, it is not less necessary than the corresponding mechanical process of arranging one's papers in which every one at once feels the importance of separating those relating to different subjects, while those referred to allied ones are placed together, each series being indicated by its appropriate label.

The habit of correct association may be attained by any one, but it requires assiduous cultivation. It not only exerts a great influence upon the acquisition of knowledge, but also upon the formation of the mental characteristic; and it is closely connected both with that activity of mind which it is so important to foster, and with that soundness of judgment upon which so much of the solidity of a character, and its usefulness to others, must depend in future life.

In many sciences our knowledge rests upon an assured and exact basis. This certainly depends upon the facility with which we can trace effects to their true causes, can predict the effects of known causes, and consequently can calculate upon the absolute uniformity with which particular results may be obtained. This is a certainty which can be secured so long as we are dealing with limited portions of inanimate matter. We can determine with absolute accuracy the effects of a mechanical combination, and we can predict the results of a chemical experiment which we have already tried. If the consequence which we expect does not follow, we are sure that some unobserved disturbing agent has prevented the conclusion which we anticipated; and a little observation will enable us to discover and obviate its effects. It is our accurate knowledge which gives us the power that in so many instances we possess over material objects; and we must remember with Bacon, "Natura non nisi parendo vincitur."

The knowledge of the philosopher differs from that of the uneducated man less in kind than in degree, and in the manner in which it is acquired. In addition to the observation of the phenomena as they occur, the man of science institutes *experiments*; that is to say, he arranges and selects certain circumstances and excludes others, so as to enable him to determine what are the necessary, and what the merely accidental antecedents of the phenomena which he is examining; and upon the skill with which those experiments are arranged depends his progress in the discovery of scientific truth.

Now, in medical science there are sources of uncertainty which do not exist in physical science. One of the most important of these arises from the fact that, in most cases, we cannot make experiments and vary them at pleasure. In nearly every case of disease we must content ourselves with the observation of the phenomena; and how much lies hid even from the most careful observation! We see complex results only, but cannot trace all the conditions necessary to produce them. Hence accurate inferences can be deduced only by slow degrees; and hence it is in so many instances difficult to estimate the true value of the conclusions at which we have actually arrived.

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The remaining subject with which you will be engaged during your first winter session is *Chemistry*, by which you are taught the nature, properties, and modes of combination of the different kinds of matter; a science of vast extent, and of fundamental importance to you. From its wide range, and its difficulty, there is no branch which the student is more often tempted to neglect than this, although its bearing upon the practical part of his profession is unquestionably very great. It will be my business, after impressing upon you its leading principles, to endeavour to guide you in selecting those parts of the science which admit of direct application to your profession. For instance, the microscopic investigations of the physiologist and the pathologist would be partial and incomplete if the various tissues were unravelled only by the aid of the scalpel or the needle. The judicious use of solvents, the application of tinctorial agents, and the various expedients of microscopic chemistry, must be called in at every step to assist the ruder dissections effected by the knife.

With the chemistry of the atmosphere and of water many of the most important problems of hygiene and sanitary science are bound up. Efficient ventilation, or the removal of that portion of the atmosphere which has become chemically altered by respiration or by combustion, and which is consequently no longer fitted for the due support of life, is one of the great objects of the sanitary reformer. It is for this purpose that he widens streets, opens courts, puts in additional windows, and inserts ventilating gratings. It is to prevent the pollution of the air we breathe by the miasmata evolved by decaying animal and vegetable matter, and the spread of pestilence and death, that it becomes necessary to close the cesspool, and to cause the closet to be properly trapped. It is for this reason that the officer of health insists upon the removal of heaps of ordure, which, when duly returned to the soil, serve as needful manure to stimulate the growth of future plants, and which, by the transforming actions of the chemistry of vegetation, again become fitted to supply food and vigour to the animal creation.

Typhus, diarrhœa, even cholera itself, may often be traced to contamination of the water supply of a district with organic impurity; and in such cases a simple chemical examination of the water has often revealed the acting cause, and thus led at once to the adoption of the appropriate remedy in the introduction of water from a purer source. As illustrations of the direct applications of chemistry to medical practice, I need but remind you of the large and important class of diseases of the kidney and bladder. The different forms of gravel and the varieties of calculous affections can only be successfully treated by carefully watching the changing chemical conditions of the urine and its deposits. In diabetes and albuminuria, it is from the application of chemical tests that the physician obtains the most rapid and certain indications of the progress of the disease and the effects of his remedies.

Few subjects offer more important matter for investigation from a chemical point of view than the various forms of dyspepsia; for there is no function more intimately dependent than digestion upon chemical changes; and yet there are few over which we at present possess less definite control. The manner in which the food becomes converted into a soluble form, suitable not merely for absorption but for assimilation, is, indeed, but little understood; and no greater service could be rendered to practical medicine than a sound interpretation of the physiology of digestion, and of the pathology of dyspepsia; and this we must look for at the hands of the chemical physiologist.

Let me, then, earnestly urge you to the diligent study of the principles of chemistry. In no branch of science is it of more importance to obtain a strong grasp of principles, and in none, from the enormous mass of facts which it embraces, is a judicious selection of the parts to be studied in detail more indispensable. At the same time there is no subject which will by its intrinsic value and interest more amply repay the time and labour bestowed upon its acquisition.

ADDRESSES.

INTRODUCTORY ADDRESS DELIVERED AT ST. BARTHOLOMEW'S HOSPITAL MEDICAL SCHOOL.*

Session 1867-68.

BY WILLIAM ODLING, M.B. LOND., F.R.S.,

Lecturer on Chemistry at the Hospital.

I have dwelt largely upon the importance of your acquiring the utmost attainable knowledge of disease, in order that you may be able hereafter to deal with disease. You are now for a few years, and must indeed continue to be all your lives, students of medicine, so as to become and continue practitioners of medicine. With you, as future practitioners, knowledge is only a means to an end, and that end the cure of disease. The medical man is a medical artist; and his ultimate object is, not to accumulate knowledge, but to multiply cures. As practitioners of medicine, then, you are called upon, not only to know, but to act. Now, as was so clearly pointed out last year, in almost every concern of life in which action has to be taken, we do not act with an absolute certainty, either of the state of affairs under which we act, or of the result that will follow our action, but upon a probability only, approximating more or less to the value of an absolute certainty in different cases. And, if we are judicious men, we are all the more cautious in acting in proportion to the importance of our act, and the inferior certainty of our knowledge. The practice of medicine, indeed, like that of any other art, may, in various ways, be good or bad, judicious or injudicious, skilful or unskilful; but, to act skilfully, it is above all things necessary for you to act with a well-founded and well-applied judgment, alike as to the conditions and consequences of your act. For this purpose, you must have an experience of cases on which to found your judgment of any particular case, and also an experience in the application of your judgment to the features of different cases. And this double experience by which you will be enabled to appreciate what is required, must be further supplemented by an experience in effecting what is required. Skill in ascertaining how matters stand, skill in perceiving what is desirable, and skill in effecting what you desire, can only be attained by constant practice in ascertaining, perceiving, and effecting. In other words, so as to express an obvious truism, for the skilful practice of medical art you must be practical men; not, indeed, as distinguished from scientific men—not as being unacquainted with the science of disease, but rather as having pursued your knowledge of that science to its most special developments, and familiarised yourselves with its most special applications. As practical men, having that prompt understanding of what is required to be done, and how to do it, which continuous practice alone can impart, you may, perhaps, have a something in addition to the merely scientific man, but assuredly nothing in contradistinction to him. Mentally, indeed, we may dissociate the science from the art of medicine; actually they are one and indivisible. Science is knowing; art is doing or practising; but it is quite impossible to know the science without practising the art, or to practise the art without learning the science of medicine. Of course, the scientific man, though possessing a knowledge of all the sciences under the sun except the science of medicine, wanting the knowledge of that, can never be a physician. And the knowledge of medicine, like any other branch of natural history knowledge, can only be acquired by working at the subject of that knowledge—that is to say, by attending to the practice of medicine and the investigation of disease. For the successful practice of medicine, then, as for the practice of any other art, that particular kind of intimate personal knowledge which results from or constitutes personal experience, is most of all required; since the conditions affecting disease in any particular case are so various that nothing but experience will enable you to estimate them even approximately. . . . .

And now, gentlemen, what has been the object of my address? It has been to satisfy you of the reality of medicine as a branch of human knowledge, and of the soundness of medical practice in so far as it is based upon knowledge and philanthropy. Every branch of human knowledge is necessarily incomplete, and very much in proportion to the complex and recondite character of the phenomena with which it concerns itself. As a consequence of its incompleteness, every branch of knowledge is more or less tinctured with error; and much unsuspected error doubtless prevails and will prevail in medicine. But the strength of our position is this, that we are desirous only for the establishment of truth. Our present views are merely the resultant of our present knowledge, and are held by us on the express tenure of changeability with greater certainty of knowledge. To acquire this greater certainty, our method is to consider, observe, and investigate phenomena; not, indeed, without an expectation of finding—not even without some unphilosophical wish to find—that the truth may lie in a particular direction; but still with the single-minded intention of learning what the fact of the matter is, and of loyally accepting it.

It is thus that our present knowledge of medicine has been established by the same method as the knowledge of every other branch of natural science; and many investigations in practical medicine will take their place amongst the finest examples of scientific work recorded. Allowing also for the different character of its subject, much of our knowledge with regard to disease will bear the most searching examination to which any branch of knowledge can be exposed; and there is no part of our professed knowledge that we fear to submit to the most rigorous ordeal, since we derive no less benefit from its refutation than its confirmation. As students of nature, we have no system of medicine to stand or fall by; for the physician, of all men in the world, is essentially *homo naturæ minister et interpres*, and nothing more. He no longer looks upon himself as the depository of some occult mystery, but as a mere student of nature; whose knowledge,

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* Extract, communicated and corrected by the Author.

indeed, so far as it goes, is a real knowledge, but who aspires to and incessantly strives for the attainment of more perfect knowledge.

The knowledge of diseases, however, is a plant of slow growth. You cannot complete the knowledge here; you can, indeed, do little more than lay the foundation for it; and the sounder and broader your foundation, the greater the degree to which you will hereafter be able to extend your knowledge. But to understand disease aright, as to understand every other phenomenon of nature, you must not only study the disease itself, but must also prepare yourselves for such study by the acquisition of extensive preliminary knowledge. It is most unfortunate that, from the defective state of physical education at schools and colleges, much of the preliminary knowledge which you ought already to possess, you will have to acquire here, whereby valuable time, which, in this home of disease, could be advantageously devoted to the work of your profession, will be seriously encroached upon; and, after all, you will not obtain such a knowledge of general science as you ought to possess, and which—in the forcible language of Prof. Huxley—you would possess "if those who regulate education in this country had a right conception of what their duties are, or of the purpose of education, and the conditions of the progress of mankind at the present time." By rights, I maintain, no one should be allowed to enter at a medical school without having a competent knowledge of the three great divisions of natural science—namely, physics, chemistry, and biology; whereby our distinct courses of comparative anatomy, of botany, and of general chemistry and physics, might be abolished from the curriculum of hospital study, as deriving no advantage from their association with hospital work. Chemistry, indeed, as the basis of vital dynamics and sheet-anchor of rational therapeutics, must ever form an important branch of medical education. But the sort of chemistry which should be taught—in our own chemical theatre, for instance, and in that magnificent laboratory which you, Sir, have lately had constructed—is very different from the chemistry which I am in the habit of teaching, and shall, I fear, long continue to teach. It should be an altogether special development of chemistry, having to the chemistry ordinarily taught much the same relation that the study of human anatomy and physiology has to the study of biology in general, and having a scarcely less direct bearing upon your strictly professional duties.

But you must make the best of circumstances as they exist, and endeavour, while here, to obtain as much preliminary science as you can. In your future careers you will necessarily have to compete with men far more experienced in disease than yourselves, and your only chance of competing with them successfully will consist in compensating as far as may be for their superior experience, by starting upon a surer foundation of medical knowledge than was possible to them at the outset of their careers. But all your preliminary knowledge must culminate in your acquiring an ultimate knowledge of disease, and the ultimate branch of your knowledge is just as scientific as the preliminary. If you content yourselves with laying the foundation—if you neglect to raise the superstructure,—you cannot be called physicians. In that case, and in that case only, will you be at a disadvantage with the so-called practical man, whose superstructure at any rate exists, though based on a much less certain foundation, and constituting a far more rickety edifice than yours might be. You cannot, I have said, complete your knowledge of disease during your study here. You may, nevertheless, among the out-patients, and in the wards, and more especially in the dead-house, of this great hospital, learn that of disease which, neglecting to learn here, you will never be able to learn elsewhere. Here alone can you acquire the art of examining disease in the living; here alone can you examine the results of disease in the dead.

The knowledge, then, both preliminary and professional, you will have to obtain during your few years of hospital study is enormous in its amount, and in its kind not only most various, but for the most part very different from any to which you have previously devoted yourselves. It is lamentable that this should be the case, and reflects seriously upon those on whom the charge of general education in this country chiefly depends. Both as regards the attainment of real knowledge, however, and the training of your powers of observation, your judgments, and your understandings, many of you will, I believe, gain more during your first year of study here than during the last half-dozen years of your previous lives. But to achieve this gain, you must work laboriously and continuously. Even the ablest of men cannot afford to dispense with work. I have set before you the career of Sir William Lawrence, not, perhaps, as an example to you in every respect, for men of his extraordinary powers could not fail of success in any walk of life, and might safely neglect the special requirements demanded by any particular walk. But even he would never have succeeded, either in surgery or any other profession, without work. Referring to Lawrence in his student-days, Sir Benjamin Brodie wrote of him, some fifty years afterwards, "I never knew any one who had a greater capacity of learning than he had, or more industry." I have said that the amount of knowledge you will have to acquire is enormous. Still it is not more than can be acquired, or more than habitually is acquired, by industrious men, an appellation which all of you must make it a point to deserve. Remember that the period of your sojourn here is the most important period, is indeed the seed-time of your lives. With a view to the harvest of ultimate success in life, if for no nobler object—for the sake of your own future happiness and self-respect, still more for the welfare of those committed to your charge, take care that that time is not misspent or frittered away. What you may if you please secure now, you will never be able to attain hereafter. Let me then entreat you to make the most of your present opportunities. Let me say to each one of you—

> "Stay, stay the present instant!
> Imprint the marks of wisdom on its wings!
> O, let it not elude thy grasp, but, like
> The good old patriarch upon record,
> Hold the fleet angel fast until he bless thee!"

FOREIGN SCIENCE.

(From our own Correspondent.)

Paris, Oct. 2, 1867.

Formation of Volcanic Sal-ammoniac.—Power of the light of the electric spark to penetrate space.—Solar radiation at high elevations.

M. **Angiolo Raniere** has presented a memoir to the Academy of Sciences relative to the martial sal-ammoniac collected on the lava of Vesuvius during the eruption of 1850, starting from the region known by the name of the *Atrio dell Cavallo*, as far as the south-east of the actual crater.

For a long time past naturalists have been divided upon the question of the origin of the sal-ammoniac disengaged by the fumes of volcanic lava. (*See third series of the "Annales de Chimie et de Physique*, 1853," pp. 289 & 292.) Some admit that the hydrochloric acid, escaping from lava in motion, united to the iron which enters into their composition, formed a perchloride of iron, which, joined to the ammonia of the atmosphere, and to the excess of hydrochloric acid of these same lavas, would give rise to this mixture, of simple ammoniacal salt, and perchloride of iron, collected in the fissures.

This opinion does not agree with facts observed by M. Raniere during the flow of the lava at the eruption of Vesuvius in 1850. He observed that no fumes existed except where the lava had invaded a cultivated and manured territory; also that these fumes were in such abundance that they gave more than 10,000 kilogrammes of salt of ammonia,* while at another spot where the igneous current had taken its

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* Antonio de Napoli, dealer in chemical products at Naples, bought more than a hundred quintals, and he has still twenty quintals left. Many other speculators have followed his example.

direction over the lava of 1834,* which was nothing but a rocky and sandy mass, there were found no salts of ammonia.

This fact demonstrates clearly that the ammoniacal salt proceeds from the decomposition of organic substances contained in the ground invaded by the lava, which effects at a great heat, a sort of distillation, during which carbonate of ammonia is disengaged, to be converted into sal-ammoniac by the action of hydrochloric acid. We shall speak of the origin of this latter presently.

After the extreme intensity of heat has fused the silicic acid of this lava it acts on the silex of the lands surrounding Vesuvius. These latter are in a great portion formed of quartz, sand, and pouzzolana; and in the same manner as quartz acts in the preparation of phosphorus, by the process of Wöhler, the silex reacts on the sea-salt of these lands, as well as on other chlorides which they contain, and gives rise to muriatic acid, and to chloride of iron, with the hydrate of the sesquioxide of iron contained in the ground. Both of these products being volatile at this very elevated temperature, the result is that they acquire an extraordinary expansive force, and when the lava is yet soft they make their way through the mass, and compose what is called "fumerollea." From this gas emanates an aqueous vapour and a mixture of perchloride of iron, chloride of ammonium, sulphurous acid, sulphuretted hydrogen, etc. This lasts as long as the lava is not quite cold.

Mr. Felix Lucas, concludes from very original theoretic considerations, that the luminous distance at which the electric spark is visible is greater than that of a permanent light the apparent intensity of which would equal 250,000 times that of the spark. The light actually employed to illuminate our new lighthouses gives a brilliancy equal to 125 carcel lamps. An electric spark possessing the illuminating power of the 200th part only of a carcel burner, is superior as to its power of projecting light. Hence we can conceive the immense effect of a warning light composed of the intermittent flashes of the electric spark proceeding from a strong Leyden jar battery. M. Lucas states that, in an experiment made in a laboratory, two apparatuses were established, one voltaic equal to 125 carcel lamps, and another spark-battery equivalent to only the 1-2000th part of a carcel wick. The photometer (such as is employed in the lighthouse administration) showed a marked superiority in favour of the spark.

Actinometric experiments made with the greatest care, at Geneva, on the Glacier des Bossons and on the summit of Mont Blanc by M. Sorel, have led him to the following conclusions:—The increase of the radiation in proportion as the altitude is less rapid than the diminution of the barometric pressure, or than the diminution of the atmospheric thickness. This result is contrary to what can be deduced from the observations made by Mr. Forbes, in 1832, on the Faulhorn, and the Brientz, (*Phil. Trans.* 1842, part ii., p. 225). The atmospheric pressure being the same, the radiation observed at an elevated altitude is incontestably more powerful than at a lower altitude.

The ratio of the intensity of the solar radiation on Mont Blanc and Geneva is as about 6 to 5. Thus the solar heat which has arrived as far as 4,800 mètres above the superior strata of the atmosphere, is subject to an absorption of $\frac{1}{5}$, in traversing at an angle of 60° to 65°, the lower strata of the air at an altitude of 400 mètres. F. Moigno.

Paris, Oct. 9, 1867.

Deep Engraving without Varnish—Spectrum of the Flame of the Bessemer Converter—Analogy of it to Stellar Spectra—Spectrum of the colour of Water and Ice through great thicknesses—New Voltaic Piles—The Pascal-Newton Forgeries.

Method of obtaining deep engraving and relief without varnish, by Mr. Joseph Balsamo, Professor of Physics at the College of Lecca, Italy. Starting from the fact that by pressing the finger on different points of a vibrating plate, a great variety of reliefs and bosses are formed by the distribution of sand at the surface, M. Balsamo thinks that pressure obtained on different points of a plate immersed in a galvanic bath would modify the deposit of metal at the surface. Experiments have confirmed his theory, and the mode of operation is as follows:—In a solution of acetate of iron, to which has been added some grammes of phosphoric acid, and some fragments of phosphorus, he plunged two plates of common iron, communicating one with the negative pole, and the other with the positive pole of a Bunsen battery of three elements. Between the two plates, and perpendicularly to their surface, a blade of glass is fixed, 210 millimètres long and 35 wide, so as to press by its edges the two plates suspended at the two poles of the pile. In order to better establish the contact between these two iron plates and the edges of the glass blade, he drives in two edges of wood, one on each side, between the sides of the vessel containing the ferruginous solution and the exterior surfaces of the metallic blades. After two days of voltaic action, the metallic iron is deposited on the blade suspended at the negative pole in paralled vertical bands on the two sides of it, a hollow groove alternating with a ridge in relief. The hollows correspond to the space occupied by the edge of the glass sheet, and the reliefs on the sides of this same plate. The vacant lines, that is to say, those on which the metallic iron is not deposited, were in consequence the nodal lines, the full lines on which the iron was precipitated were the reliefs, or lines of vibration.

M. Balsamo has substituted for the straight piece of glass a curved one of an S shape, so that the points of contact of the glass can form a slight sinuosity, and the iron is deposited in sinuous planes alternating with sinuous hollows. In forming the designs with the aid of glass or clay, porcelain, etc., all the parts in contact with the edges of the design will be reproduced as many times, on the same surface, as the free space left by contours is more extended. Damask work, designs in engraving or in relief work, repeated on the same surface, can be obtained thus by a simple application of the negative type against the blade suspended at the positive pole. In place of acetate of iron we can employ other solutions of iron or metallic salts.

In a communication to the Italian Society—called the Forty—of Modena, Father Secchi made the following very interesting and curious observation on the spectrum of a terrestial flame that struck him as very similiar to the spectrum of certain yellow and red stars. This flame is that which proceeds from a converter in which Bessemer steel is being made; and this spectrum, well known by directors of iron works, when the iron is completely decarbonised, presents a series of very fine and very numerous lines or streaks which remind one of *a* Orionis and *a* Herculis, only that it is reversed. This results, undoubtedly, from the great number of metals burning in the flame, and the spectrum presents several lines well known and determined; also, this flame seemed to be the only one comparable with that of the coloured stars, and there is nothing improbable in this fact when we consider the composition of aerolites in which iron predominates. But what is most important in our terrestrial flames, we have a fertile and abundant field of observation of spectra which are closely allied to those of certain stars. M. Secchi states that he is indebted to M. Lemonnier, director of the Terre-Neuve Works, near St. Etienne, for this observation.

M. Secchi had formerly ascertained that the spectrum of the colour of sea water is deprived of its red portion at small depths, and successively of the yellow and green, at least partially, for the greater depths, and then it appears of a violet blue. He tried to find out whether the same was the case in glaciers, and made some interesting experiments in an artificial grotto in the Grindenwald glacier.

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* This lava destroyed a whole well populated district of more than two hundred dwellings in the commune of Ottagano at a spot called Tersigno. It furnished also several hundred quintals of salts of ammonia. M. Raniere is of opinion—and his theory is in perfect harmony with facts—that the muriatic acid proceeds from either rock salt in the ground or from the infiltration of sea-water.

This cavern was 100 mètres deep, transparent in its walls, through which the solar light penetrated. This light was of a fine blue tint. In this shade of colour the red was extremely weak, so that in this grotto human countenances had a cadaverous aspect almost alarming. On looking towards the entry, at a certain distance in the cavern it appeared to be lit up with a red light, undoubtedly the effect of contrast. The thickness of the superposed mass was not enough to show a greater effect than the almost complete absence of the red, and a great diminution of the yellow. The ice was said to be 15 mètres thick, but it was probably less. The ice was perfectly compact, limpid as crystal, but with a few air bubbles. The hardness was not considerable.

At the Academy of Sciences, on the 23rd ult., M. Peligot presented, in the name of M. J. E. Balsamo, a memoir on the unipolarity of iron in liquids, and a new voltaic pile.

The pile is formed of two blades of iron, one plunged in dilute sulphuric acid, the other in a solution of chloride of sodium separated from the acidulated water by a porous diaphragm. The iron of the acidulated water acts as zinc, and that of the saline solution acts as copper. The current, constant and of considerable intensity, proceeds from the property possessed by iron of polarising itself differently in certain solutions between which osmogenic action takes place.

If two blades of iron of the same molecular constitution be suspended at the two poles of a galvanic bath (acetate of iron and phosphoric acid) animated by the current capable of decomposing the salt of iron of the bath, the plate suspended at the positive pole will be attacked as usual, while the blade suspended at the negative pole is covered with a homogeneous and thick coating of iron. Experiments have proved that the first iron is electro-positive, as zinc, and that the second acts electro-negatively, as copper; perhaps it is because the iron suspended at the positive pole is combined with a small quantity of phosphorus. M. Balsamo plunges, at the same time, in oxalic acid, two small magnetised bars of the same surface and of the same weight, one having its north pole in the liquid and its south pole out of it. The second bar is in the contrary position. The first acted as zinc, the latter as copper, and a current of electricity was the consequence.

The insertion in the Chemical News of the letter of Mr. Robert Grant (*Amer. Reprint*, Dec., 1867,) makes us revert to the question of the authenticity of M. Pascal's letters. The long discussion of Mr. Grant only proves one thing, and that is, that the figures in the notes of Pascal are those of the third edition of the *Principia;* or rather that Newton has only inserted in his third edition the figures that he had received in 1658. It does not prove at all that Pascal was not in possession, or could not be in possession, of observations exact enough to deduce the figures of the notes. He does not demonstrate either the astronomic authenticity of the figures which should serve as a base for the calculations of Newton.

But M. Chasles has brought to light letters from Galileo, Flamsteed, Huygens, Polignac, etc., etc. We are also informed that the observations which have served as a basis to Pascal's calculations come from Kepler and Galileo, but the figures in the third edition of the *Principia* (1760) come from the hands of Galileo, who had them, as he declares, from Pascal.

To resume: 1. Acquainted with the fact that the essential difference between the writing in the autographs and that of the authentic letters of Newton consisted principally in the conformation of the *e* and *d*, M. Chasles has shown that many of these documents had the two characteristic forms required. 2. These autographs date evidently as far back as the 17th century, and contain four authentic signatures of Newton. Sir David Brewster affirms that after a publication brought out in this century in the General Dictionary, or the Macclesfield Correspondence of 1841, these signatures were made. 3. Messrs. Hirst and White, thinking that they had made a discovery, and proud of finding in the collection of Desmazeaux and Clarke the text of the notes of Newton, did not expect to learn from M. Chasles that Desmazeaux, whose collection he has, only reproduced the documents passing through his hands, and that Newton sent, under the form of notes to Clarke, arguments in favour of Leibnitz. 4. M. Grant finds in the third edition of the *Principia* in 1725, Pascal's figures purposely different from those of the second edition, but he was not aware that the same figures were attributed in 1760 to Pascal by an authentic letter of Galileo.

F. Moigno.

Paris, Oct. 24, 1867.

Preparation of Hydrogen on the large scale.—Chemical Manures.—The Chemistry of Rotten Eggs.—Still another Cure for Cholera.

For the very costly process of the preparation of hydrogen by iron and sulphuric acid, M. Giffard now substitutes the decomposition of steam by incandescent coke. The gas is produced in a sort of furnace charged at the back with coke, divided by refractory stones at the front into a great number of channels which are traversed by the gas. When the fire is well lighted, the sides of these channels attain a red heat, and the coke is uniformly red throughout its thickness, which is considerable. Then the damper is shut, the ashpit closed, and a jet of steam is made to play on the under surface of the coke. By traversing this mass of coke, the steam is decomposed, producing carbonic oxide and hydrogen gases.

At the upper part of the boiler there are nine small jets of steam, which pass through the carbon and mix with the hydrogen and oxide of carbon as far as the red-hot channels, where a new reaction takes place. The carbonic oxide gas is more highly oxygenated at the expense of the steam, and is converted into carbonic acid gas, while the hydrogen is set at liberty. The system of tubes is very ingeniously contrived; the tube which unites the two boilers and supplies the four cylinders is prolonged on the opposite side in case of need. Two tubes, which start from the principal trunk, conduct the jets of steam which pass over the coke, and those which traverse it for the production of gas. Two other tubes called blowers, leading to the chimney and the ashpit, assist the combustion by jets of steam. The second produces a reversed draught in order to produce a downward combustion. Lastly, two groups of tubes furnished with and controlled four ways by cocks, conduct the steam to two cylinders, the object of which is to open and shut, one the ashpit door, and the other the damper of the chimney. The gas on quitting the generating furnace is necessarily charged with much steam, and it passes into tubes kept constantly surrounded by cold water, changing continuously, which condenses the greater part of the steam; the water of condensation falls into the bottom of a sort of vertical tubular boiler, transformed into a refrigerator, and is let out by a discharge cock. The gas then passes through a lime purifier, in which it is desiccated before it arrives at the balloon. The purifier is a large case of strong boiler plate, with a man-hole at top for introducing the lime, and a grating at the bottom on which the lime rests, and beneath which the gas passes. At a small distance above the grating there are moveable plates revolving on their axes. In the ordinary position in which they are placed, vertically on their edges, the gas enters by interstices similar to those of a Venetian blind. But when the lower part of the lime is exhausted the plates are turned horizontally; they then form a floor on which the unslacked lime rests.

The production of gas is intermittent. When the steam has in part extinguished the coke and cooled the sides of the refractory stone, the admission of the steam is cut off; the ashpit and damper closed, then one or other of the blowers are set in motion, and the operation of gas-making commences.

M. Georges Ville, the learned professor of the Museum, has rendered to the agricultural world an immense service. In fact, plants which live in the ground and seem to know the best constituents for their well-being, are perhaps the best chemists as far as regards the choice of their elements. M. G. Ville has examined and ascertained, by the aspect even, what elements exist in a sufficient quantity, and what are wanting in the soil to nourish the vegetable.

On the property of M. Payen, at Boucourt (Aisne), there is an experimental plot of ground, which is quite perfect in its way, and which has already furnished important results. This piece of ground is laid out similarly to that of Vincennes, where, by the different chemical manures combined by the formulæ of M. Ville, we remark the same ascending scale of crops, from the weakest to the most luxuriant, without the law governing the culture having shown a single exception. Not far from the border of a road, in a flinty land of very bad quality, a plot was manured with 40 tons to the hectare; another parcel of the same ground received a *complete* manure of 400 kil. of superphosphate of lime, 200 kil. nitrate of potash, 250 kil. sulphate of ammonia, and 350 kil. of sulphate of lime—in all 1.200 kil.—the cost of which was 325 francs per hectare. Stable dung produced a miserable crop of wheat; the chemical manure gave a splendid return. From a letter addressed to the *Journal de l'Aisne* we learn the following:—

A hectare of sand treated by the complete manure produced—

1. 8 hectolitres of wheat, at 27 francs,.....	756 f.	0 c.
2. Straw, 6,070 kilos., at 0 f. 4 c.,........	242	80
3. Small straw,.......................	4	
	1,002 f.	80 c.

The same ground treated with good farm manure, 40 tons per hectare, only produced—

Foreign Science 2.

1. 28 hectolitres, 50 litres at 27 francs,.....	229 f.	50 c.
2. Straw, 1,696 kilos., at 0 f. 4 c.,..........	67	84
3. Small straw,..........................	1	50
	298 f.	84 c.

M. Al. Donne read a note on rotten eggs, and the manner of action upon the organic products which result from his collection of eggs. The following experiments, the ideas of which have been suggested by M. Balard, respond completely to the conditions of the problem. Old eggs are taken and well shook up so as to mix the yolk with the white; they are plunged into a vase, which is half filled with distilled water; the vase is put then under the receivers of an air-pump. While the vacuum is being made, small air bubbles cover the surface of the egg-shells, penetrating by the pores into the exterior air. The eggs are kept for many hours under the bell-glass without necessarily having a perfect vacuum. When a great portion of the gases of the egg have thus passed, air is let into the bell receiver; the vase is left, and the eggs remain in the water for four hours; the water penetrates into the egg, and by augmentation of weight it sinks deeper in the water; it is then drawn out, wiped, and left alone in an egg-cup. Eggs thus treated decompose and rot most easily; left in a stove at 30° or 35° C. in daylight (which is perhaps essential to the vitality), they exhale at the lapse of eight or fifteen days, perhaps three weeks, a fetid odour; often the same substance exudes through the pores of the shell. In another series of experiments—instead of leaving the eggs in the free air, M. Donne left some in water. In two or three days the water became turbid, and it was peopled with monads and vibrios visible by the microscope. The egg itself was rotten and presented no trace of animation.

M. Poznanski has recently investigated the effects of prussic acid administered in cases of cholora and intermittent fever, in which alteration and carbonisation of the blood takes place. Experiments on dogs and on cholera patients show that half a drop of pure prussic acid suitably administered is well adapted for the treatment and cure of cholera.

F. Moigno.

REPORTS OF SOCIETIES.

BRITISH PHARMACEUTICAL CONFERENCE.

Fourth Annual Meeting at Dundee. President, Professor Bently, F.L.S., M.R.C.S., etc.

(Continued from page 249, American Reprint, Nov., 1867.)

"*On Burgundy Pitch.*" By Daniel Hanbury, F.R.S.

The authors of the British Pharmacopœia have defined Burgundy Pitch (*Pix Burgundica*) as a *resinous exudation from the stem of the Spruce Fir, Abies excelsa* DC. (*Pinus Abies* L., P. *excelsa* Lam.) *melted and strained*. They have thus followed the London College of Physicians, which for nearly a century and a half has included this substance in its Materia Medica, indicating in the later editions of its Pharmacopœia a similar botanical origin.

On the Continent the term *Pix Burgundica* (which is not frequently applied) appears to have a less definite signification than with us, being used synonymously with *Resina alba* to designate the resins of various coniferous trees after purification by being boiled in water and strained.

In France as in England the term *Burgundy Pitch* (*Poix de Bourgogne*) is by the more accurate writers restricted to the melted and strained resin of the Spruce Fir, of which substance the following description is given in the last edition of the Codex:

[*Translation*] Burgundy Pitch is of brownish yellow, solid and brittle in the cold, flowing when warm, very tenacious, having a peculiar odour, and an aromatic taste without bitterness; not completely soluble in alcohol in the cold. There is frequently substituted for it another product called white pitch [*poix blanche*], prepared with *galipot** or a mixture of yellow resin and Bordeaux turpentine, melted and mixed with water; this artificial pitch has a strong smell of Bordeaux turpentine, and a very marked bitter taste. It is entirely soluble in alcohol.

Where then is true Burgundy Pitch manufactured? Is it actually met with in commerce? By what characters may we judge of its purity?

The authors of the British Pharmacopœia mention it as a production of Switzerland, where the Spruce Fir is certainly found in great abundance. But I have it upon excellent authority, that of my friend Dr. Flückiger of Bern, that at the present time no terebinthinous resins are collected in Switzerland for commercial purposes. Neither is true Burgundy Pitch produced in France, as its name would seem to indicate, *Pinus maritima*, Lamb., being in fact the only tree the resin of which is collected in that country as an industrial product.

I examined the various collections of forest-products in the French Exhibition. From Finland I discovered a suite of specimens illustrating this very subject. Baron Linder of Svarta, near Helsingfors, is the exhibitor of the resin of the Spruce Fir in two forms, namely:

1. The crude resin as exuded from the trunk of the tree and described in the following words: "*Barras* ou gomme concrète, adhérente aux sapins (*Pinus Abies*). Produit brut servant à la fabrication de résine, etc., etc.—Prix 12 francs les 100 kilogr.

2. The resin purified by melting in contact with the vapour of water, and straining. It is thus described on the label attached to the specimen: *Résine jaune* cuite (a vapeur d'eau à chaleur modérée) de *barras* de sapin (*Pinus Abies*). Prix 40 francs les 100 kilogr.: production annuelle 35,000 kilogr.

Of these two resins, the first is not found in English commerce: the second constitutes genuine Burgundy Pitch, precisely such as may be bought in the London market. The quantity of this purified resin produced annually, it will be observed, is very considerable, being equivalent to 77,000 pounds, or more than 34 tons weight.

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* (*Note by translator*) *Galipot*, dry resin collected in France from the trunks of *Pinus maritima*, Lamb.

The Paris exhibition shows that true Burgundy Pitch is also produced in Germany.

Another exhibitor of genuine Burgundy Pitch is Mr. Theodor Müllner, of Hinter Brühl Post Mödling, near Vienna, who shows *Fichtenharz* or crude resin of the Spruce Fir and *Fichtenpech*, which is the same in a purified condition. The latter may be regarded as a type of good Burgundy Pitch.

These contributions to the Paris Exhibition show that the resin of the Spruce is collected for trade purposes in Finland and in Germany, and in the first named country upon a very considerable scale. It does not, however, appear that it is ever termed *Burgundy Pitch* in the places where it is produced.

Although genuine Burgundy Pitch (usually, it must be admitted, in a very impure state) has been always obtainable in the London market, it is rarely found genuine in the shops, an artificial compound being very generally supplied in place of it.

In examining the characters of genuine and spurious Burgundy Pitch, I have noted the following differences:

True Burgundy Pitch.	*Artificial Burgundy Pitch.*
Colour dull yellowish-brown; fracture shining conchoidal; translucent; some samples contain much water, and are opaque and of a dull grey colour, and require straining to free them from impurities.	Colour usually more brilliant than that of the true Burgundy Pitch.
Odour peculiarly aromatic.	Odour weak and hardly aromatic.
Not wholly soluble in alcohol of ·838, but leaves a small amount of fine white flocculent matter.	Still less completely soluble in alcohol of ·838.
Placed in contact with double its weight of glacial acetic acid in a vial, is dissolved with the exception of a small amount of flocculent matter.	Similarly treated, forms a turbid mixture, which soon separated into two layers, a thick oily liquid above and a bright solution below.

The foregoing characters apply to most of the artificial Burgundy Pitch which I have examined, and may be useful, so far as they go, for distinguishing the genuine from the spurious. The odour of true Burgundy Pitch is in itself an excellent criterion which cannot be conveyed by description. Solubility in glacial acetic acid serves to reveal the presence of fatty matter, which is a common, perhaps an essential, ingredient in the artificial Burgundy Pitch made in this country.

From what has preceded may be deduced the following

Conclusions:

1. True Burgundy Pitch is the melted and strained resin of *Abies excelsa*, DC.
2. An artificial compound is usually sold in lieu of it, both in this country and on the Continent.
3. True Burgundy Pitch is produced on a large scale in Finland, also of very fine quality in Baden and in Austria.
4. True Burgundy Pitch differs palpably from the artificial, and may be easily distinguished from it.

I. *Report on the Advantages or Disadvantages of the Employment in Pharmacy of Nitric Acid of Specific Gravity* 1·5.

II. *Report on the Nitro Hydrochloric Acid of the British Pharmacopœia, and the Changes in it on keeping.*

By W. E. HEATHFIELD, F.C.S.

The inquiry proposed in reference to the first of these two subjects having been rendered supererogatory, in consequence of the change prescribed in the British Pharmacopœia, which has appeared since the announcement of these questions, I pass it over with the comment that it has been difficult to procure nitric acid uniformly of the gravity of 1·5; that it is very rarely free from a considerable quantity of nitrous acid, as evidenced when the acid is poured into water with a view to dilution; that it is uncertain in strength, from its tendency to decompose, and that it is inconvenient to pack, dangerous in transit, and unmanageable in use. The acid of the British Pharmacopœia of 1867 is doubtless an excellent substitute, containing, as it does, 70 per cent. of monohydrated acid, this water being combined with the acid as a base, whilst the accompanying 30 per cent. are in such a state of combination as to be termed the constitutional water. It undergoes no change on keeping.

Referring to the second inquiry—the nitro-hydrochloric acid, and the changes in it on keeping—it is to be observed, that since the institution of these experiments, the British Pharmacopœia of 1867 has been presented with an alteration in the formula and directions for the production of this acid, which yields the following results: The specific gravity of the two acids on admixture and after cooling was 1·277, but on standing for 24 hours, as directed, was 1·268. On adding the quantity of water for the production of the dilute acid, the specific gravity was found to be but 1·063, and 352·4 grains, or 6 fluid drachms, required but 840 measures of volumetric solution of soda for neutralization.

This experiment having been conducted with a view to determine the loss of hydrochloric acid consequent upon leaving the mixed acids for twenty-four hours, the operation was conducted so that on the mixture of the two acids in a loosely-stoppered bottle, the escaping chlorine should be collected under a bell-glass, and should be received into a solution of potassa. This solution, at the end of the twenty-four hours, was subjected to estimation by means of nitrate of silver, and was found to be charged with chlorine, which, calculated as hydrochloric acid, was found to be in such a proportion as to have diminished the strength of the nitro-hydrochloric acid by about 3 per cent. The loss of nitric acid was not estimated.

Proceeding somewhat differently, with a view to the production of dilute nitro-hydrochloric acid, the following process was adopted: The proportions of acids ordered in the Pharmacopœia of 1867 were united, and on cooling the specific gravity was 1·277. The water was then added, and the specific gravity was 1·074, thus corresponding to the theoretic gravity of the Pharmacopœia of 1864. 352·4 grains required 1·000 measures of volumetric solution of soda. This experiment was made on the 31st of May, and the tests were again applied on the 29th of August, when no variation had taken place, thus proving that the diluted acid was not impaired by keeping for a moderate length of time.

Whatever may be the estimation in which the process for the production of diluted nitro-hydrochloric acid is held, it is clear that it can scarcely attain the result desired, viz., uniformity. If the acids are mixed as directed, there must necessarily be loss, for it is not easy to imprison the escaping vapours, and an explosion would be likely to occur in a bottle well stoppered; in one not so, as directed, the escape of vapours is considerable, as indicated by the experiments detailed in this paper.

"*Notes on Tinctura Opii and Liq. Opii Sedativus.*"

By Mr. ALFRED SOUTHALL, Birmingham.

In continuation of a subject which was brought forward at the last meeting of the Conference, viz., the analysis of various specimens of ordinary commercial opium; in order, further, to show the extremely uncertain medicinal value of different samples, I have since examined a variety of specimens of tincture of opium, some of which have been kindly forwarded to me by Dr. Attfield. These specimens were, I believe, procured indiscriminately from the establishments of various pharmaceutists, and show a variation in strength which may well rather alarm the prescriber for the welfare of his patient.

Taking the standard of strength required by the British Pharmacopœia, which states that 100 grains of opium should yield at least 6 to 8 per cent. of morphia, the consequent strength of tincture of opium, B.P., should be not less than 0·5 per cent. of morphia. The following is my result of nine samples of tincture:—

No. 1 specimen contained 0·3 per cent. of morphia.
" 2 " " 0·5 " "
" 3 " " 0·6 " "
" 4 " " 0·5 " "
" 5 " " 0·2 " "
" 6 " " 0·5 " "
" 7 " " 0·4 " "
" 8 " " 0·7 " "
" 9 " " 0·5 " "

Good commercial opium, such as is commonly found in the English market (as our analysis last year showed), contains frequently as much as 10 to 13 per cent. of morphia; and the Pharmacopœia, laying no restriction upon a maximum yield of morphia, opens a wide door for a great diversity in the strength of its opium preparations, so that a tincture yielding from ½ to 1 per cent. of morphia is within the Pharmacopœia limits.

Although liq. opii sedativus is not officinal, yet this form of administering opium is scarcely less important than the tincture. It is, however, interesting to notice in the analysis of the eight following samples, that the same wide diversity exists:—

No. 1 specimen contains 0·6 per cent. of morphia.
" 2 " " 1·2 " "
" 3 " " 0·7 " "
" 4 " " 1·0 " "
" 5 " " 0·5 " "
" 6 " " 0·8 " "
" 7 " " 1·5 " "
" 8 " " 1·1 " "

"*Remarks upon the Uses of Bisulphite of Lime in Pharmacy.*"

By Wentworth Lascelles Scott, F.C.S., *etc.*

I have undertaken to lay before the British Pharmaceutical Conference, in a few words, the results of some experiments instituted with a view of discovering a means of preventing the rancidity and decomposition to which various ointments and fatty preparations are liable, if kept for any length of time.

A series of specimens of freshly-made spermaceti and other ointments, cold-cream, bear's grease, and simple lard, were placed in similar pots, and allowed to rest in a warm situation; a duplicate series, to which a very small proportion of bisulphite of lime had been added, being put by the side of the first.

In the course of six or seven months, most of the first series had become more or less decomposed; they had an acid reaction and disagreeable odour, while those to which the bisulphite had been added remained absolutely fresh and sweet. In consequence, I now treat all preparations of fatty or oleaginous substances with a little of this salt, applied in the form of strong solution, and have never yet found it to fail.

For ointments a fluid drachm to each pound is quite sufficient to preserve them, while it has no injurious action whatever, and is quite compatible with the great majority of ointments and oily preparations,—a remark which does not apply to the alkaline sulphites or bisulphites which have from time to time been brought forward for similar purposes.

Beef-tea or broth in hospitals or otherwise may be prevented from turning sour by stirring in a few drops of the bisulphite of lime solution to each pint of the soup; and the same plan will enable us to keep jellies, which ordinarily decompose so rapidly in the organic germ-laden air of the sick-room, for many days unimpaired; these are, in my opinion, considerations of some moment in all circumstances, but most especially in the habitations of the poor.

Clothes or matting, soaked in the same solution and hung up, act as disinfectants of the most effective kind, and do not exhale the peculiarly unpleasant odour of carbolic acid, or the irritating vapours, so distressing to the bronchial system, of chloride of lime.

I have successfully employed the bisulphite of calcium for the preservation of numerous anatomical and other specimens, as it does its work perfectly, and without occasioning the great changes of colour and contraction of muscular structure so frequently produced by ordinary antiseptics; moreover, its special advantage over the preparations of mercury and arsenic lies, to my thinking, in the fact that it is not poisonous, and can therefore be handled with perfect safety.

There are numerous substances employed in pharmacy,—such as musk, castoreum, lard, and other fatty matters,—which are more or less injured by decomposition or keeping for any length of time. To these the bisulphite can be applied with considerable advantage.

Notes on the Use of the Microscope, and its Crystallographic Application. By W. W. Stoddart.

After referring to the history of the microscope, the author spoke of it as a source of the greatest assistance in saving time by indicating what the chemist afterwards verifies with his reagents.

The analytical chemist will tell you to the uttermost part of a fraction the proportion of C, H, O, Ca, K, etc., but he cannot tell in what state of combination they existed till the lens showed the granules of starch or the vegetable cell. The mineralogist would know that his tripoli was silica and alumina, but how could he possibly guess that it was composed of myriads of elegant and most beautifully sculptured vegetable skeletons? So with the retail chemist; how (without the microscope) would he be able to tell that his wholesale brother had been putting bean-flour with his fænugreek, or lignum vitæ with his jalap?

A good example of the large amount of knowledge obtainable in a short time, and very commonly required from the dispensing chemist, is in the examination of urine or urinary deposit. We will suppose a clear example to be given with no apparent deposit. Evaporate and ignite a few drops on a bit of platinum foil. While this is going on put a drop of the secretion on a glass slip with a very little nitric acid, when in a few minutes crystals will appear, which under the microscope show the well-known rhomboids of *nitrate of urea.*

Examine another drop as it is under a quarter-inch lens, when *oxalate of lime*, epithelial scales, etc., may be detected. Now dissolve off the ash left on the foil with a drop or two of distilled water. Place a drop on two glass slips. To the one add the smallest quantity of ammonia, and dry. The lens will then show *phosphate of lime, triple phosphate*, and *chloride of sodium.* To the other drop add bichloride of platinum, and evaporate to dryness. If *soda* be present, you will have acicular crystals of the platino-chloride; or if *potash* be there, you will find cubical crystals of the corresponding salt. Thus in a few minutes, by the aid of the microscope, no less than seven distinct salts may be readily detected, besides a great number of others.

Few fluids can be found, whether natural or artificial, whether a secretion or a chemical solution, that do not contain substances which, by some means or other, may be made to separate as crystals. Now, as these crystalline attributes are so universal and constant, the author has founded on it his method of determining the name and nature of the crystalline constituents of a given fluid,—a method by which, without any chemical test, but simply a microgoniometer, the name may be determined.

Crystals may be obtained from a given solution for microscopical purposes in six different ways, no matter how small the quantity may be:—

1. By simple deposition by cooling; as the well-known triple phosphate, so often seen in animal secretions.

2. By precipitating salt in a comparatively insoluble form; as the sulphocyanide of strychnia or bitartrate of potash.

3. By fusion; as in the case of salicine and several of the alkaloids.

4. By galvanic deposition; as in the detection of lactic acid.

5. By sublimation; as in arsenious and benzoic acids, thein, etc.

6. By evaporation.

In all the previous methods the object usually is to obtain *separate* and characteristic crystals, whose natures are only to be known by their peculiar form or conformative testing. By the mode now to be described the author has obtained certain results which, as they have not hitherto been published, he wishes now to lay before you, hoping that to some they may prove useful.

A drop of the given solution is placed on a glass slip, and slowly evaporated over the flame of the spirit-lamp, or in a drying chamber. A crystalline residue is left which, to the eye only, appears simply a magma of crystals without any definite arrangement. From a careful study of these, considerably magnified, the author noticed a certain arrangement of lines peculiar and constant to every salt. Again, on every slide it will be noticed that two angles always predominate over the others, and that the same salts have these two angles invariably the same. It is thought, therefore, that a table might be constructed from these angles, so that a measurement and reference to the table would give the name of the salt.

The goniometers used by the author are that made by Ross and that invented by Dr. Leeson; the latter being more correct while the former is more easily used.

Ross's goniometer is a positive eye-piece, across the field of which is a very fine line, the whole being made to revolve in a circle very finely graduated. When used, the engraved line is placed over or parallel to one side of the angle to be measured. The line is then revolved by means of the rack-work till it coincides with the other side of the angle, when that portion of the graduated arc traversed by the vernier gives a very correct measurement of the angle required.

The beautiful instrument of Dr. Leeson is an ingenious application of the phenomena of double refraction. It is equally adapted for measuring opaque or transparent crystals, microscopic or the largest crystals. It consists of a double refracting prism of Iceland spar, which is mounted over the eye-piece, and the whole fitted into a very finely-divided circle. When, therefore, the crystal is viewed through this prism two angles are produced, which revolve round each other as the prism is revolved. The amount of rotation, when applied to the angle, gives the measurement required.

The author feels that he has not worked out the subject as it deserves; indeed, so much more work remains to be done, requiring more time than he has at his disposal, that having made public the *modus operandi*, he hopes some one will continue its development.

Table of Angles.

Name of Crystal.	Predominating Angles.	
Sulphate of magnesia	120° 4′	105°
Bicarbonate of potash	90°	128° 5′
Nitrate of potash	76° 30′	103° 30′
Ammonia alum	90°	120°
Tartaric acid	97° 10′	83° 30′
Oxalic acid	74° 2′	106° 8′
Cholesterine	79° 30′	100° 30′

Mr. C. Kerr, of Dundee, read a paper on the interference of the excise in the sale of quinine wine, observing that something must be radically wrong, when chemists were made to pay license for making medicated wine with British wine, and not for making them with foreign wine; and now that quinine wine is a preparation of the British Pharmacopœia, he proposed that the Excise Board be communicated with on the subject.

"*On Granular Charcoal.*" By Wentworth Lascelles Scott, F.C.S., etc.

For some years past the value of charcoal, for internal use, has been gradually more and more recognised, and probably it would have been employed to a still greater extent, but for some little difficulties in the way of its convenient administration.

I may truthfully claim the originality and priority as regards granular charcoal, as it is now many years since my first experiments were made with this preparation, with the kind assistance of my friend the late Mr. Frank B. Fowler.

Granular Charcoal has the several advantages of being a definite preparation, easy of administration, and not liable to alter by keeping.

I prefer to use box, willow, or lime-tree wood for conversion into charcoals for medical purposes, merely on account of their texture and absorptive powers; and the carbonised matters, when free from all volatile substances, should be cooled out of contact with air, and boiled for some time in a dilute solution of hydrochloric acid, followed, after copious washings with pure distilled water, by a little weak ammonia.

The dried fragments of charcoal thus purified are then ready for a second ignition, which may be effected in tubes, cylinders, or retorts of metal or porcelain; after which, and before they are cold, they must be quickly pulverised and passed through a sieve of from 80 to 100 apertures to the inch.

Nine pounds of this finely-divided carbon may then be intimately mixed with one pound of pure sugar (which has been passed through a No. 30 sieve), and about four ounces of arabine or gum acacia in the state of impalpable powdere The whole should next be slightly moistened by means of an Atkinson's diffuser, or other similar instrument, with a few ounces of warm distilled water, to which has been added about 1½ ounces of tincture of benzoin, and a little mucilage; it is then ready for granulation, which is effected upon a flat steam-pan in the usual manner, at a temperature of 215° to 225°; a little extra care and attention should be given to the manipulation in granulating charcoal, as compared with other preparations; an additional rolling kind of action being required, which is readily learnt after a few trials.

The charcoal should be sifted when perfectly dry, and while yet warm, and secured in well-stoppered bottles or jars. I would recommend sieves of Nos. 6 and 16 gauze respectively.

Granular charcoal, when properly made, should possess a hard compact structure, and a sweet and slightly aromatic taste; it should not soil the fingers when dry, but must disintegrate very quickly without exhibiting any gritty particles in the presence of moisture; further, its integral porosity is by no means destroyed, as good granular charcoal may absorb fully eight and a half times its volume of sulphuretted hydrogen at ordinary temperatures, and proportionate quantities of other gases.

It is to this very property of the absorption or liquefaction of gases by charcoal, that I wish to draw your attention for a few moments. We all know that upon this alone, or very nearly so, depends the value of charcoal as a disinfectant and as an oxidiser, in whatever way it be employed, and we are very generally acquainted with the fact, that its power of taking up many of the easily liquefied or more soluble gases is very great indeed. As an instance, take ammoniacal gas; in the generality of scientific manuals and text-books some notice is taken of this, but in very loose terms, the amount of absorption being variously given up to "about ninety times the volume" of the charcoal itself, while my own experiments show that charcoal is capable of absorbing no less than 122 volumes of ammonia.

Now, putting aside certain collateral points for the moment, we may state generally that charcoal is taken internally, for the purpose of absorbing and masking the action of any acidulous and soluble gases that may be present in excess, thereby preventing or greatly diminishing their injurious action. Granting its usefulness in this respect, the question immediately arises, why not sometimes reverse the proposition? Why should not charcoal be made the carrier of gaseous bodies suited for the treatment of certain forms of disease, but which, under all ordinary methods, are either impossible or very difficult to administer?

My late experiments have been directed towards this question, and I am decidedly of opinion that charcoal, saturated with various gases, may hereafter become useful remedial agents. The subject is naturally one which cannot be treated lightly, and which requires some extended and patient labour for its proper development, but as far as I have already gone, the results are, in my opinion, most encouraging.

"*Analysis of Ordinary Commercial Specimens of Jalap, showing their relative Value in proportion of Resin of Jalap compared with market price.*

By Mr. ALFRED SOUTHALL, Birmingham.

	Description.	Resin.	Market Price.
No. 1 ..	Jalap tops ..	5 per cent. ..	4*d.* per lb.
" 2 ..	" " ..	12 " ..	5*d.* "
" 3 ..	" Tampico	9½ " ..	10*d.* "
" 4 ..	" "	10½ " ..	1*s.* 0*d.* "
" 5 ..	" "	30½ " ..	1*s.* 0*d.* "
" 6 ..	" "	29 " ..	1*s.* 6*d.* "
" 7 ..	" "	12½ " ..	1*s.* 6*d.* "
" 8 ..	" "	33¾ " ..	2*s.* 0*d.* "
" 9 ..	" "	27 " ..	2*s.* 0*d.* "
" 10 ..	" Vera Cruz	15½ " ..	4*s.* 0*d.* "
" 11 ..	" "	17½ " ..	4*s.* 0*d.* "
" 12 ..	" "	17½ " ..	4*s.* 0*d.* "
" 13 ..	" "	12½ " ..	4*s.* 0*d.* "
" 14 ..	" "	23 " ..	4*s.* 4*d.* "
" 15 ..	" "	20½ " ..	4*s.* 6*d.* "
" 16 ..	" "	16¾ " ..	4*s.* 10*d.* "

In order to ascertain the medicinal value of the supplies of jalap, as found in the shops of pharmaceutists, I procured five specimens of powdered jalap at different establishments, and found the result, in percentage of resin, as follows:—

No. 1,	13 per cent.	of resin.
" 2,	15 "	"
" 3,	9½ "	"
" 4,	16½ "	"
" 5,	17 "	"

The commercial value of jalap imported from Tampico is much inferior to the kind imported by way of Vera Cruz, but an average of seven samples of each kind here analysed, show that the Tampico is richer in resin than the Vera Cruz; the average in the one case being about 22 per cent., and in the other 17½ per cent.

I have made an experiment with the purgative effects of the two varieties, and find them much the same. The resin from Tampico jalap is somewhat darker than that from the Vera Cruz variety, and has a distinctive peculiarity of smell, but I have not discovered any difference in chemical character.

PHARMACEUTICAL SOCIETY.

FIRST MEETING OF THE SESSION.

Wednesday, October 2, 1867.

T. H. HILLS, Esq., Vice-President, in the Chair.

SEVERAL donations to the library and museum were announced, and the thanks of the meeting given to the donors. Amongst them were some well executed drawings by Mr. Brady, illustrating the appearance of quinodine, the urinary deposits, etc., under the microscope.

The CHAIRMAN then proceeded to present to the successful competitors the prizes and certificates of merit awarded at the conclusion of the last session, first calling upon the Professors to report the results of their respective examinations.

Professor REDWOOD reported the results of the examinations in botany, also alluded to the exemplary conduct of the pupils, as well as to the progress they had made.

Professor ATTFIELD reported the results of the examination in chemistry and pharmacy, and in doing so he spoke very highly of the conduct of the class, both as regards attendance and behaviour.

Professor BENTLEY, in reporting the results of the examination in practical chemistry, which were very satisfactory. In speaking of examinations, he said they were not the best proofs of a man's qualifications, although they are the best we possess. Knowledge gained by practical experiments was more lasting than that derived solely from books.

The CHAIRMAN then gave some excellent advice to those who had received the prizes, speaking of the good they would receive by attending the meetings and rallying round the Society. He had attended the meetings regularly for a number of years, and never went home without deriving some good.

Mr. MORSON made some remarks upon a remarkable case of the crystallisation of borotartrate of potash. A solution of borotartrate of potash had been placed in a bottle previous to its being converted into scales. On examination it was found to have become solid, and retained the shape of the bottle, which was broken. He had mentioned it to Dr. Redwood, who would give them the results of his experiments.

Professor REDWOOD said that borotartrate of potash, or soluble cream of tartar, was considered an uncrystallisable substance. It was obtained by making a solution of boracic acid, or borax, and cream of tartar, and evaporating to dryness, or, if required in the form of scales, it was evaporated to a syrupy consistence and laid upon plates. He had examined it under the microscope, and made several experiments, but had not been able to discover why it had assumed such an unusual form.

Dr. ATTFIELD made some remarks, in which he suggested that a quantitative analysis might throw some further light on the subject.

ACADEMY OF SCIENCES.

SEPT. 30, 1867.

(FROM OUR OWN CORRESPONDENT.)

The Pascal-Newton Forgeries.—Sulphuric Acid in Living Mollusca.—Transformation of Wood Spirit into Aldehyd.—Experiments on Projectiles.

THE secretary, M. Coste, read a letter, on the Pascal documents, from Mr. Robert Grant, Regius Professor of Astronomy at the University of Glasgow, transmitted by M. Le Verrier, and inserted also in the *Times*. The learned author of the history of physical astronomy demonstrated invincibly —and we agree with him—that the numbers expressing the masses of the sun, the earth, Jupiter and Saturn, the densities of these bodies, and the force of gravity at their surface, that are found in Pascal's notes, were copied from the third edition of the *Principia* given by Newton in 1726; and from this simple fact Mr. Grant concludes that *all the mass of documents communicated by M. Chasles to the Academy of Sciences are forgeries.* Evidently this conclusion is not contained in the premises, and Mr. Grant errs against the rules of logic. He ought to have confined himself to his first alternative: either Pascal had received from an unknown observer the elements of calculation identical with those of Newton; or, the numbers of his note had been simply copied from the third edition of the *Principia*; and this note is not in Pascal's handwriting.

M. Chasles confessed that it is difficult to explain the coincidence pointed out by Mr. Grant; but his letter does not invalidate the capital fact of the relations between Pascal and Newton, corroborated by irresistible proofs. Here is a very striking one. In one of his letters to Huyghens, Pascal defined the quantity of movement, and gave as its value the product of the mass by the square of the velocity, and deduced from this expression different practical conclusions. Huyghens, in his answer, expressed to Pascal the fear of an error on his part. The quantity of movement, he said, was proportional, not to the product of the mass by the square of the velocity, but to the product of the mass by the velocity. The two letters of Pascal and Huyghens are in the hands of M. Chasles; also, a letter in which Newton communicates to Huyghens this same definition of the quantity of motion, and the answer of Huyghens affirming that he formerly pointed out to the late M. Pascal the error into which he had fallen. The close approach of these four letters, which cannot have been forged, leave not the least doubt as to the communications made by Pascal to Newton. The same relations between these two great geniuses is still more evident from the very curious and extraordinary series of letters, or projects of letters exchanged between Newton, James II., and Louis XIV. We again repeat that the mass of proofs in the possession of M. Chasles is so overwhelming that we are forced to yield before the evidence.

M. Coste read also a letter in which Sir David Brewster, to whom M. Chasles had sent four of MS. notes of Newton, gave an account of the comparisons made by the Earl of Portsmouth, the Earl of Macclesfield, and Sir Frederick Madden, between these notes and authenticated letters and signatures of Sir Isaac Newton. The conclusion of this examination shows that not only were the letters forgeries, but that the forger could never have seen the writing or signature of Newton otherwise than in the general Dictionary, or in the Macclesfield correspondence published in 1841. M. de Khanikof, as ocular witness of one of these comparisons, had reported to M. Chasles that the cause of this judgment was principally owing to the permanent difference between the *e* and the *n* of the notes from those in the authentic documents; now, in searching anew in the immense collection of M. Chasles, he found notes in which the *e* and *n* were the same as those in the English MSS., and others in which the same letters have a different form in the English documents, and in the French notes placed by M. Chasles in the hands of Sir David Brewster; the two forms are also found in a Latin letter of Newton, of which Father Secchi has brought a facsimile from Geneva. In fine, the handwriting of a man varies with time; the differences between the writing of the same hand corresponding turn by turn in English, French, German, and Latin are sufficient to account for all the differences pointed out by the friends of Sir D. Brewster, and remove any serious objections raised by these comparisons. The fact that Newton had four or five different signatures, faithfully found in the notes of M. Chasles, and the extremity, to which Sir David Brewster is reduced, of pretending that the forgeries are recent and posterior to 1841, when speaking of the documents from the collection of M. Desmaizeau, are powerful arguments in favour of the authenticity of the autographs of M. Chasles.

M. Dumas communicated a curious note, by which M. de Luca determined in the liquid contained in living mollusca the presence of a thirtieth part, or about 3 per cent. of pure sulphuric acid; and stated also, that the same mollusca plunged in water disengages a considerable quantity of carbonic acid.

M. Dumas also laid on the table a work of great interest by Dr. Hofmann, on the transformation of wood-spirit into aldehyd, a problem which MM. Dumas and Peligot had vainly attempted to solve. Dr. Hofmann placed in a sufficiently long tube a spiral of platinum, which he raised to the temperature of incandescence by means of a voltaic current, then he traversed the tube by a continuous jet of the vapour of wood-spirit; this vapour is sufficiently heated to be decomposed, and transformed into aldehyd, which can be collected in the form of a continued stream; the operation can be continued for several days, and it has been proved that more than two-thirds of the wood-spirit is converted into aldehyd.

M. Dumas resumed the very original experiments made by M. Melsens on projectiles. By causing a leaden ball to fall into water from the height of about a mètre, he found that the ball drew along with it twenty times its volume of air. This same ball projected several mètres, by powder, to the interior of a cylinder, filled with water, the two vertical openings of which are shut by diaphragms of plaster, introduced into the cylinder nearly a hundred times its volume of air. If the initial velocity is small, the hole is about the same size as the ball (11 millimètres); by increasing the velocity it is much enlarged in size, and when considerably increased the hole becomes enormous. It is impossible to assign the cause of the increase of the hole to the ball alone. Also, when the velocity of projection is excessive, there is a double border inside and outside formed round the holes where the ball enters and quits.

October 14, 1867.

Lever Barometer.—New Mountain Barometer.—Solar and Lunar Halos and Coronæ.—The Pascal-Newton Forgeries.—Solar Spots.—Water Conduits.

The discussion between M. Radau and the Rev. Father Secchi on the lever barometer is not yet terminated. At the last meeting, M. Secchi tried to prove that the barometer did not require any thermometric corrections. M. Radau now affirms that Father Secchi, in his demonstration, has confounded the dilations produced by 10° with those produced by 1° C.; the consequence is that the calculated numbers are false. In reality, the error of the barometer is more than a millimètre for 10°, instead of being the hundredth part of a millimètre, as M. Secchi affirms.

M. F. de Bruno, Professor at the University of Turin, presented a barometer formed of two or three concentric tubes of glass or cast iron—a sort of lever barometer which possesses the considerable advantage of being able to be transported without great danger of rupture, or the loss of the vacuum. His idea is simple and ingenious.

M. A. Decharme, Professor at the Imperial Lyceum of Angers, observes with much care the great and small solar and lunar halos and coronæ; he has ascertained that—1st. At Angers these meteoric phenomena are more frequent than is generally believed. From the 30th August, 1866, to 30th August, 1867, he had observed 33. 2nd. In all cases they were followed by rain or snow on the same day, the day following, or, at latest, on the next day for a very small number. 3rd. That in general the rain is the nearer and more abundant in proportion to the brilliancy of the phenomenon. The study of halos can thus furnish precious indications as prognostics of the weather.

M. Faugère writes to the Academy a letter truly incredible; also a comparison of Pascal's notes with the only page of the illustrious philosopher which was at his disposition, and has very roughly concluded the falsity of all the documents of M. Chasles. At present the comparison of a letter of King James, kindly placed in his hands by M. Chasles, with the fac-simile of a similar letter found by him in a printed work, he declares apocryphal to the letters of M. Chasles. He does not even remark, as M. Morin observes, that often the letters of sovereigns are neither written nor signed by them, and that they are not the less authentic documents. He asks simply that all the autographs of M. Chasles be submitted to an enquiry confided to the direction of the Imperial Printing Establishment, and to the most learned connoisseurs of its administration. M. Chasles declares formally that he denies the competence of M. Taschereau, whose ability in discernment of autographs appears to him to be doubtful; that he is disposed to publish the entire collection of these documents. Meanwhile he places them at the disposition of all those who wish to consult them, but that they shall not leave his possession. He remarked that this enquiry has been already made, for several of the most important pieces with those of the British Museum, the Royal Society of London, by many amateurs, by the aid of a fac-simile taken from the most authentic autographs, and the comparisons have successfully overruled all the objections put forward. M. Chasles reverted also to the origin of his autographs; he thinks that he can satisfy to all purposes the indiscreet questions that have been put to him in affirming that they formed part of the collection of Desmaizeau. He is convinced that by indicating the series of hands through which they passed into his own, no step would be gained as to proving their authenticity. Seven portfolios of the same collection—that of Desmaizeau—have remained in London, where he died, and which may be found in the British Museum or elsewhere by proper search, which will serve to demonstrate most forcibly the authenticity of the last portfolio remaining in France, for the possession of which English amateurs have made vain endeavors since the death of Desmaizeau. It seems to us that he can say nothing more explicit; and the search for the portfolios of Desmaizeau is the only reasonable way of putting an end to this painful discussion. Let the Royal Society of London order a commission of its members to examine seriously the autographs in the possession of M. Chasles. We repeat that the falsifying of these thousands of documents is an utter impossibility. And the fact that the pretended forger makes the relations between Newton and Pascal date as far back as the time when Newton was only 14 years old, is in itself a convincing proof of their authenticity.

M. Le Verrier made a discourse in which he tried to demonstrate, in the most positive manner, the false origin of the astronomical documents attributed to Pascal. The President, and the Secretary, M. Elie de Beaumont, begged of him to reserve his proofs for the commission to which was referred the proposition of M. Faugère. M. Le Verrier insisted on being heard by the Academy, inasmuch as that the overwhelming proofs in his possession will arrive soon from England; but the language of M. Le Verrier was so far from being academical, and so injurious to M. Chasles, that it was received with marked signs of disapprobation by the whole assembly. M. Saint Claire Deville read a letter, in which M. Kirchhoff answered to a question made by M. Chasles, as to his theory of solar spots.

Father Secchi read a paper on the admirable water conduits made in the Roman Compagna with full success.

MANCHESTER LITERARY AND PHILOSOPHICAL SOCIETY.

Ordinary Meeting, October 1st, 1867.

Edward Schunck, Ph.D., F.R.S., *etc., President, in the chair.*

Dr. Crompton, alluding to the paper he read in October, 1866, "On the Portraits of Sir Isaac Newton," said, that while preparing his essay for publication in the Memoirs of the Society, he had opened up fresh sources of information, and become possessed of facts of considerable interest respecting the portraits of Newton. He had examined about twenty portraits of the great philosopher, all of which were considered to be originals, and most of which were undoubtedly painted a long time ago; but he had seen no portrait which, in his opinion, was of equal importance and interest with the Kneller Newton of 1689, to which he last year directed the attention of the Society, as by far the most valuable portrait of the great philosopher in existence, and of which he exhibited an admirable engraving by Mr. Oldham Barlow. His extended inquiries into the subject of the portraits of Newton had led him to the conclusion that there are several (if not many) which pass current as portraits of him which are most decidedly representations of other persons. In the National Portrait Exhibition at Kensington of the present year there were four portraits of Newton, two of which he feels sure have no real claim to be regarded as authentic or genuine. These two are pictures contributed by the Earl of Dartrey and by the Marquis of Exeter. The former purports to be a portrait of Newton when he was a Bachelor of Arts, and to be painted by Lely. It represents a young man with his hand resting on a globe; and there is an engraving of the picture, done many years back, but he had no hesitation in saying that the picture had no right to be considered a portrait of Newton. The features were not Newton's, and it was most improbable that the poor Trinity College Sizar would, when a Bachelor of Arts (that is, between January, 1665, and July 7, 1668) have cared, or, if he had cared, could have had the means to obtain a portrait of himself. He apprehended that because the portrait represented a young man with his hand on a globe, some imaginative person had supposed that it must be a representation of the great philosopher who had explained the system of the universe. But this portrait represents one of the poorest specimens of humanity; and certainly not a Newton, but rather a Simple Simon. The Marquis of Exeter's picture has hardly any greater claim to be regarded as a Newton. It represents a man with a bald head, but we have the clearest evidence that Newton was not bald.[*] The two Vanderbank portraits, representing him the year before his death without his wig, show that he had a beautiful head of silver white hair. There is another portrait of Newton by Thornhill, taken at an earlier period, which represents him without his wig, but with short white hair and no trace of baldness. The Portsmouth Kneller of 1689, which represents him when he was 47 years old, shows him without a wig and with abundant grey hair. This is a sufficient ground for rejecting this picture; but no one who has studied the portraits of Newton could be brought to believe that this is a representation of the great philosopher. Last year, when reading his paper, Dr. Crompton remarked that he had been unable to discover where the original Kneller of Newton, engraved by Houbraken, was to be found. He then pointed out that in some impressions of the Houbraken print it was stated to be in the possession of Mr. Conduit. Dr. Crompton went to the Earl of Portsmouth's seat at Hurstbourne Park, and there found the original picture. It is dated 1702 and signed by Kneller. The Earl of Portsmouth, Newton's collateral descendant, therefore possesses the two most important portraits of Newton. This portrait of Newton was engraved also by Smith, about 1712. A replica, if not an earlier copy, without any name of an artist upon it, is in possession of the Duke of Devonshire at Holker. His Grace informs me that he cannot positively trace its coming into the possession of his family, but he conjectures that it may have been at Holker before the marriage of one of the Lowthers with Lady Mary Cavendish, through whom this Holker property passed to the Cavendishes. There is an exact replica or early copy of the Portsmouth Kneller of 1689 at Lord Galway's, except not having the name of Newton or of Kneller upon it, which Dr. Crompton has examined, and which, in an old catalogue of the pictures, is said to be painted by Bond. Dr. Crompton said that he had examined the three oil paintings of Newton in the Royal Society's collection; also the Kneller at Hampton Court, which is dated 1689, and differs from the Portsmouth Kneller of 1689 only in the position of the hands; also all the portraits of Newton belonging to Trinity College, Cambridge; the Indian Ink drawing of Newton in the Pepysian library at Magdalen College; and a portrait at Mrs. Miles', at Firbeck Hall, Yorkshire, which resembles very much the Egremont Newton by Kneller, engraved as a frontispiece to Sir David Brewster's first life of Newton, published in the Family Library. Dr. Crompton exhibited this edition of Brewster's life, as well as the larger one in two vols. In the text of both the engravings, which differ very greatly from

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[*] Mr. Conduit, Newton's nephew by marriage, when describing Newton's personal appearance, says, "With a fine head of hair as white as silver, without any baldness, and when his peruke was off, was a venerable sight."—Brewster's *Life of Newton*, 1855, vol. ii. p. 413.

each other, are said to be from a picture in the possession of Lord Egremont. Sir David Brewster is unable to explain how it is that two engravings from distinct and different portraits should thus happen to be spoken of in the text, except that his publisher must have selected the second portrait and got it engraved. It is, I think, unmistakeably taken from Smith's print of 1712 or an early impression of Houbraken's engraving. The portrait prefixed to the small Life of Newton is the same as that engraved in Lodge's portraits. At present Dr. Crompton is unacquainted with the historical evidence regarding the genuineness of this portrait. It is signed by Kneller and dated 1716; but Dr. Crompton has not yet seen it, nor one in the possession of Mr. Turner, of Stoke Ashford, near Grantham.

In Lord Portsmouth's collection at Hurstbourne, besides three genuine and authentic portraits of Newton, there is a fourth picture with the name of Newton painted upon it. It was described to Dr. Crompton by a gentleman who had had an opportunity of a very close examination of it, as the earliest and most important portrait of the great philosopher. But Dr. Crompton found neither date nor artist's name upon it, and the picture, which is mounted upon panel, has a crest in red wax behind it, which is not the crest of the Portsmouth family, but could not be sufficiently determined what it was for want of a magnifying glass. The history of this picture will be investigated further; though it is certain that it cannot be a portrait of Newton, for the features are not his, and the eyes are brown, while Newton's were bluish grey. It is most earnestly to be desired that all the portraits of Newton might be collected together at Kensington next year for comparison with each other, and that the portraits of other great men (where there are several) should be thus exhibited in juxtaposition. It would probably then be evident that there exist many spurious ones, and an opportunity would be thus afforded of determining which are the best as well as the true.

Mr. R. D. DARBISHIRE, F.G.S., referred to a paper "On the Existence of a Seabeach on the Limestone Moors near Buxton." (*Trans. Manchester Geol. Soc.*, v. p. 273), in which Mr. John Plant, F.G.S., had described as sea beach the surface of the limestone rock as the same is seen when bared of sward and surface clay above the quarries on Grin Edge and Harper Hill, south-west of Buxton, and to Mr. Plant's conjecture that this worn surface probably extended nearly to the crown of the hills.

His own observation had marked on each hill, above the stratum whose upper surface exhibited those indications of wear, a stratum of somewhat different texture still subsisting in the shape of a slight vertical cliff or reef. This bed had not worn in the same manner as the "beach," that is to say, with many interlacing fissures having a close *chevaux de frize* of limestone points, but rather in great blocks with round, curved edges or holes.

In connection with this bed, Mr. Darbishire had obtained specimens from each hill exhibiting what he believed to be the remains of the burrows of Pholas shells.

On the top level of Grin Edge, close to the ruins of the tower, one stone had a group of seven holes. They were placed like Pholas holes as he had collected them on Great Orme's Head; and, though the surface of the stone about them was much worn, taken along with the specimen next described, it seemed more fitting to ascribe to them a similar origin, than to attribute them to the natural wear of the stone, notwithstanding the variety and singularity of many of the forms in which atmospheric or aqueous corrosion affect the limestone rock.

This stone lay amongst a heap of others near the ruins of the tower, and had doubtless been brought up a few feet. The height above the sea of the tower is stated on the Ordnance Map as 1.435 feet.

On Harper Hill, in two large blocks of the overlying stratum, he had detected more characteristic holes. Both blocks were lying in the sward above the "beach" surface, and were a few feet below the rock *in situ*, from which they had evidently been detached. Of the first of these specimens he exhibited a photograph. It showed in the underside of the edge of a projecting ledge or table of stone six well marked holes from ½ in. to 1 in. in diameter, and one an inch deep, and traces of two others. The holes were grouped just as Pholas holes usually are, and apparently were quite independent of the structural fissures of the stone.

According to measurement with an aneroid barometer, the stone in which those holes occurred was about 1,380, and the reef from which it had fallen about 1,400 feet in elevation.

If the holes were really Pholas burrows, they would indicate the elevation of these hills since the period of glacial action by sea or land.

Mr. BINNEY, F.R.S., F.G.S., remarked on the great elevation of these remains if the observations were accurate, and observed that they were on the west side of the hills. Mr. Prestwich had discovered shells in shingle on the western slopes of the Axe-edge hills towards Macclesfield, at the height of 1,150 feet. He would like to hear of observations on the eastern side of the Derbyshire hills if any traces of marine action were to be discovered there.

Mr. DARBISHIRE had not the specimens at hand, but would produce them, in connection with a series of similar remains from Orme's Head, to which he proposed to call the attention of the Society at an early meeting.

CHEMICAL NOTICES FROM FOREIGN SOURCES.

Benzylic Ether, Nitro-derivatives of.—Ed. Grimaux. Nitrobenzylic hydride dissolves in alcoholic potassic hydrate, with formation of potassic nitrobenzoate, and an oil which probably is nitrobenzylic alcohol. The reaction takes place in the following manner:—

$$2[\text{Ȼ}_7H_6(N\Theta_2)\Theta]+KH\Theta= \\ =\text{Ȼ}_7H_4(N\Theta_2)\Theta_2K+\text{Ȼ}_7H_7(N\Theta_2)\Theta.$$

This oil cannot be distilled at ordinary pressure without undergoing decomposition; in a vacuum it boils between 178° and 180°C. Phosphoric chloride converts it into nitrodracethylic chloride (the nitrobenzylic chloride of Beilstein and Geitner, *Ann. Chem. Pharm.* cxxxix. 337), which, on being boiled with an alcoholic solution of potassic acetate, forms the ether $\text{Ȼ}_6H_4(N\Theta_2)\text{Ȼ}H_2(\text{Ȼ}_2H_3\Theta_2)$.—(*Comptes R.* lxv. 211.)

Orcin, Methyl-, Ethyl-, and Amyl-derivatives of.—V. de Luynes and A. Lionet. The action of alcoholic-iodides and potassic hydrate upon orcin gives rise to the formation of three series of compounds, in which, according to the conditions of the experiment, one, two, or three atoms of hydrogen of orcin are replaced by alcohol-radicals. The authors have prepared by this method—

1. Methyl-orcin,	$\text{Ȼ}_7H_7(\text{Ȼ}H_3)\Theta_2$,
ethyl-orcin,	$\text{Ȼ}_7H_7(\text{Ȼ}_2H_5)\Theta_2$,
and amyl-orcin,	$\text{Ȼ}_7H_7(\text{Ȼ}_5H_{11})\Theta_2$.
2. Diethyl-orcin,	$\text{Ȼ}_7H_6(\text{Ȼ}_2H_5)_2\Theta_2$,
and diamyl-orcin,	$\text{Ȼ}_7H_6(\text{Ȼ}_5H_{11})_2\Theta_2$.
3. Trimethyl-orcin,	$\text{Ȼ}_7H_5(\text{Ȼ}H_3)_3\Theta_2$,
triethyl-orcin,	$\text{Ȼ}_7H_5(\text{Ȼ}_2H_5)_3\Theta_2$,
and triamyl-orcin,	$\text{Ȼ}_7H_5(\text{Ȼ}_5H_{11})\Theta_2$.

—(*Comptes Rendus*, lxv. 213.)

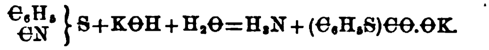
Glycolic Hydriodate, and New Synthesis of Alcohols.—A. Butlerow and M. Osokin. Glycolic hydriodate (iodhydrin) $\text{Ȼ}_2H_5I\Theta$ is readily formed by the action of potassic iodide upon glycolic hydrochlorate; potassic chloride and iodide are extracted from the mixture by water; the remaining oil is washed first with a solution of sodic hydrate, then with water, and finally dried over dehydrated sodic sulphate, and distilled in a vacuum. Iodhydrin is energetically acted upon by zincic methide or ethide, and if water is added to the product of the reaction, iodhydrin is again formed, besides hydrocarbons:

$$\left.\begin{matrix}\text{Ȼ}_2H_4I\\RZn\end{matrix}\right\}\Theta+2H_2\Theta=\text{Ȼ}_2H_5I\Theta+RH+ZnH_2\Theta_2$$

If, however, certain conditions are observed during the reaction, the alcohols $Ꞓ_3H_8Ɵ$ and $Ꞓ_4H_{10}Ɵ$ may be obtained.—(*Zeitschr. Chem.* N.F. iii. 369.)

Acetonic and Oxyisobutyric Acid.—Markownicoff has prepared acetonic acid according to Städeler's method, and compared it with oxyisobutyric acid. He finds that they agree as regards the temperature required for their sublimation (50°C.) and fusion (79°—80°), and that their characteristic zinc-salts show the same properties. From these experiments, and others published on a former occasion (Zeitschr. ii. 502), the author concludes that Frankland's dimethoxalic acid, Städeler's acetonic acid, and his oxyisobutyric acid, first obtained from monobromisobutyric acid, are identical.—(*Zeitschr. Chem.* N.F. iii. 434.)

Sulphophenyle—C. G. Wheeler. By the reduction of phenylsulphonic chloride by means of zinc and sulphuric acid, phenylic sulphhydrate and phenylic bisulphide are formed. On dissolving the mixture of the two in warm alcohol, the bisulphide is obtained in crystals, while the oily sulphhydrate remains in solution. Bromine acts readily upon phenylic bisulphide, forming a crystalline body of the composition $Ꞓ_6H_5BrS$, which is readily soluble in ether, moderately so in alcohol, insoluble in water. The author intends to convert this bromide into a cyanide, which on being treated with potassic hydrate is expected to yield an acid corresponding to oxybenzoic acid, and identical, perhaps, with the acid obtained from potassic sulphhydrate and chlorbenzoic acid. The reaction would be the following:—



$$\left.\begin{matrix}Ꞓ_6H_5\\ ꞒN\end{matrix}\right\}S+KƟH+H_2Ɵ=H_3N+(Ꞓ_6H_5S)ꞒƟ.ƟK.$$

(*Zeitschr. Chem.* N.F. iii. 436.)

Benzyl-Amides.—H. Limpricht. The action of alcoholic ammonia upon chlorbenzyl gives rise to the formation of mono-, di-, and tri-benzylamine, which may easily be separated by converting them into hydrochlorates and subjecting the latter to fractional crystallisation.

Tri-benzylamine crystallises in large plates; it may be distilled in small quantities at above 300°C. without undergoing decomposition. The chlorhydrates heated in an atmosphere of dry chlorhydric acid to 250°C. is resolved into chlorbenzyl and dibenzylaminic chlorhydrate:

$$(Ꞓ_7H_7)_3N,HCl+HCl=Ꞓ_7H_7Cl+(Ꞓ_7H_7)_2HN,HCl.$$

The free base, when distilled with bromine and water, gives oil of bitter almonds and bromhydrate of dibenzylamine:

$$(Ꞓ_7H_7)_3N+Br_2+H_2Ɵ=Ꞓ_7H_6Ɵ+(Ꞓ_7H_7)_2HN,HBr+HBr.$$

A similar reaction takes place when heated with iodine and water in sealed tubes to 120°. Dry bromine added to an etheric solution of tribenzylamine precipitates an amorphous yellow body of the composition $(Ꞓ_{21}H_{21}N)_2Br_6$. Fuming sulphuric acid converts tribenzylamine into a sulpho-acid, the baric salt of which has the formula $Ꞓ_{14}H_{12}NS_2Ɵ_6,Ba$.

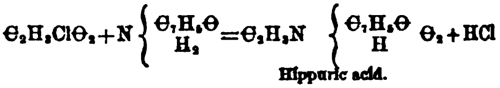
Dibenzylamine is a thick colourless liquid, insoluble in water, readily soluble in alcohol or ether. It forms well crystallisable salts with chlor-, brom-, and iodhydric acid, which are soluble in hot water, sparingly so in cold.

Monobenzylamine awaits further investigation, not having been obtained in sufficient quantity.—(*Zeitschr. Chem.* N.F. iii. 449.)

Hippuric Acid, Synthesis of.—N. Iazukowitsch. The reaction by which sarcosine (methl-glycocyne) is formed (acting upon chloroacetic acid with methylamine) may be employed for the synthesis of hippuric acid (benzoyl-glycocyne), using benzamid instead of methylamine:

$$Ꞓ_2H_3ClƟ_2+N\left\{\begin{matrix}ꞒH_3\\ H_2\end{matrix}\right.=Ꞓ_2H_3N\left\{\begin{matrix}ꞒH_3\\ H\end{matrix}\right.Ɵ_2+HCl$$

Sarcosine



$$Ꞓ_2H_3ClƟ_2+N\left\{\begin{matrix}Ꞓ_7H_5Ɵ\\ H_2\end{matrix}\right.=Ꞓ_2H_3N\left\{\begin{matrix}Ꞓ_7H_5Ɵ\\ H\end{matrix}\right.Ɵ_2+HCl$$

Hippuric acid.

Equivalent quantities of benzamide and chloracetic acid were heated in a sealed tube to 150°—160°C. The contents of the tube, which had become solid on cooling, were extracted with ether, the insoluble portion saturated with calcic hydrate, and the calcic salt analysed; it had the composition of calcic hippurate $2(Ꞓ_9H_8NƟ_3).Ca+3H_2Ɵ$, the acid obtained from this salt by precipitation with chlorhydric acid, had the composition of hippuric acid, $Ꞓ_9H_9NƟ_3$.—(*Zeitschr.* N.F. iii. 466.)

Nitric Acid in Water.—T. Fuchs determines nitric acid in water by the following method:—Two litres of water are boiled down to about 200 c.c., and during evaporation pure potassic permanganate is added (the object of which is to convert nitrites into nitrates), until a permanent pink colour is obtained. The concentrated liquid is filtered, pure sulphuric acid added, and distilled into a flask containing baric carbonate suspended in water. The distillation is interrupted when sulphuric acid begins to go over. The contents of the receiver are filtered, and in the filtrate, which contains baric nitrate and chloride, the barium is determined in the usual manner. The amount of chlorine being known from a separate experiment, all data are given for the calculation of the quantity of nitric acid present in the 2,000 c.c. water.

The error caused by the oxidation of ammonia to nitrous and nitric acid the author finds to be inappreciable.—(*Zeitschr. Analyt. Chem.* vi. 175.)

Phosphoric Acid and Nascent Hydrogen.—R. Fresenius. It has been stated by Herapath (*Pharm. Journ.*, vii. 57) that phosphoric acid was reduced by zinc and sulphuric acid, so that hydric phosphide was mixed with the hydrogen evolved. Fresenius tried the experiment, but with a negative result. 100 grammes of zinc were slowly dissolved in diluted sulphuric acid in presence of 10 grammes of sodic phosphate. The gases, after passing a small wash-bottle containing water, were conducted through two U tubes filled with a neutral solution of argentic nitrate. A small quantity of black precipitate was formed, which on examination was found to contain arsenic and silver, but not a trace of phosphorus in any form. A similar experiment in which no phosphate had been added to the zinc gave the same result exactly.—(*Zeitschr. Analyt. Chem.* vi. 203.)

Volumetric Determination of Iron.—A. C. Oudemans. The inaccuracies attached to the method of the direct determination of iron by means of sodic hyposulphite, as first proposed by Scherer, have been removed by the following modification of the process. To a solution of ferric oxide, which may contain much free chlorhydric acid, are added a few drops of a solution of a cupric salt, and potassic sulphocyanide sufficient to render the liquid dark red. A standard solution of sodic hyposulphite is then added until the red colour has entirely disappeared, which point may be observed with great accuracy. The action of the small quantity of cupric salt consists in causing a more rapid deoxidation of the ferric salt.

The analyses given in illustration of this method are very satisfactory.—(*Zeitschr. Analyt. Chem.* vi. 129.)

Hypogaeic Acid.—K. Schröder. The oil, extracted from the seeds of aracnis hypogaea by means of carbonic disulphide, was saponified with sodic hydrate. The soap decomposed with chlorhydric acid, and the mixture of arachidic, oleic, and hypogaeic acid subjected to repeated crystallisations from alcohol, until the last-named acid was obtained in a state of purity.

Hypogaeic dibromide, $Ꞓ_{16}H_{30}Br_2Ɵ$, is obtained by the action of bromine on the acid at a low temperature. This bromide, on being treated with alcoholic potassic hydrate at 100° C., is converted into monobromhypogaeic acid, $Ꞓ_{16}H_{29}BrƟ_2$;

when the reaction is made to take place under pressure and the temperature raised to 170°, palmitolic acid, $\Theta_{16}H_{28}\Theta_2$, a lower homologue of stearolic acid, is formed, the decomposition taking place according to the equation:

$$\Theta_{16}H_{30}Br_2\Theta + 2KH\Theta = \Theta_{16}H_{28}\Theta_2 + 2KBr + 2H_2\Theta.$$

Monobromhypogaeic acid again treated with bromine is converted into monobromhypogaeic dibromide,

$$\Theta_{16}H_{29}Br_3\Theta_2,$$

and this when digested with alcoholic potassic hydrate is changed into monobrompalmitolic acid,

$$\Theta_{16}H_{27}Br\Theta_2.$$

Fuming nitric acid oxidises palmitolic acid to suberic acid, suberic hydride, and palmytoxylic acid:

$$2\Theta_{16}H_{28}\Theta_2 + 7\Theta = \underset{\text{Palmytoxylic acid.}}{\Theta_{16}H_{28}\Theta_4} + \underset{\text{Suberic acid.}}{\Theta_8H_{14}\Theta_4} + \underset{\text{Suberic hydride.}}{\Theta_8H_{14}\Theta_3}$$

Freshly precipitated argentic oxide and water acting upon hypogaeic dibromide at 100° convert it into oxyhypogaeic acid, and the latter, on being boiled with an alkaline hydrate takes up water, forming dioxypalmitic acid:

$$\Theta_{16}H_{30}Br_2\Theta_2 + Ag_2\Theta = \Theta_{16}H_{30}\Theta_3 + 2AgBr,$$

and oxyhypogaeic acid

$$\Theta_{16}H_{30}\Theta_3 + H_2\Theta = \Theta_{16}H_{32}\Theta_4$$

dioxypalmitic acid.—(*Ann. Chem. Pharm.* cxliii. 22.)

NOTICES OF BOOKS.

The Calendar of the Pharmaceutical Society.

PHARMACEUTISTS will find this book of great value as a reference on all matters connected with the Society. In addition to the list of members and associates which was formerly published in the July number of the Journal, it contains the various Acts of Parliament relating to Pharmacy, the regulations of the Board of Examiners, the rules of the Benevolent Fund, and much that will be useful to those interested in Pharmaceutical matters.

CORRESPONDENCE.

The Soda Trade.

To the Editor of the CHEMICAL NEWS.

SIR,—I beg here to offer some remarks on Mr. Wright's paper in your last impression. (*Amer. Reprint Chemical News*, Nov. 1867, *p.* 239.) He complains of the trade use which retains the old equivalent (24) for sodium, and it can certainly not be denied that it would be far better if the right equivalent (23) were taken instead. But I must protest against Mr. Wright's calling this well-known practice "a barefaced fraud." The facts of the case are known to most or all important buyers of soda-ash, and all bargains are made on the express condition of going by the analysis of some analytical chemist who may or may not take 24 as equivalent for Na, but will take 24 in most cases. The choice of the analytical chemist is practically always in the hands of the buyer. Does Mr. Wright really mean to impute a "barefaced fraud" to the great majority, if not to the whole, of the chemical manufacturers in this country? Besides, would they on their side submit to the equivalent 44 for peroxide of manganese if they considered it a "barefaced fraud," and not a custom of the trade which is understood by buyer and seller?

Mr. Wright further supposes that for the estimation of chloride of lime the iron process is the one generally used, and he asserts that all its known sources of inaccuracy tend to heighten the apparent percentage of chlorine. But he overlooks two important sources of possible errors, viz., the efflorescence of the ferrous salt and the escape of chlorine during the operation, the latter of which can hardly be totally avoided, and both of which lower the apparent percentage. It is, consequently, generally assumed that the iron process shows a little lower percentage (say 1 per cent) of available chlorine than the arsenite of soda process, and the latter, far more reliable one, is rightly superseding the former more and more in the laboratories.

I am, etc., A PRACTICAL CHEMIST.

The Soda Trade.

To the Editor of the CHEMICAL NEWS.

SIR,—In answer to the remarks of "A Practical Chemist" in your last impression (*Am. Reprint*, Dec. '67, *page* 319), I beg to observe that the practice stigmatised as a "barefaced fraud" is not so much the mere use of 24 as the equivalent of sodium, as the habitual invoicing of sales of soda-ash as containing a considerably higher percentage of available alkali than that really present. To take an example: pure Na_2CO_3 contains (on the supposition that $Na=24, C=12, O=16$) 59·26 per cent. of Na_2O; whilst of $Na=23$, it contains 58·49. the difference between the two being 0·77 per cent. out of 59·26: on a 48 per cent. ash, accordingly, the difference would be $\frac{48}{59\cdot26}+0\cdot77$. or 0·62 per cent.; that is, the real percentage would be 48—0·62, or 47·38 per cent. If, however, such ash were invoiced at 2 per cent. above its real strength, it would be called 49·38 per cent.—say 49 per cent.; and if the commercial analyst referred to by either party adopted such a mode of computation as would allow this invoice to pass unchallenged (and the writer has known many such instances), the result would be that the purchaser would pay for 49 units, whilst the ash, even on the supposition that $Na=24$, only contained 48 units; accordingly, it does not appear a very extravagant statement that in this case the purchaser is "defrauded."

If the taking $Na=24$ were a point simply affecting trade customs, it would be of no more material consequence than the selling of some goods by the ton of 20 cwt. and others by the ton of 21 cwt.; but as none of the higher class of chemists accept this equivalent, and as there is no reason whatever to be assigned for its retention, whilst, in addition to the scientific inaccuracy, there is the objection that it opens the way to disputes, and is the cause of considerable discrepancies in the analysis of the same substance by different analysts, it is evident that the abolition of this custom would ultimately be attended with advantage to the manufacturer, and would also tend to raise commercial analysts from the somewhat low position they unfortunately hold at present.

With respect to the iron process for bleaching powder determinations, I have only to remark that visibly effloresced crystals of ferrous sulphate are uniformly more or less peroxidised, and are totally unfit for use. With pure chemicals and careful manipulation I have always obtained precisely identical results with both the iron process and the sodium arsenite process, in the absence of chlorate in the sample analysed.—I am, etc.,

CHARLES R. A. WRIGHT, B.Sc.

Edinburgh, October 5, 1867.

To the Editor of the CHEMICAL NEWS.

SIR,—I have read with much pleasure the communications of Mr. Wright, giving us, as they do, a brief glance at all the methods capable of being employed in commercial determinations. As he does not mention it, I think the process I employ for the determination of manganese must not be generally known. I place a given weight of the manganese with HCl in a small flask with a leading tube attached. I absorb the Cl by means of a solution of $NaOCO_2$, using an inverted retort for a condenser; then by means of a solution of arseniate of soda I estimate the Cl. and thus arrive at the percentage of MnO_2; or rather I have prepared a set of tables for this and other determinations, by referring to which I can, without calculation, or at least with the simplest possible, arrive at what I want. Of course I claim no credit for this adoption. The thing appears so obvious to substitute two commonly occurring materials for the KI and SO_2 of Bunsen's process, that I should have supposed it in use in every laboratory where such determinations were made.—I am, etc.,

P. H.

Dr. Claus on Succinic Acid.

To the Editor of the Chemical News.

Sir,—Under the title "Claus on Succinic Acid," Mr. Church communicated in the *Laboratory* an answer to the remarks which I have made, concerning his experiments on the action of nascent hydrogen on oxalic and succinic acids. I am sorry to be forced to take notice of this reply of Mr. Church; sorry the more as the reply misrepresents what he said in his first communication; and I am forced the more as Mr. Church appears to charge *me* with such a misrepresentation.

With respect to the first point, that Mr. Church did not assert that oxalic acid yields by reduction a substance isomeric with acetic acid—I cannot know in how far he himself lays stress upon his own examinations, and if Mr. Church said: "If the preliminary examination of these substances has conducted me to a correct conclusion, I am right in supposing the last-mentioned acid to have the formula assigned to it, we have a new isomer of acetic acid;" I must think Mr. Church himself believes that he is right in his speculations, and in this supposition I quoted Mr. Church's communication. But Mr. Church himself seems to know very well how much, or better, how little authentic his examinations are, and I now share this view with him entirely, and I promise, in the future, never more to quote any of his communications or to believe them.

The second point, that Mr. Church pretends never to have stated, "that succinic acid might be make to yield butylactic acid," is wrong, and in his citation all that he said about *this* acid is left out. Mr. Church quotes from his communication: "I have commenced a few experiments in this direction also. . . . But the products," etc. What Mr. Church here leaves out and signs with the three points is verbatim as follows:—

"Succinic acid, after the prolonged and energetic action of nascent hydrogen as above described in the case of oxalic acid, suffers a similar change. I have not endeavoured to moderate the action so as to form the intermediate, or butyloxylic acid, but have pushed it to the extreme, so that butylactic acid might be obtained. The operation was performed in a retort; towards its conclusion a powerful odour, resembling that of butyric acid, was noticed in the aqueous distillate. The mixture of zinc-salts in the retort was evaporated, sulphuric acid added in excess, and the liquid shaken up with ether. From this ethereal solution (besides some unchanged succinic acid) a deliquescent acid was obtained, the properties and salts of which agreed completely with the butylactic acid of Wurtz. I have likewise submitted suberic and phthalic acids to the above described treatment, and the reactions promise interesting results." But Mr. Church's last remark: "and it would be altogether premature to express any opinion as to their composition" belongs not to the butylactic acid, but to the products of these reactions on suberic and phthalic acid.

I suppose Mr. Church never thought that his answer would come into my hands, otherwise I cannot understand how he could thus deny his own words. I do not like to discuss this behaviour of Mr. Church in your journal, but for the sake of the truth I must ask you to make this letter public.—I am, etc.,

A. Claus.

Freiberg, August, 1867.

⁂ The following is Professor Church's communication referred to in the above letter:—

"The researches of M. Claus have shown certain results contradictory, according to this chemist, of my earlier experiments. Would you permit me to deny the assertions put into my mouth by M. Claus? I will do so very briefly. (1) I did not assert that oxalic acid yields by reduction 'a substance isomeric with acetic acid.' (2) I never stated that 'succinic acid might be made to yield butylactic acid.' All my experiments were merely preliminary, and, as the following extracts from my paper* show, were not deemed by myself to be adequate proof of my anticipations.

"'If the preliminary examination of these substances has conducted me to a correct conclusion, and I am right in supposing the last-mentioned acid to have the formula assigned to it, we have a new isomer of acetic acid. . . . .

"'It will not be unreasonable to expect corresponding results from the action of nascent hydrogen on the homologues and analogues of oxalic acid, and the acid and neutral ethers of these acids.

"'I have commenced a few experiments in this direction also. . . But the products of these reactions, obtained only within the last few hours, await further purification and analysis: and it would be altogether premature to express any opinion as to their composition.'"

"Royal Agricultural College, Cirencester,
April 23, 1867."

The Science and Art Department.

To the Editor of the Chemical News.

Sir,—Since the publication of the new Directory of the Department of Science and Art, after the annual examinations of 1867, we are now in a better position to understand the intentions of the Government with respect to its masters certificated in science; perhaps therefore you will permit me to re-open a subject of interest to no inconsiderable number of your readers.

On comparing the new Directory with the old, two important contractions strike us.

I. The omission of the list of certificated masters.

II. The omission of the rules for their special examination in November, they being henceforth required to undergo the same examination as students in May.

These significant changes taken in conjunction, render the supposition not improbable that the committee of the Council of Education meditate hereafter sweeping away the distinctions of the somewhat privileged class of certificated teachers, by first inundating it with a flood of new-comers of lower qualifications, and subsequently throwing down all boundary marks.

As the new rule stands, which came into operation this year, all persons above twelve years of age who shall have obtained a first or a second class at the May examinations of students, shall be deemed qualified to receive payment on results; *i.e.*, shall receive the monetary encouragement hitherto granted to certificated masters alone; this extension admits this year (I believe) between 1,000 and 2,000 additional teachers, and in the course of three years may be expected to swell the present total number of licensed teachers (about 400) to 5,000; this number will then be found too costly for the application of the former bonuses, and the Government grant will be withdrawn.

Nor will the preceptor have cause pecuniarily for complaint, since he has been yearly warned by the department that these allowances are temporary, and may cease whenever the system, whose infancy they were intended to foster, shall have acquired self-supporting vigour. He will, if provident, like the wise steward of the parable, make provision against the day of being cast out, and will seek some more permanent and less speculative remuneration. Here his Government certificate might be a recommendation if its *prestige* were not suffered to be dimmed in the eyes of the scientific public.

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* *Journal Chem. Soc.*, 1864 [2], p. 301.

On the old plan each candidate for the diploma had to pass a practical as well as a theoretical test; *e.g.*, in chemistry three hours were assigned to him to analyse and describe some such unknown mixture, as silica with sulphates of baryta and magnesia.* Thus every successful master was known to have some slight acquaintance with manipulation, while a certificate of the first grade implied a certain higher degree of proficiency in qualitative analysis. For this is now substituted an examination in theory of perhaps greater difficulty, coupled with the written answering of such questions as the following:

Q. How is brass separated into its elementary constituents?

Q. A mixture contains Cu, Pb, Fe, Ba, Ca, Mg; how are the several constituents detected?

Q. What is the action of sulphide of ammonium upon sesquichlorides of iron and chromium and chloride of nickel?

But the examinee has an option of questions and may omit all the practical, still obtaining the required first or second class; hence there is now hardly any guarantee that future candidates shall have any experience in the laboratory or in the handicraft of their profession.

This practical skill, however, in such subjects as chemistry, mineralogy, botany, physiology, and geology, is essential to their masterly understanding, and is moreover by far the more costly and laborious portion of the science to acquire.

This, if any, is the grievance of the old régime of masters, that they are liable to be confounded with a different class of aspirants; it might, however, be so easily removed by two trifling concessions from the department, that I am tempted to recommend, as they have courted suggestions—

I. Since the present examinations are wholly conducted in the evenings of May, that the mornings of the same days be partly devoted to an additional voluntary examination in the practical portion of such above-named subjects, and that to those candidates only who pass this probation should the masters' printed certificate be issued.

II. That in the register of certificated (which should be retained in some public form), those examined before 1867 should have some distinctive mark attached, *e.g.*, that of their grade.

This arrangement will save both the Government and the master expense; the additional trouble to the four or five examiners concerned will be small.

The department will thus, by equitable and conciliatory measures (considering the pre-eminent character of their noble phalanx of examiners), shortly attain to an influence upon the scientific studies of the country possessed by no other body, whether learned society or university; even in May last they examined 8,439 papers belonging to about 5,000 different individuals.—I am, etc.,

M. A.

Dr. Claus on Succinic Acid.

To the Editor of the Chemical News.

Sir,—Permit me a few words in reply to Dr. Claus's communication to your paper of last week. (*Amer. Reprint, Dec.*, 1867, *page* 320.)

Dr. Claus wishes it to be understood that I made two unconditional assertions with reference to two particular points noticed in my paper "On some Metamorphoses of Oxalic Acid." I affirm that I did not make these two unconditional assertions. On the contrary, I gave it to be understood that my results as to (1) the existence of an isomer of acetic acid, and (2) the conversion of succinic into butylactic acid, were not sufficiently complete to warrant any positive assertion. In the case of the supposed isomer of acetic acid I expressly said—"If the preliminary examination of these substances has conducted me to a correct conclusion, and I am right in supposing the last-mentioned acid to have the formula assigned to it, we have a new isomer of acetic acid." Dr. Claus ingeniously omits the word "and" in his quotation, thus making me say, "I am right, etc." I ask you, Mr. Editor, is this omission honest? If it be not a printer's or clerical error, Dr Claus either does not know the significance in English of such an omission, or he knows it too well.

With reference to butylactic acid, I stated my examination of the product in question to be merely qualitative. I certainly intended my remark as to its being premature to express any opinion as to the composition of my products, to apply to the supposed butylactic acid. While stating that the properties and salts of the acid I obtained did correspond with those of the true acid, I affirmed nothing concerning its composition, not having submitted it to analysis.

Dr. Claus accuses me of certain omissions in my letter to the *Laboratory*. My letter, which you quote, contained naturally those paragraphs only of my original paper which showed the reservation I had felt it necessary to make: fuller extracts would have been out of place; nor have I newly expressed any opinion as to the correctness of my original results and anticipations. I will indeed acknowledge freely any errors I may have committed.

I hardly care to refer to the uncourteous tone of Dr. Claus's letter, or its offensive innuendoes. What occasion can Dr. Claus have for telling your readers that he promises never more to quote any of Mr. Church's communications or to believe them? Does he wish me to say how deeply such a terrible threat grieves me, and that I have in consequence no longer any incentive to chemical work?

Dr. Claus supposes that I never thought my answer would come into his hands. Is not this supposition perfectly gratuitous? On what shadow of foundation does it rest? It is not only gratuitous but foolish. Dr. Claus says, moreover, that I have denied my own words. He says this, but his attempts to prove it will I am sure be generally considered ineffectual. I have quoted my own words, not denied them.

But it is not worth while to argue concerning these matters with a disputant who descends to offensive personalties. If his position were a strong one he would not need them: a weak position they will not avail to strengthen.—I am, etc.,

A. H. Church.

Cirencester, October 21, 1867.

Volatility of Sesquichloride of Iron.

To the Editor of the Chemical News.

Sir—After reading Mr. Skey's note on the volatility of sesquichloride of iron at common temperatures, given in last week's Chemical News, (*Amer. Reprint, Dec.*, 1867, *page* 289.) I tried some experiments upon the subject. Without at present in any way disputing the fact, I would draw the attention of this gentleman to a possible source of error. In my experiments I found that the vapour from pure hydrochloric acid caused a faint tint in an aqueous solution of the ordinary crystals of sulphocyanide of potassium.

M. Stas states that hydrochloric acid evaporated in an open vessel becomes contaminated with impurities; and an examination made by myself has shown the presence of iron in the light particles of dust deposited on the upper shelves of lofty rooms. It may be safely stated, then, that minute particles of iron are continually floating about in the atmosphere.

Now, considering the great delicacy of the test, and the excessively small quantity of iron necessary to effect colouration in a solution of an alkaline sulphocyanide, it must surely be difficult to prepare on a manufacturing scale crystallised sulphocyanides which will not give a faint pink tint by contact with hydrochloric acid.—I am, etc.,

Henry Seward.

Chemical Patents.

To the Editor of the Chemical News.

Sir,—I write for information, or, at all events, to throw some

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* An actual exercise proposed in 1865.

light upon a critical expression of yours, which under slight modification is not uncommonly to be met with in the pages of the Chemical News. To quote an instance of this remark in illustration,—I may state that having been recently engaged in a controversial dispute, in another journal, as to the originality of a certain invention for producing chlorine, and my opponent having referred in condemnatory terms and for reasons of his own to a totally different patent of mine, viz., "for improvements in the manufacture of inflammable gases," he makes an extract in support of his views from the columns of some *un-named* authority; but having already seen the remark myself, I know that authority to be the Chemical News. The extract referred to is the following:—You say, speaking of my patent—"This is another case of patenting well-known chemical processes." Now, Sir, having already stated that I seek for information, will you be so kind as to furnish me and your readers generally with other than a very exceptional list of chemical patents which are not founded on well-known chemical processes? Take for instance Howard's or Scoffern's patents for refining sugar. The remarkable power possessed in so high a degree by animal charcoal of absorbing colouring matters, as well as the coagulative property of serous albumen when heated beyond 140°F., were facts I apprehend as thoroughly known as was the abstract physical principle of the vacuum pan prior to the date of the first of these patents; as was also the depurative power of acetate of lead, anterior to the date of the second patent. Again, and taking at random another invention out of many. In "Brankart's Patent Copper Smelting Process,"—the chemical facts upon which this patent rests were thoroughly well known before they were applied to this specific purpose by the inventor. Was it not known that sulphide of copper roasted under certain conditions would produce sulphate of copper, and that such sulphate of copper, when dissolved, would by the superior affinity of iron throw down the copper in a metallic state, and by the appropriation of the negative elements present produce sulphate of iron or copperas? Take another instance. Mr. Gill, a few years since, invented a lamp without flame in which a cylindrical coil of platinum wire, the hundredth part of an inch in diameter, and making about ten turns, was maintained in a state of incandescence by the vapour of alcohol. The chemical principle here was also old, having been discovered years before by Sir Humphry Davy. I do not suppose I have selected the happiest illustrations of the application of known chemical principles to useful purposes, but they will at least serve to prove my position, namely, that in innumerable instances, what are called chemical inventions, and are recognised by the law to be legally such in the abstract, and therefore sound and patentable, are all more or less so many other cases of patenting well known chemical processes. It is true that instances may be occasionally brought forward where a chemical or physical *discovery* is recorded for the first time in the specification of a patent, securing its *application* to some useful purpose; but this is an exception to the rule, and not the rule itself.

And again, it occasionally occurs that a chemical principle is so well known in its application to any particular purpose that a patent will only hold good by claiming certain specific machinery or apparatus as applied thereto. Where, however, such is not the case, where a useful and economical result is obtained by combining several separate but well known chemical principles together, in consecutive order, so as to constitute what may be termed a circle of affinities, and to lead to important manufacturing results not hitherto obtained,—then I think the judgment of the general reader should not be warped, the co-operative power of capital suspended, nor the criticism of the malicious invited. First, by sweeping the bulk of chemical patents into one condemnatory sentence, and saying of each in succession as it appears—"This is only another case of patenting well known chemical processes"—as though there were really no novelty whatever in the case, either as to *method* of operation or useful *results.*

The reason that induces me to speak is that such remarks are calculated to do considerable injury, and rests, as it appears to me, on no solid or legal foundation.—I am, etc.,

Isham Baggs.

54, Chancery Lane.

Ozone.

To the Editor of the Chemical News.

Sir,—The following is an account of the development of ozone during July, August, and September:—

In July there were periods with very little ozone from the 1st to the morn. of the 3rd, from the 5th to the morn. of the 13th, on the aft. of the 16th, morn. of the 18th, from the 19th to the 21st, from the aft. of the 23rd to the 28th, and on the afts. of the 29th, 30th, and 31st. Considerable quantities of ozone were present on the aft. of the 13th, and from the 22nd to the morn. of the 3rd. Large amounts on the aft. of the 3rd and morn. of the 4th, from the 14th to the morn. of the 16th, on the 17th and aft. of the 18th, and on the morns. of the 29th, 30th, and 31st.

In August there were periods with very little ozone from the 4th to the morn. of the 5th, aft. of 7th. aft. of 9th to 11th, 15th, aft. of 19th and morn. of 20th, and from the 26th to the 31st. Considerable amounts on the morns. of the 8th and on the 21st. Large amounts on aft. of the 5th, on the morns. of the 6th, 7th, and 9th, the 14th, 17th, morns. of 18th, 19th, and aft. of 20th. There was a period of variable development from the 22nd to the 25th. In September there were periods with very little ozone from the 1st to the morn. of the 2nd, on the 3rd, morn. of the 8th, from the 9th to the 11th, aft. of 12th to morn. of 14th, 15th to morn. of 17th, from the aft. of the 19th to morn. of 21st, on the aft. of 22nd and morn. of 23rd, morn. of 24th, and from the 25th to the 28th. Considerable quantities on the morn. of 12th, morn. of 19th, aft. of 24th, and aft. of 30th. Large amounts aft. of 2nd, 4th to morn. of 7th, aft. of 8th, aft. of 14th, aft. of 17th, to 18th, aft. of 21st and morn. of 22nd and 29th to morn. of 30th.—I am, etc.,

R. C. C. Lippincott.

Bournemouth.

Recovery of Sulphur from Alkali Waste.

To the Editor of the Chemical News.

Sir,—In your impression of 26th of July, 1867 (*Amer. Reprint*, Sept., 1867, page 120), in an article on the recovery of sulphur from alkali waste, by Ludwig Mond, that gentleman says "the most successful of them has probably been that of Mr. Benjamin Jones, a workman who assisted me in working my process out on a large scale in 1863, at the works of Messrs. Hutchinson and Co., Widnes. One of the three patents which he took out between December, 1863, and May, 1864, has been worked a short time in Warrington, but was, however, quickly abandoned."

Now, Mr. Mond must have been conscious at the time of writing the above, that I was not working at Messrs. Hutchinson's at the time to which he alludes, and I may add that I only know Mr. Mond by report, never having either seen or spoken to him, to the best of my knowledge.

It is true that my patent was worked for some time at Warrington, but the reason of its discontinuance was not through inefficiency of the process. I shall esteem it a favour if you will kindly insert this in your next impression.—I am, etc.,

Benjamin Jones.

Castle Hill, Hindley, near Wigan, Lancashire, October 19, 1867.

MISCELLANEOUS.

Solder for Steel.—The best solder for fine steel work, according to the *American Artisan*, is composed of nineteen parts of silver, one part copper and one part brass. Borax is the best flux.

The Newton-Pascal Forgeries.—Dr. T. Archer Hirst F.R.S., writes to the *Times* as follows:—

Sir,—In a communication to the British Association at Dundee, I stated that M. Chasles, desirous of submitting his newly acquired papers to every possible test, had forwarded specimens of the alleged handwriting of Newton to Sir David Brewster and myself through his friend M. de Khanikof. Sir David Brewster has since submitted five of these specimens to the inspection of the Earl of Portsmouth, the Earl of Macclesfield, amd Sir Frederic Madden, and the unanimous verdict of these authorities, as recorded in the *Athenæum* of September 28, is "that the handwriting is not that of Newton."

On Thursday last, M. de Khanikof accompanied me to Burlington House, for the purpose of further comparing these specimens with the authentic letters of Newton in the possession of the Royal Society. We were assisted in our investigations by Dr. Sharpey, and the result was perfectly conclusive—in short, entirely in accordance with the verdict above quoted. We also searched for evidence of a more positive nature touching the origin of these documents, and were rewarded with success. Without troubling you with the details of this new investigation, I may state that our efforts were first directed towards obtaining further information relative to the Pierre Desmaizeaux whose name so frequently appears in M. Chasles' documents. We found that at the commencement of the 18th century this gentleman resided in London; that on tha 3rd of November, 1720, he was admitted a Fellow of the Royal Society by Sir Isaac Newton, then president; and that shortly before his election he had presented to the Society a copy of his new work, entitled, *Recueil de diverses Pièces sur la Philosophie, la Religion, etc., par MM. Leibnitz, Clarke, Newton, etc.* (Amsterdam, 1720). On turning over the pages of the second volume of this work Mr. Walter White (assistant secretary to the Royal Society) had the good fortune to discover that three out of the five of the alleged specimens of Newton's handwriting were *verbatim* copies of isolated passages occurring in the French translation of three letters originally written by Newton in English. To each passage thus extracted the forger had appended Newton's name as a signature.

Without further comment on this annihilating fact, I pass to a more astounding one. Here is an exact copy of a fourth document alleged to have been written by Newton:—

"La realité de l'espace n'est pas une simple supposition; elle a esté prouvée par les argumens que j'ay rapportez, auxquels on n'a point repondu. On n'a point repondu non plus à un autre argument; savoir; que, l'espace et le temps sont des quantitez,ce qu'on ne peut dire de la situation et de l'ordre. "I. NEWTON."

Newton is here made to copy and sign a garbled translation of a passage to the authorship of which even he has not the slightest claim. The real author is the well-known Dr. Samuel Clarke, rector of St. James, Westminster, between whom and Leibnitz a celebrated discussion on the principles of natural philosophy and religion was conducted by letters in 1715–17. In 1717 Dr. Clarke, after having had Leibnitz's letters carefully translated from French into English, and his own from English into French, published the whole correspondence in duplicate. From a copy of this work now in the British Museum I extract the following paragraph in Dr. Clarke's fourth reply to Leibnitz:—

Page 134.	Page 135.
"Sec. 14. La realité de l'espace n'est pas une simple supposition; elle a été prouvée par les arguments rapportez ci-dessus, auxquels on n'a point répondu. L'Auteur n'a pas répondu non plus a un autre argument, sçavoir, que l'espace et le temps sont des quantitez; ce qu'on ne peut dire de la situation et de l'ordre."	"Sec. 14. The reality of space is not a supposition, but is proved by the foregoing arguments, to which no answer has been given. Nor is any answer given to that other argument, that space and time are quantities, which situation and order are not."

It will be observed that in copying the passage on the left—not from Dr. Clarke's work, but from Desmaizeaux's *Recueil*, where the French edition *only* of the correspondence was re-published,—the forger has, in one or two places, slightly departed from the original text. His motive for so doing has obviously been to render the extract somewhat less unsuited to the illustrious name he had the audacity to append thereto.

M. Chasles, whose disinterested integrity as a historian is beyond question, has hitherto declined to admit the possibility of the fabrication of the numerous documents in his possession. "Un faussaire," he has urged, "qui aurait fabriqué toutes ces lettres, toutes ces piéces, pour prouver qu'il a existé des relations entre Pascal et Newton, aurait eu bien du talent, puisqu'il aurait fait tout à la fois du Pascal, du Newton, du Moutesquieu, du Leibnitz," etc. We know, at all events, in what manner this *faussaire a fait du Newton* without the expenditure of any talent whatever; and, knowing this, we cannot but regard the entire collection of documents as wholly untrustworthy—as wholly unworthy, indeed, of the further patronage of the eminent author of the *Aperçu Historique*.

Technical Education.—The following is a brief outline of the provision made by law in the Netherlands, in order to secure proper technical education for all classes of society. Section 12 of the law, which came into operation on the 1st of July, 1863, sanctions and orders the establishment of the following four descriptions of schools, viz. *a*, schools destined for the education of the ordinary artisans, and labourers, and agricultural labourers; *b*, in a higher class of schools; these are divided into such with three years and five years' course of instruction; *c*, agricultural colleges; *d*, a polytechnic school. Now Holland has always been a country where public instruction has ranked very high, as it does yet. The general government has established fifteen of the schools alluded to *sub. b.*, five of which are with a five years' course of instruction. Moreover, the municipal authorities of all larger towns have established schools as specified *sub. b.* Besides these schools there are in Holland three Athenæa established at Amsterdam, Deventer, and Maastricht, these being schools whereat in addition to physical science, also law, medicine, and literature are taught; no degrees however are granted at the Athenæa; besides these there are three universities, while there are distinct schools for educating officers of the army and navy. The whole number of inhabitants of the Kingdom of the Netherlands in Europe is a little over three millions, and the occupation is chiefly agricultural and mercantile, rather than industrial. The law of 1863 on middle class and technical education was made after the lower class education had been thoroughly revised. There are several private middle class schools which have modified their programme in accordance with the law alluded to.

Forthcoming Scientific Books.—Amongst Messrs. Churchill's literary announcements for the ensuing session we find the following:—

"The Microscope and its Revelations." With 400 plates and wood engravings. By W. B. Carpenter, M.D., F.R.S. Fourth edition.

"Companion to the British Pharmacopœia of 1867," comparing it with the Pharmacopœias hitherto used in Britain; including that of 1864; also the new editions of the U.S., the French, and the Prussian Pharmacopœias. By Peter Squire, F.L.S. Fifth edition.

"On the Action and Use of Oxygen. With selected cases proving its value in the treatment of various intractable diseases." By S. B. Birch, M.D., M.R.C.P., Lond. Second edition.

"The Pharmacopœias of the principal Hospitals of London." Arranged in groups for easy reference and comparison. By Peter Squire, F.L.S. Second edition.

"Germinal Matter and the Contact Theory. The key to the prevention and study of zymotic disease." By James Morris, M.D., Lond. Second edition.

"Urine, Urinary Deposits, and Calculi, and on the Treatment of Urinary Diseases." With numerous plates. By Dr. Lionel S. Beale, F.R.S. Third edition, very much enlarged.

"Chemical Notes to the British Pharmacopœia of 1867. By Charles Henry Wood.

"The Induction Coil: being a Popular Explanation of the Electrical Principles on which it is constructed." With a series of beautiful and instructive experiments. By Henry M. Noad, Ph.D., F.R.S., F.C.S. Third edition, with engravings.

"The Calendar of the Pharmaceutical Society of Great Britain."

"First Lines for Chemists and Druggists preparing for Examination." By John Steggall, M.D. Third edition.

"On Local Anæsthesia and Volatile Fluid Spray in Medicine and Surgery." By Benjamin W. Richardson, M.D., F.R.S.

"The First Step in Chemistry: a new method of teaching the elements of the Science." By Robert Galloway. Fourth edition

The Borax Company, engaged in taking out borax, in Lake county, will soon be in condition to extract five tons of this article per day from the borax lake, as they have just received a new and powerful steam dredger and an immense pump, with which to exhaust the water from the coffer dams. This pump weighs something over 1,000 pounds, and is to be worked by steam.—*Mining and Scientific Press.*

Poisoning by Caustic Potash.—A death has recently occurred at Pendleton, caused by drinking the contents of a jug supposed to contain porter, but which in reality contained a solution of caustic potash. The deceased drank it on the 22d May, and lived until the 20th September. Mr. Heywood, surgeon, Pendleton, who had attended the deceased, stated that he had been unable to swallow anything during his illness, and he attributed death to starvation in consequence of that inability. He had made a *post-mortem* examination of the body, and found a stricture of the œsophagus, extending over four or five inches, rendering swallowing an impossibility. The injury had been caused by taking caustic potash.

The Atmosphere of the Metropolitan Railway.—The inquiry respecting the death of E. Stainsby, who died on the 28th of August, at the King's-cross station of the Metropolitan Railway, after travelling from Bishop's-road station, was resumed on Friday last by Dr. Lankester. The inquest had been twice adjourned for the purpose of having scientific evidence as to an analysis of the atmosphere in the tunnels of the railway laid before the jury, so as to lead them to a just conclusion as to whether the death of Elizabeth Stainsby was accelerated by the condition of the atmosphere in the railway between Bishop's-road and King's-cross stations. The Coroner said he had received a communication from Mr. Fenton, the general Manager of the Metropolitan Railway Company, in which he stated that the scientific evidence which the company wished to lay before the jury was not yet completed. The scientific referee whom he (the coroner) had appointed, had, he believed, completed his experiments, but still he thought it well that the jury should have the whole evidence before them in order that they might arrive at a right conclusion. He had received assurances from a great many persons that the ventilation of the Metropolitan Railway had been much improved. Therefore, setting aside any result at which the jury might arrive as to the death of Elizabeth Stainsby, it appeared that from the fact of the deceased and others having complained of the atmosphere of the railway, the directors had done something to remedy the evil. Therefore, the jury would not feel that their time was lost in keeping a watch over this railway for the benefit of the public. It had been said that the jury and he were unnecessarily interfering with the railway company, but he might remark that he had received many communications from persons who stated that they had felt very much oppressed in their breathing while in the tunnels of the railway, and from others who had been advised by their medical attendants to give up travelling upon it. He had also received many letters suggesting modes of improving the ventilation in the tunnels, so that the jury would see the inquiry was very important, and absolutely necessary for the public interests. Everything that could be put before the jury in the shape of information ought to be adduced, both for the sake of the public and of the company. He proposed that the inquiry should be adjourned to Wednesday, the 30th of October. The jury acquiesced in the suggestion, and the inquiry accordingly stood adjourned.

Royal Polytechnic Institution.—A few evenings since, we visited this Institution by invitation from Mr. Pepper, and it gives us considerable pleasure to be able to commend the entertainment. The Polytechnic fulfils now, as it has for many years, a useful purpose, in instructing while amusing its audience. Many who would shrink from reading for instruction, or from attending strictly scientific lectures, would take pleasure in attending the entertainment given here, and at the same time learn a little of some branch of science. Those who have seen the various optical delusions and have heard the popular lectures on electricity, sound, light, etc., which have from time to time been delivered here, will refrain from quibbling. One of the leading features in the present entertainment is the lecture on the Paris Exhibition, which perhaps deserves the lion's share of praise. Few would imagine how good an idea of the Exhibition may be gained from this lecture; the positions for photographing seem well chosen. The photographs are projected, magnified, on to a screen. Among those which were most noticed were the exhibitions of glass, of the fine arts from Italy, and of English designing, and machine-made jewellery; the photographs, too, of the intricate machinery came out very clearly. In the reading of "The Bridal of Belmont," by Mr. Millard, a lighter feature in the entertainment is found. This poem offers a fitting subject for a ghost scene, and is taken advantage of; a rather curious musical instrument is also introduced here, which appeared to give great satisfaction. The only other point to which we need allude is the Automatic Leotard, which as a piece of mechanism is very clever.

Preservation of Stone.—This subject, which has attracted the attention of so many chemists, seems now to have been brought to a very successful point. We have received some specimens of chalk treated by a process discovered by Messrs. Dent and Brown, of the Chemical Department, Woolwich. Their process consists in the application of a solution of oxalate of alumina to the stone. The experiments date from December, 1865. and the results they have now obtained are most encouraging. The process is applicable to limestone, dolomite, and chalk, and may, we think, be made subservient to the preparation of lithographic stones. Oxalate of alumina is readily soluble in water, and the solution, which is simply applied with a brush, is made of a strength varying with the porosity of the material to which it is to be applied. The specimens we have before us are left in the original condition at one end, and have been prepared with the solution at the other. The physical characteristics of chalk so treated are—lightness. the possession of a glazed surface approaching somewhat in appearance to marble. and greatly increased hardness; in this respect the stone is about equal to Fluor spar, or 4 in Mohs' scale. Furthermore, the lime being transformed into one of the most insoluble and unalterable of its compounds, and the alumina being precipitated, the pores are filled with a substance almost unacted upon by water or by the impurities present in the atmosphere of large cities. We should be glad to hear that the discoverers had one of the experimental bays of the Houses of Parliament placed at their disposal. They might thus prove their process to be a formidable rival to that of their colleague Mr. Spiller, which according to present appearances is likely to be the most successful of all the numerous schemes now *sub judice* at Westminster.

The British Association.—A meeting of the Local Invitation Committee of Exeter and Plymouth for securing the visit of the British Association to one of these places in 1869, has been held to consider what steps should be taken to end the present rivalry. After much discussion, it was decided to refer the question to the last three Presidents of the British Association, Professor Phillips, Mr. Grove, and the Duke of Buccleuch, to say which town should retire and help the other.

Solubility of Anhydrous Alumina in Ammonia.—If alumina is heated to whiteness, and pounded up fine, then allowed to remain in contact with ammonia for a few hours, a tolerably heavy gelatinous precipitate is produced by chloride of sodium or ammonium.—*William Skey, New Zealand.*

Composition and Quality of the Metropolitan Waters in September, 1867.—The following are the Returns of the Metropolitan Association of Medical Officers of Health:—

Water Companies.	Total solid matter per gallon.	Loss by Ignition*.	Oxidisable organic matter †.	Hardness.		Organic and other ammonia.
				Before boiling.	After boiling.	
Thames Water Companies.	Grains.	Grns.	Grains.	Degs.	Degs.	Grains.
Grand Junction	17·80	1·15	0·56	13·0	4·0	0·001
West Middlesex	17·47	1·50	0·52	12·5	4·0	0·003
Southwark and Vauxhall.	17·93	1·55	0·78	12·5	3·5	0·007
Chelsea	19·33	1·50	0·93	13·5	3·5	0·003
Lambeth	18·50	1·95	0·72	13·5	4·0	0·003
Other Companies.						
Kent	27·00	1·55	0·24	19·0	7·0	0·000
New River	14·73	1·40	0·28	12·5	4·0	0·002
East London........	17·91	1·60	0·48	12·5	4·0	0·003

* The loss by ignition represents a variety of volatile matters, as well as organic matter, as ammoniacal salts, moisture, and the volatile constituents of nitrates and nitrites.

† The oxidisable organic matter is determined by a standard solution of permanganate of potash—the available oxygen of which is to the organic matter as 1 is to 8; and the results are controlled by the examination of the colour of the water when seen through a glass tube two feet in length and two inches in diameter.

Impermeable Oil Barrels.—The Titusville, Pa., *Herald* describes the process for rendering casks or barrels air-tight and impermeable to oil, spirits, turpentine, and all volatile fluid, that has been in operation in its vicinity for a year, with such good results. The barrel having passed from the finishing hands of the workmen, the process of permeating is in this wise. The barrel is placed over tubes through which hot air is injected into it for twenty-four hours, thoroughly heating the wood and opening the pores. Any worker in wood knows that glue will not stick to a cold surface as well as to a warm one, and this fact is a serious inconvenience in the old fashion of glueing barrels by hand. From these tubes the barrel is placed in a framework that braces the head, and allows it to revolve on either axis. Hot glue is poured into it, and distributed over every portion of the inner surface. A tube is then introduced through the bung hole, and a pressure of twenty pounds of air to the square inch applied, forcing the glue into every crack and crevice and even the pores of the wood; so great is the pressure that the glue is often forced through the pores to the outside of the barrel. The package is then impermeable and as tight as a bottle.

CONTEMPORARY SCIENTIFIC PRESS.

[Under this heading it is intended to give the titles of all the chemical papers which are published in the principal scientific periodicals of the Continent. Articles which are merely reprints or abstracts of papers already noticed will be omitted. Abstracts of the more important papers here announced will appear in future numbers of the Chemical News.]

Le Génie Industriel. June, 1867.

F. L. Roux: "*A Method of Preserving Armour Plated and other Iron Vessels by means of Copper Sheathing.*"—Brissonneau: "*New Apparatus for the Manufacture of Sugar.*"—E. Grisdale: "*A new Apparatus for Washing Photographic Proofs.*"

Comptes Rendus. July 8, 1867.

Becquerel: "*Third Memoir on some newly-discovered Chemical Effects of Capillary Action.*"—Daubree: "*On the Classification adopted by the Paris Museum for the Collection of Meteorites.*"—Secchi: "*On the Nebula of Orion.*"—N. Zinin: "*On Benzoin and its Derivatives.*"—Reboul and Truchot: "*Researches on the Isomerism of the Acetylene Series.*"—T. Hiortdahl: "*On Protosulphide of Cobalt.*"

July 15.

Chasles: "*Note on the Discovery of the Laws of Attraction by Pascal.*"—Sorel: "*Note on a new Cement made by slaking Magnesia with a solution of Chloride of Magnesium.*"—J. Nickles: "*On Fluomanganous Acid and other new Manganese Compounds.*"—Lecoq de Boisbaudran: "*Experiments on Supersaturation.*"—Balbiani: "*On a simple Method of Detecting the Presence of Corpuscles on Silk Worms.*"

July 22.

Duhamel: "*Note on Pascal's Correspondence relating to the Laws of Attraction.*"—Faye: "*Note on Pascal's Correspondence relating to the Laws of Attraction.*"—Chevreul: "*Note on Pascal's Correspondence relating to the Laws of Attraction.*"—Chasles: "*Note on Pascal's Correspondence relating to the Laws of Attraction.*"—Chevreul: "*Report of the Author's Lectures on Chemistry delivered at the Museum of Natural History.*"—Daubree: "*Contributions to the Knowledge of the Structure of Meteorites.*"—C Matteucci: "*On the Secondary Electro-motive Power of the Nerves, and its Application to Physiology.*"—Zaliwsky-Mikorski: "*Note on a New Syphon.*"—G. Grimaud de Caux: "*A Comparative Study of the Results of the Removal of the Sewage Waters of Paris, Vienna, London, Marseilles, and Venice.*"—J. Raynaud: "*On a Practical Method of Determining the Voltaic Constants of a Battery.*"—G. Lechartier: "*On the Reproduction of Mimetese and of some Cloroarseniates.*"—Bizio: "*Some new Researches on Glycogen.*"—Phipson: "*Note on a Simple Method of detecting Iodine in the Presence of Bromine.*"

July 29.

Chasles: "*Some further Communications relative to Pascal's Letters on the Laws of Attraction.*" Duhamel: "*Some further Communications relative to Pascal's Letters on the Laws of Attraction.*"—C. Matteucci: "*On the Secondary Electromotive Force of the Nerves, and its Application to Physiology.*"—H. Scoutetten: "*On some Surgical Instruments found at Herculaneum and Pompeii.*"—Faugere: "*On the Pascal Correspondence.*" Benard: "*On the Pascal Correspondence.*"—P. Blaserna: "*On the Duration of Induction Currents.*"—R. D. Silva: "*On Titaniferous Iron Sand from Santiago, Cape de Verd Islands.*"—E. Grimaux: "*On the Nitro Derivates of the Benzylic Ethers.*"—V. De Luynes and A. Lionet: "*On the Methylic, Ethylic, and Amylic Derivatives of Orcin.*" Chevreul: "*Remarks on J. Lemaire's Experiments on the Properties of Phenic Acid.*"

Bulletin de l'Académie Royale de Belgique (*Classe des Sciences*)

June.

A. Quetelet: "*Note on the Publication of the Meteorological Annals of the Royal Observatory at Brussels.*"—A. Quetelet: "*Extract from Chacornac's Letter on Sun Spots, and on the Disappearance of the Crater of Linnæus.*"

Monatsbericht der königlich-Preussischen Akademie.

April.

Riess: "*On Double Induction, and on the Theory of the Electrophorus.*"—G. Rose: "*On the Meteorite which fell at Knyahinya, in Hungary, on June 9, 1866.*"—J. Philipp: "*On the Sulphocyanogen Compounds of Mercury.*"—C. Rammelsberg: "*On Phosphorous Acid and its Salts.*"—E. Reusch: "*On the Effect of Pressure on Rock Salt and Calcite.*"—H. Helmholtz: "*On N. Baxt's Experiments on the Velocity of Propagation of Irritation in the Motor Nerves of the Human Body.*"

Poggendorff's Annalen. April 12.

H. Buff: "*On the Inductive Action of a Voltaic Current on the Mass of the Conductor.*"—C. Bohn: "*On Negative Fluorescence and Phosphorescence.*"—E. Voit: "*On the Diffusion of Liquids.*"—W. Von Bezold: "*On Binocular Vision.*"—A. Schrauf: "*On the De-*

[English Edition, Vol. XVI., No. 412, pages 221, 212, 214, 220, 221; No. 409, page 186; No. 410, page 196.]

duction of the Form of Crystals from the Refractive Equivalents of the Elements of which the Crystal is composed."—E. Reusch: "*An Experiment with Prince Rupert's Drops.*"

May 16.

A. Topler: "*On the Construction and Performance of the Rotating Electrophorus.*"—A. Mitscherlich: "*On some new Methods of Determining the Constitution of Organic Compounds.*"—W. Beetz: "*On the Influence of the Motion of the Origin of a Sound on its Pitch.*"—G. Jenzsch: "*On the Laws of the Formation of Twin Crystals of Quartz.*"—A. Brio: "*On the Optical Properties of Hyposulphate of Baryta.*"

Annalen der Chemie und Pharmacie. Supplement. Vol. 5. No 1.

C. Zwenger: "*On the Melilotic Acid, and on its Preparation from Coumarin.*"—L. Darmstaedter: "*On Potassio-Chloride and Ammonio-Chloride of Gold.*"

July 1867.

H. Wichelhaus: "*On the Constitution and Composition of Organic Acids containing three Atoms of Carbon.*"—H. Schroder: "*On Hypogæic Acid.*"—O. Hausskneciit: "*On some Derivatives of Erucic Acid.*"—C. Weinhold: "*On Oxyphenylene Sulphurous Acid.*"—H. Kolbe: "*Remarks on the foregoing Paper.*"—H. Kolbe: "*On the Sulphuric and Sulphurous Ethers.*"—R. Warlitz: "*On the Sulphurous Ether Isomeric with Ethylsulphuric Acid.*'—R. Otto and L. Brummer: "*On Sulphochlorobenzoic Acid, and on some Derivatives of the same.*"

Annales de Chimie et de Physique. July.

A. Cornu: "*Researches on Crystalline Reflection.*"

Journal des Fabricants de Papier. June 15.

E. Bourdilliat; "*On Testing the Chemical Products used in Paper-making: Starch.*"

July 1.

E. Bourdilliat: "*On Testing the Chemical Products used in Paper-making. Continuation: Ultramarine. Antichlore. Prussiates of Potash.*"

Journal für Praktische Chemie. June 8.

A. Kenngott: "*On the Alkaline Re-action of some Minerals.*"—A. Kenngott: "*On Richmondite, Osmelite, and Neolite.*"—A. Kenngott: "*On Pyrophyllite, Hydrargillite, Pennine, Chlorite, and Clinochlore.*"—L. R. Von Fellenberg: "*Analysis of a Green Mineral (Anorthite? Felspar?) from the Bernese Oberland.*"—L. R. Von Fellenberg: "*Analysis of Serpentine from the Malenkerthal in the Grisons.*" L. R. Fellenberg: "*Analysis of Calcite from Merlingen.*"—T. Bail: "*On the Formation of Yeast.*"—E. Carstanjen: "*On Thallic Acid.*"

June 20.

B. Weiss: "*On the Colouring Matter of Saffron.*"—R. Wagner: "*On the Detection of Woollen Fibres in Silk Goods.*'

June 27.

R. Hoffmann: "*On the Causes of the Brittleness of the Bones of Horned Cattle.*" J. C. Leuchs; "*On the Bitter Principle of Hops, and on Deoxidation as a Means of Destroying the same.*"—F. Stolba: "*On the Quantitative Estimation of Lead by Precipitation by Zinc.*"—F. Stolba: "*An Analysis of some Ancient Bronze Objects, from the Collection in the Bohemian Museum at Prague.*"—F. Stolba: "*On the Analysis of Bone Black.*"—F. Stolba: "*On the Estimation of the Water of Crystallised Fluoride of Calcium Compounds.*"—J. Wolf: "*On the Constitution of some Aniline Colours.*"

Dingler's Polytechnisches Journal. June.

J. P. Reininghaus: "*An Apparatus for Measuring and Regulating the Supply of Fluids.*"—G. Bischof, Jun: "*On the Colorimetrical Estimation of Copper.*"—C. Thiel: "*On the Preparation of Meat Biscuits.*"—C. Simeons: "*On the Manufacture of Articles in Mica by M. Raphael, of Breslau.*"

June.

C. Kuhn: "*On Pouillet's Method of Applying Lightning Conductors to Powder Magazines.*"—G. Lunge: "*Notes on Technical Chemistry: 5. On the Manufacture of Bone Black, Sulphide of Ammonium, and Super-phosphate of Lime.*"—R. Brimmeyer: "*Notes on Technical Chemistry: 5. On the Decolourising Power of Bone Black. 6. On the Aniline Dyes at the French Exhibition.*"—R. Wagner: "*On the Detection of Woollen Fibres in Silk Yarn and Fabrics.*"

L'Invention. July.

Poirrier and Chappat: "*On a Method of Manufacturing Dyes by the Action of Alcohols on Aniline.*"—Holliday: "*A new Process for producing Red and Violet Aniline Colours.*"—Dangeville and Gaufin: "*A Process for Discharging Aniline Colours from Fabrics.*"—Quenneau: "*On the Use of Crude Limestone for Disinfecting Beet Juice.*"—Junemann, Du Rieux, and Roettger: "*On the Extraction of Sugar from Saccharine Solutions by means of Lime.*" Heurtebise: "*On an Economical Process for Producing Hydrogen Gas by Heating Carbonic Oxide in Contact with Superheated Steam.*"

Génie Industriel. July.

E. Firvet: "*On Measuring Temperatures.*"—Schinz: "*An Improved Pyrometer.*"

Comptes Rendus. August 5, 1867.

Faye: "*On the Influence of External Causes on the Formation of Sun Spots.*"—H. Targioni-Tozzetti: "*On the Wax of* Coccus caricæ."—V. Poulet: "*On the Presence of Infusoria in the Breath of Persons suffering from Whooping-Cough.*"

August 12.

Sir David Brewster: "*On the Authenticity of the Newton and Pascal Correspondence.*"—Chasles: "*On the Authenticity of the Newton and Pascal Correspondence.*"—Duhamel: "*On the Authenticity of the Newton and Pascal Correspondence.*"—E. Hopkins: "*On a Method of destroying the Magnetic Polarity of Iron Ships*"—Wolf and Rayet: "*On the Spectra of Three Stars in the Constellation Cygnus.*"—P. Volpicelli: "*On the Correlations of Galvanometers and of Gauss' and Lamont's Methods of Calculating the Horizontal Intensity of the Earth's Magnetism.*"

Monatsbericht der Königlich-Preussischen Akademie der Wissenschaften zu Berlin. May, 1867.

W. Foerster: "*On the Influence of the Density of the Air on the Rate of a Clock, especially on the Normal Clock of the Berlin Observatory, and on the Performance, when enclosed in an Air-tight Case, of an Electro-magnetic Clock made by F. Tiede.*"—Poggendorff: "*On the Development of Heat in the Path of the Electric Spark.*"

Sitzungsberichte der Kaiserlichen Akademie der Wissenschaften zu Wien. (*Mathematisch-naturwissenschaftliche Classe.*) February, 1867.

S. Stricker: "*Researches on the Vitality of the Colourless Blood Corpuscles in Man.*"—F. Rochleder: "*Note on the Constituents of the Root-Bark of the Apple-Tree.*" J. F. Schmidt: "*On the Recent Changes in the Crater of* Linnæus."

Sitzungsberichte der Königlich-Bayerischen Academie der Wissenschaften. (*Mathematisch-physikalische Classe.*) January 12, 1867.

Vogel, Jun.: "*On Washing, Pressing, and Preparing Peat.*"—Von Gorup-Besanez: "*On Pyrocatechine, a Product of the Action of Iodine and Phosphorus on the Rhenish Beech Wood Tar Creosote.*"

Poggendorff's Annalen der Physik. No. 5, 1867.

H. Knoblauch: "*On the Interference Colours of Radiant Heat*" L. Pfaundler: "*Contributions to Chemical Statics; being an Attempt to explain the Phenomena of Dissociation and Affinity according to a new Theory.*"—J. Phillipp: "*On the Action of Sulpho-cyanide of Potassium on the Salts of Mercury.*"—E. Lommel: "*On the Cause of the Redness of the Sky in the Evening, and of some Phenomena in Connection therewith.*"—A. Haagen: "*A Determination of the Refractive Index and Specific Gravity of some Liquid Haloid Compounds: Tetrachloride of Carbon, Chloroform, Chloride of Ethylene, Bromide of Ethyl, Bromide of Amyl, Bromide of Ethylene, Iodide of Methyl, Iodide of Ethyl, Iodide of Amyl, Bisulphide of Carbon, Chloride of Sulphur, Terchloride of Phosphorus, Terchloride of Arsenic, Pentachloride of Antimony, Chloride of Zinc, Chloride of Silicium, and Chloride of Sodium, à propos of the Question of the Refractive Equivalent of the Elements.*"—E. Schoene: "*Note on Crystallised Hydrate of Potassium.*"—P. Riess: "*On the Cause of the Undulations produced in Metallic Wires by the Electric Discharge.*"—G. Quincke: "*Remarks on L Daniel's Paper, published in the 'Comptes Rendus' of May 11, 1867, on the Transport of Matter by the Voltaic Current.*"—Kindt: "*On the Spectrum of the Phosphorescent Light of Phosphorite, Clorophane, and other Varieties of Flour Spar.*"

PATENTS.

Communicated by Mr. Vaughan, F.C.S., Patent Agent, 54, Chancery Lane, W. C.

GRANTS OF PROVISIONAL PROTECTION FOR SIX MONTHS.

2505. F. H. Pattison, and J. W. H. Pattison, Glasgow, N. B., "A new or improved metal-founder's blacking."—Petition recorded September 4, 1867.

2571. W. Baker, Tipton, Staffordshire, "Improvements in the manufacture of iron, and in furnaces used in the manufacture of iron."

2577. H. R. Lückes, Coleford, Gloucestershire, "The manufacture of artificial or compressed fuel in the treatment of coal, peat, charcoal, tan, sawdust, and woody fibre, whereby those substances, either together or separate, can be utilized when in a state of powder or minute division, and converted into a serviceable fuel."—September 11, 1867.

2608. P. Dumont, Stoke Newington Road, Middlesex, "An improved manufacture of soap." A communication from A. L. Labather, A. Bizet, and L. F. J. Lecat, Place Saint Jacques, Compeigne, France.—September 16, 1867.

2611. C. Holste, Henrietta Street, Covent Garden, Middlesex, "Improvements in blast furnaces." A communication from F. Lürmann, Oesede, near Osnabrück, Prussia.—September 17, 1867.

2637. J. G. Willan, Saint Stephen's Crescent, Baywater, Middlesex, "Improvements in the manufacture of iron."—September 19, 1867.

2679. W. Beardmore, W. Brock, and A. C. Kirk, Glasgow, N. B., "Improvements relating to furnaces."

2685. A. Ziegele, Mincing Lane. London, "Improvements in the manufacture of Epsom salts." A communication from Messrs. Vorster and Gruneberg, Cologne, Prussia.—September 23, 1867.

2711. R. W. Bennie, Glasgow, N.B., "Improvements in the manufacture of moulders' blackening."

2721. J. Fordred, Blackheath, Kent, "Improvements in Bleaching and purifying paraffin."—September 26, 1867.

2731. L. de la Peyrouse, Somerset Street, Portman Square, Middlesex, "Improvements in the treatment of paraffin, fatty, and resinous matters." A communication from L. Krafft, Rue de L'Eglise, Neuilly, near Paris.—September 27, 1867.

2756. E. P. Alexander, Lincoln's Inn Fields, Middlesex, "Improvements in the manufacture of cast steel, and in furnaces to be employed therein. A communication from F. Ellershausen, Ottawa, Canada.

2760. G. Allibon, Worcester, and A. Manbré, Baker Street, Middlesex, "Improvements in Apparatus applicable to the conversion of cereal and vegetable substances into saccharine matter, in treating and purifying saccharine substances extracted from malt, fruits, and vegetables, in treating fatty matters, and in the manufacture of chemical products."—October 1, 1867.

2782. H. D. Pochin, Salford, Lancashire, and E. Hunt, Manchester, "An improvement in the construction of furnaces, flues, and vessels which are subjected to high temperature."—October 3, 1867.

2801. J. Anderson, Londonderry, Ireland, "Improvements in obtaining chlorine, sodium, potassium, phosphorus, and their compounds."—October 6, 1867.

2816. C. D. Abel, Southampton Buildings, Chancery Lane, "A new or improved process and apparatus for refining camphor." A communication from C. E. Perret, Boulevard de Strasbourg, Paris.

2819. D. Swan, jun., Mary-hill, Lanarkshire, N. B., "Improvements in the manufacture of zinc."

2822. J. H. Brown, Romsey, Hants, "Improvements in utilising refuse animal matters, and producing skins and sheets therefrom."—October 7, 1867.

NOTICES TO PROCEED.

1646. E. Meldrum, Bathgate, Linlithgow, N.B., "Improvements in the purification of paraffin."—Petition recorded June 4, 1867.

1652. N. Rausch and E. L. Darlet, Brussels, "Improvements in the manufucture of artificial fuel."—June 5, 1867.

1752. W. E. Newton, Chancery Lane, "Improvements in the preparation of pulp for the manufacture of paper." A communication from A. Aussedot, Rue St. Sebastian, Paris.—June 15, 1867.

2102. C. Klug, Regent Street, W., "An improvement in the preparation of chocolate and cocoa."—July 17, 1867.

2395. C. W. Siemens, Great George Street, Westminster, "Improvements in furnaces, and in processes and apparatus in connection therewith, principally applicable to metallurgical operations."—August 21, 1867.

NOTES AND QUERIES.

Scarlet Ink.—Can any reader of the Chemical News put me in the way to make a scarlet ink, to write scarlet on blue paper with a steel pen?—B. W.

Detection of Sulphur in Petroleum.—Is there an easy test for the presence of sulphur in refined petroleum? I find a difficulty in knowing when it is sufficiently refined, as I have no ready test for this objectionable impurity.—Refiner.

Preserving Fresh Flowers.—I shall be greatly obliged if any of your courteous readers will kindly tell me how I can preserve cut flowers in a room. I remember reading somewhere that, adding some chemical substance to the water, would cause them to keep fresh for a week or more, but I have forgotten the name of the chemical.—Emily D.

Clarifying Oxymels.—Can any of your readers inform me of the plan generally adopted by pharmaceutists for clarifying oxymels?—Scilla Maritima.

Succinic Acid.—Is the following reaction of succinic acid known? It has puzzled me considerably, and, before I found out the cause, led me into some mistakes. I have been in the habit of using succinic acid as a means of separating iron from solutions in quantitative analysis. A few weeks ago I was surprised to find that no precipitate was produced under circumstances in which I fully expected to see one, as I knew there was a ferric salt in solution. The solution was almost neutral, and I had added succinate of potash, but no precipitate of succinate of iron took place. After many experiments to ascertain the cause, I ascertained that the non-precipitation was owing to the presence of acetate of potash, which I had added in rather large quantity to get rid of free mineral acid. Following this up, I have ascertained that the presence of acetic acid, or a soluble acetate, partially or entirely suspends the ordinary reaction of succinic acid in solutions of ferric salts. Perhaps a note of this may prove useful to some of your readers, if inserted in your valuable "Notes and Queries" column.—J. Hooper.

German Yeast.—In reply to F. Ireland, a valued correspondent sends us the following information. The bulk of what is erroneously called in this country German yeast is made at Schiedam, Delftshaven, and Rotterdam. In a large cylindrical wooden vat, 1·15 meters high, and 1·6 meters diameter, a large quantity of tepid water is poured, and 180 kilogrammes of coarsely-ground rye and barley malt are added; this mixture is beaten into a thin paste, and the vat closed with a well-fitting lid; in order to prevent too rapid cooling, it is covered with a stout blanket, and left at rest for about two hours. At the end of this time the paste is diluted with very cold water, and thin distillers wash till perfectly fluid. Beer-yeast of good quality is now added in the proportion of half a kilogramme for every 2,000 litres of fluid; the whole is well stirred and left to settle. Fermentation soon commences, and after from five to seven hours the larger portion of the fluid is carefully drawn off from the sediment and collected in a tank; thence it is pumped into shallow wooden troughs placed under shelter and surrounded on all sides by louvre blinds. Each trough is 4 meters long, 2·75 broad, and 0·5 deep. The liquid shortly becomes very turbid, a thick mass, not unlike boiled starch, settles to the bottom, and the surface also becomes covered with a thick coherent cream-like scum; each of these deposits is the yeast; after 24 hours the intermediate fluid is withdrawn and returned to the original vat, whence it had been taken. The yeast is collected in bags made of strongly-woven linen canvass, and submitted to great pressure for 24 hours; it is then fit for use, and is exported as dry yeast. When one-third by weight of best barley malt, and two-thirds of rye of good quality are used, and the operations carefully conducted, and when it is *not* intended to obtain spirits, the quantity of yeast may be very much increased. There is no truth in the assertion that such materials as chalk, bone-ash, plaster of Paris, French chalk, pipe-clay, etc., are used as adulterants. A small quantity of the best starch is, in the summer, sometimes mixed with the yeast. It makes it keep better in hot weather.

To Preserve Fresh Flowers.—Put a pinch of nitrate of soda into the water every day when you change it. This will preserve flowers for a fortnight. Nitrate of potash, in powder, has nearly the same effect.—Isabel de W.

Detection of Sulphur in Petroleum.—"Refiner" will find the following plan, suggested I believe by Dr. Vohl, answer his purpose:—Digest the oil for some hours at a gentle heat with a small piece of sodium. Water being added, the aqueous solution is to be tested with nitroprusside of sodium. The presence of sulphur is shown by the production of the well known purple colouration.—E. S. P.

Scarlet Ink.—Take garancin of best quality one ounce, digest with liquor ammoniæ one ounce, add one pint of cold distilled water, triturate together in a mortar, filter, and dissolve in the solution half an ounce of gum arabic; or take pure carmine twenty grains, liquor ammoniæ three fluid ounces, dissolve, then add eighteen grains of powdered gum.—Dr. A.

Succinic Acid.—The property of acetic acid to prevent the ordinary reaction of succinic acid alluded to by your correspondent, J. Hooper, in your last week's Notes and Queries column (*American Reprint, Dec.*, 1867, *page* 327), is not new, although it does not seem to be generally known. More than forty years ago, two French chemists, named Lecann and Serbat, pointed out that the presence of acetic acid takes from succinic acid the power of forming precipitates with solutions of iron, copper, lead, and barium. Neither will a mixture of acetate and succinate of potash precipitate solutions of these metals.—Abel Scott.

Development of Heat.—I was reading in a scientific work the other day that air may be driven with so much force as to set solid substances on fire; and that ice may be dashed with such violence against another piece of ice that sparks of fire will be produced by the collision. Can any one tell me where the record of these experiments may be found, and who was the experimenter? I am aware that Sir H. Davy produced heat by rubbing two pieces of ice together, but I have never heard of fire being produced by the ice; perhaps some of your readers may enlighten me.—Crucis.

Sulphur in Petroleum.—It is not quite clear whether your correspondent wants a test for sulphur or any of its acids, since in refining petroleum sulphuric acid is used; however, it seems at all events best to convert any sulphur compounds into sulphuric acid, and test for the latter. The oil may be decomposed by careful treatment with nitric acid, and afterwards the residue, after complete decomposition of the oil, diluted with water, and treated with nitrate of baryta to detect sulphuric acid. It must not be lost sight of that when sulphuric acid is used for refining, this will have to be removed entirely first, while one must also bear in mind that its use entails the possibility of engendering sulpho-oils not attacked by alkalies.—A.

Clarifying Oxymels.—Oxymels can be obtained brilliantly bright without any body being used for their clarification, by keeping the honey at a temperature of 212 deg. Fahr., and separating the scum rising to the surface as long as any is formed, then filtering through twill cloth. The colour of the oxymel, however, is much darker than the product obtained by the process of British Pharmacopœia in 1867, which merely directs the honey to be melted.—Scilla Maritima.

Development of Heat.—In my early chemical days I was in the habit of attending lectures at the Polytechnic Institution, London. The lecturer, whenever he was discoursing upon heat, always brought in this story: "There is an old saying, that you may go to the mint, and carry away as many sovereigns as you please, provided you pick them up with your naked fingers as they fall from the press." The reason you were allowed this privilege, was because they dropped from the die so hot it was impossible to hold them. Can any of your readers say whether the story is a myth, or is it founded upon some former press, as I visited the mint a few days since, and found that the coin came from the stamp comparatively cold.—Vraisemblance.

Preserving Fresh Flowers.—Flowers may be kept in pretty fair condition, for say a week or ten days, according to the species selected for bouquets and the time of the year, by renewing the water every alternate day, and while doing so rejecting decayed flowers and leaves, and taking care to cut off from the stems immersed in water, with a sharp pair of scissors, about from a quarter to half inch of the length; then should be added to the water about a pinch of salt, and a few grains of saltpetre for every pint of fluid; when flowers are very much faded they may be revived by immersion of the stems for two or three minutes in hot water, or better yet in strong spirits of wine, or Eau de Cologne; in some cases liquid ammonia may be advantageously applied to the stems for a few minutes to revive flowers. These recommendations are applied by several of the largest horticulturists of Ghent and other parts of Belgium, and found to answer in practice very well if properly applied. To keep well, flowers should not, after being cut, be placed in localities where there is tobacco-smoke, or bad ventilation, neither should the rooms be too much heated.—A.

Gun-Cotton.—Can any of your readers inform me what works have been published upon the new explosives, nitro-glycerine, gun-cotton, and Scheeltzes wood gunpowder, and if any and what chemical authorities have reported upon them?—K.

Transparency of Red Hot Metals.—Sir,—I noticed a short time since, in one of the numbers of the Chemical News, some letters on the transparency of red hot metals. A few weeks ago, I went over some steel works in the North of England, and there the manager spoke of it as a well-known fact that steel at a white heat was transparent. In proof of it he showed that, when the molten metal was being poured out, the edge of the crucible appeared to be distinctly visible through the molten metal. This could only be seen directly the crucible was taken out of the furnace, before it had cooled in the least. If the metal is not transparent will any of your correspondents say whether this appearance can be accounted for in any other way?—Exon.

Bleaching Palm Oil.—Sir,—Would you oblige me by giving me some information through the medium of your Notes and Queries column in reference to "Bleaching Palm Oil." I am connected with a works at which palm oil is used, and occasionally it is required to be bleached. I have applied myself to the task of bleaching it with only indifferent success; I follow (as I believe) the method once patented by Mr. Watt. I first introduce the orange coloured oil into a tub open at the top into which an open steam jet is conducted; I allow the steam to blow into it until its temperature marks 130 deg. or 134 deg. F. I then add to the oil a saturated aqueous solution of 1 lb. commercial "Bichrome" for each cwt. of oil to be operated upon, as nearly the same temperature as the oil as possible. I then add a quantity of hydrochloric acid (commercial) equal in weight, as nearly as I can guess, to double the weight of bichrome before being dissolved. I then agitate the mixture with a paddle, and the characteristic odour of nascent oxygen is quickly perceptible, which continues to emanate for 1½ or 2 hours without decolourising the oil to any sensible extent, the agitation being kept up the whole of the time. Parnell describes this process as requiring only five minutes for decolourising the oil. I must therefore be most egregiously at fault, for even after the expense of so much labour the product is not at all satisfactory. I have even employed double the quantity of the bleaching agents with no better success.—Geo. Johnson.

TO CORRESPONDENTS.

F. E. Wright.—For the purpose you name we recommend "Bloxam's Chemistry," published by Churchill. The price is not so high as you say you are willing to go to, but we presume that will be no objection.

A subscriber from the commencement of the Chemical News.—The sign ∞ in mathematical language is the sign of infinity. Consult the article "Crystallography" in Watt's Dictionary.

W. E. Bickerdike.—Received with thanks. We believe a native hydrated ferric oxide is now used.

E. M. Delf.—We are sorry we cannot accede to our correspondent's request. The journal is not now in our possession. It can, however, be obtained through a foreign bookseller.

H. W. Tapper.—Will o' the Wisp paper is simply gun-paper, *i.e.* paper rendered explosive like gun-cotton. The flame may be coloured by soaking it in nitrate of baryta, strontia, etc.

W. E. Gill.—Received with thanks.

Wm. Johnson Sollas.—A letter addressed to Messrs. Bailey and Sons, Wolverhampton, will obtain for you the desired information.

Henri Du Chemin-Creux.—Your letter has been forwarded.

H. Bowler, Philadelphia.—The journal shall be forwarded if possible.

J. D. M.—No fuller particulars have been published for obtaining oxygen from bleaching powder than appeared in the number of the Chemical News containing the announcement.

Ellora.—The new cement you refer to is not oxy-chloride of manganese, but oxy-chloride of magnesium. It was described by M. Sorel, before the French Academy, a few months ago.

A. R. A.—Nearly all specimens of fluor spar are very phosphorescent. The native carbonates of lime are so in a less degree. M. Heinrich has published some very interesting experiments on this subject. He exposed the substances for about 10 seconds to the light of a clear day, but out of the direct rays of the sun to prevent their becoming heated; the observer was for 30 or 40 minutes previously in a perfectly dark chamber. The diamond and fluor spar shine for above an hour, but nothing else for more than a minute. When shining, if a deep cut be made in the substance, it will be seen to be as luminous at the bottom as at the surface.

Chemicus Junior.—They will be proceeded with.

F. J. Hawker.—The suggestion shall receive attention. We are greatly obliged for the same.

St. Helens.—Chinese blue is a synonym of Prussian blue. See Chemical News, vol. xv., p. 310. (*Eng. Ed.*)

An Old Subscriber.—They were discontinued mainly because to do bare justice to the subjects would have nearly filled our columns each week. Everything of importance will still be noticed.

Messrs. Cocker, Brothers.—We are unable to give our correspondents the address required. A short note in our advertising columns would doubtless procure it.

W. H. H. Haverford West.—1. Plateau's description for preparing glycerine soap liquid was given in our 4th vol., page 290. (*Eng. Ed.*) 2. Dangerous in a small room. 3. The heat evolved in the combustion of a certain weight of mixed oxy-hydrogen gases is the same whatever the rate of combustion. 4. If our correspondent will favour us with a call we will see if an arrangement can be made.

Enquirer.—The report has been long out of print.

Theta.—Hayesite is another name for Boronatrocalcite.

S. A. Y.—No process is yet known for the quantitative separation of the two metals.

Distiller (*O. J. Q.*)—Bisulphide of Carbon has been found in many specimens of American Petroleum. The portion distilling below 80 degs. C. contains nearly all.

James Peterson.—Artificial lithographic stones have been made in France, but we know not with what success. Their manufacture should not present any insuperable difficulty. All would depend on the demand, and price willing to be paid. Send a specimen of what you have produced.

F. Garland.—You will probably find a paste made of starch, glycerine, and plaster of Paris, answer your requirements. It will retain its plasticity and adhesiveness longer than most other cements.

W. O. Lever.—The impurity has got into your test solution from the glass bottle in which it has been kept. This is a far more frequent source of impurity in reagents than many chemists would imagine.

Books Received.—The *Philosophical Magazine* for October.—*Scientific Review.*—"Second Report of the Quekett Microscopial Club." —Fourth Report of the British Association of Gas Managers, with Rules, Regulations and List of Members.—The *Shipwrecked Mariner* for July.—*Science Gossip*, for October.—The Calendar of the Pharmaceutical Society of Great Britain for 1867–68." *Popular Science Review*, for October. "A Lecture on the Sewage difficulty," by Baldwin Latham, C.E. "An Introduction to Pharmaceutical Chemistry," by John Attfield, Ph.D., F.C.S. London: Van Voorst. "The Ophthalmic Review," by J. Zachariah Lawrence. London: Hardwicke. "Squire's Companion to the British Pharmacopœia, 1867." Fifth Edition. London: Churchill. "Practical Hints to the Medical Student," by William Allen Miller, M.D. LL.D. "The Nation" for October 10, 1867. "The Chemical News," American Reprint, Vol. 1. No. 4, for October 1867. "Silliman's American Journal of Science" for Sept. 1867. "The Standard" for October 21 and 22.

Communications have been received from Lord Sackville Cecil; F. Rimington; E. M. Delf; Samuel Highly; Robert Hogg; Dr. Sansom (with enclosure); John Newlands; James Brown and Son; H. W. Tapper; Clayton and Co.; Dr. R. Oxland (with enclosure); William Baxter (with enclosure); R. Robertson (with enclosure); W. Bywater (with enclosure); J. Heywood; Samuel Sharpe; J. Blackburn; Dr. G. Lunge (with enclosure); F. Versmann; Maxwell Simpson, F.R.S. (with two enclosures); W. Millar; L. Stokes (with enclosure); D. Forbes, F.R.S. (with enclosure); E. C. C. Stanford; H. Dixon; W. H. Smith (with enclosure); L. Power; W. E. Gill (with enclosure); J. Maculloc (with enclosure); H. Dixon; Henry Bower; Prof. H. How (with enclosure); W. Inglefield; W. Godwin (with enclosure); J. Watson; J. D. Muter; P. Sharp (with enclosure); S. W. Hinde; C. G. Williams, F.R.S.; F. Cochrane; Dr. Odling, F.R.S (with enclosure); W. H. Harrison; A. Smart (with enclosure); J. Salter; T. Maguire (with enclosure); W. Tillson; H. Day (with enclosure); O. Schmidt; W. J. Suffolk; H. Monfit; W. Johnson Sollas; Dr. Adriani; John Heywood; Alex. Parkes; J. Herschel; John Bray, F.C.S.; E. P. Sandbury (with enclosure); R. Corbett (with enclosure); G. F. De Winton; E. Schenck (with enclosure); John Spencer; Maclachlan and Stewart; B. W. Gibsone; T. A. Readwin; P. Hart (with enclosure); S. Mellor; F. J. Hawker; Cocker, Brothers; G. Johnson (with enclosure); T. J. Reeves (with enclosure); R. Jamieson (with enclosure); Dr. A. Claus (with enclosure); Rev. C. W. Kett; R. E. Bibby; C. Umney (with enclosure); S. F. Peckham (with enclosure); W. H. Hofman (with enclosure); E. C. Durham; W. Graves (with enclosure); R. M. Brown; W. Andrew (with enclosure); A. B. King (with enclosure); H. Henson; — Remington (with enclosure); F. O. Jackson (with enclosure); R. Simpson; A. Jones; M. Wilson Smythe (with enclosure); W. Bird; Samuel Highly (with enclosure); J. Cliff; C. Nelson; B. W. Gibsone; W. Bywater; J. W. Swindells; W. Chapman; J. Johnson; Professor Gustavus Hinrichs (with enclosure); W. Y. Dent; Miss Becker (with enclosure); W. A. Townsend and Adams; J. Seeley (with enclosure); Professor A. H. Church (with enclosure); D. Forbes, F.R.S. (with enclosure); Dr. W. A. Miller, F.R.S.; Henry Seward; J. D. Munro; W. Mackie; William Rogers (with enclosure); A. Francis; W. Hickman; J. Jelley; E. Delafield; W. Thomas (with enclosure); Rev. B. W. Gibsone; John Parnell (with parcel); Mr. Thorne; R. Campbell, jun., Canada; "Enquirer"; W. Sugg; C. Mullingbach (Hanover); Arthur Warner; C. R. Wright (with enclosure); H Greenwood (with enclosure); Alment and Johnson (with enclosure); W. Preece (with enclosure); Bailey and Son (with enclosure).

THE

CHEMICAL NEWS

AND

JOURNAL OF PHYSICAL SCIENCE:

(WITH WHICH IS INCORPORATED THE "CHEMICAL GAZETTE.")

A Journal of Practical Chemistry

IN ALL ITS APPLICATIONS TO

PHARMACY, ARTS, AND MANUFACTURES.

EDITED BY
WILLIAM CROOKES, F.R.S.

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MDCCCLXVIII.

VOLUME II., AMERICAN REPRINT,

BEING PARTS OF

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THE NEW YORK PRINTING COMPANY,
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NEW YORK.

PREFACE

The publishers of the authorized American reprint of the London Chemical News submit their second semi-annual volume to the public with gratification, and desire to renew their indications of its permanent interest and value. This volume will bear witness, in common with its predecessor, to the high place occupied by this journal in the region of pure and practical science.

In these earlier stages of the re-issue in America, it may be well to repeat that the Chemical News is not a journal of recent origin. In connection with its predecessor, The Chemical Gazette, it has, for more than twenty-five years, fully and faithfully represented the progress of Chemistry and cognate sciences in England and throughout the world. Its present position has been attained by successive improvements, until it now numbers among its contributors nearly every chemist of note in Europe and America. In its columns some of the most important chemical and physical discoveries have for the first time been made known, and investigators frequently make use of its pages to secure priority of a discovery, by the bare mention of facts and results, before publishing their full papers. The good influence of this journal, in the progress of pure chemical research and advancement, has been felt for years. It is now everywhere cited as the great repository of chemical knowledge, discussion, and authority. Its editorial staff is made up of gentlemen in the first rank of science. Moreover, as the Chemical News is not the organ of any institution, clique, professional or trading firm, its conductors are under no liability to act or write, at any time, in other than a fearless and independent manner. No trade puffs are ever inserted here—no unworthy books or patents are ever commended. Its character in these and kindred respects is one of honest pride with its proprietor and publishers.

But however high the position of this journal as a treasury of all that is fresh and valuable in chemistry, it would be a mistake to consider the Chemical News as covering that department of science alone. It is hardly less a periodical of importance to the medical profession, for it often contains papers giving the methods and results of the thorough application of chemistry to medicine. These are of such a nature as to present the fruits of studious observation and thought in a manner elsewhere unattained. Several papers in the current volume may stand in support of this, as also to show the attention given by its editors to public sanitary questions.

The Chemical News, again, is a rich medium of information to every theoretical and practical pharmaceutist, druggist, and apothecary. In its reports of the British Pharmaceutical Society, and the British Pharmaceutical Conference, with occasional papers upon special topics, and its Chemical Notices from Foreign Sources, it constantly supplies matter indispensable to every well-trained and furnished member of this respected and important calling. It affords much gratification to the American publishers to be the means of advancing the character and qualifications of so numerous and wide-spread a class in the community.

Numbers of extensive manufacturers in this country, of many sorts, have for years past prized the Chemical News as a valued auxiliary and guide in the various processes by which their goods and fabrics have been prepared for consumption and use, and the number of such persons is on the increase.

Photography and the finer arts are the objects of watchful and conscientious notice at the hands of those who prepare the Chemical News. New processes and profitable suggestions in this connection are not seldom brought out for the first time in its pages.

In its analyses of metals, its record of the developments in mining throughout the world, its attention to mineralogy, its discussions of mechanics and electricity, it is believed to be without a competing rival.

To fill so wide a range of application is apparently difficult of accomplishment. That it is done, and in the most thorough way, no regular reader of the Chemical News need be informed.

The general features of this journal may be thus summed up in detail:

1. Leading and Editorial articles by the well-known Editor, Mr. William Crookes, F.R.S., on all topics within the proper scope of the journal. 2. Graphic pictures of Foreign Science, by its Paris Correspondent, the Abbé Moigno. 3. Reports of the learned Societies—the Chemical of London, the Royal, the Pharmaceutical, the Royal Institution, Manchester Literary and Philosophical Society, British Medical Association, British Association for the Advancement of Science, Royal Geological Society of Ireland, Quekett Microscopical Club, Glasgow Chemical Society, Philosophical Society of Glasgow, French Academy of Sciences, etc., etc. 4. Fresh, prompt, and impartial Notices of Scientific Books. 5. Columns for Correspondence. 6. Chemical Notices from Foreign Sources, giving a condensed account of every important chemical paper in the world, as soon as it is published. 7. Miscellaneous Paragraphs. 8. Contemporary Scientific Press, a feature introduced at the suggestion and request of many leading chemists. It purports to give, as soon as possible after publication, the title of every chemical paper in the world. 9. Lists of English Patents. 10. Notes and Queries. 11. Brief Answers to Correspondents, Lists of Communications, Books Received, etc., etc.

Desirous of increasing the value of the Chemical News in America, and widening its influence, the publishers have been induced to add a new feature to it which cannot fail to give it high importance in a commercial as well as scientific aspect. They therefore propose to give hereafter, in connection with the reprint of the London publication, a monthly general price-current of drugs, paints, and oils. The title of the publication henceforth will therefore be, The Chemical News and Journal of Physical Science, and American Druggists' Price-Current.

W. A. TOWNSEND & ADAMS.

June 1, 1868.

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Vol. II. No. 1. American Reprint.

ON THE MANUFACTURE OF SALTPETRE.

BY J. H. SWINDELLS.

I HAVE lately examined several lots of refuse (muriate of soda, etc.) from various saltpetre makers, and find a serious loss of potash and other materials. The refuse from saltpetre works is mostly sold for manure, either alone or mixed with other substances. From the analysis I give below it will be seen that this refuse in many cases contains a large amount of potash, etc., thus showing bad and careless working.

Analysis of muriate of soda.

Muriate of soda,.................	70·50
Muriate of potash,...............	8·81
Nitrate of potash,...............	6·15

The rest was made up of sulphate of lime, insoluble matter, and water. Another sample showed very near working; it gave me

Muriate of soda,.................	97·13
Nitrate of soda,.................	1·10

Insoluble matter, sulphate of lime, etc., formed the remainder.

Twelve samples of refuse gave me an average of 5·6 per cent. of potash lost in working. Several samples gave me a large amount of insoluble matter, owing, I should imagine, to the "sweeping up" of the factory and to the muriate of potash, which in many cases I know contains a larger amount of foreign matter than it ought to do, although it is sold, generally speaking, with a guarantee that it contains "80 per cent."

I will now give a short account of the method generally followed in the manufacture, and a recommendation of my own to prevent loss of materials. In all the manufactories I have been able to visit the process was as follows:—A certain weight of muriate of potash and nitrate of soda was put into an iron pan along with a quantity of water. The steam was then blown into the "mixture," which is stirred about till such time as it is thought the muriate and nitrate are dissolved. The liquid is then run or syphoned into a tank, and after a certain time the resulting crystals are removed, and invariably refined. The last process is simply done by dissolving down the crystals and re-crystallising. I found that no hydrometers were used for ascertaining the density of the liquids, but that everything was done on the "guess" principle. By following this process a loss will always occur; for how is it possible to tell when all the materials have thoroughly dissolved? Moreover, it takes a longer time to dissolve the materials together than it would occupy if the muriate and nitrate were dissolved separately.

The chemical action is too well known to be described here; I will, therefore, proceed to consider the best way of preventing any loss of materials. I would recommend that the muriate of potash should be dissolved in as little water as possible; then dissolve the nitrate of soda also in as little water as may be convenient, add the two solutions together and boil for one hour or so. This will precipitate a portion of the muriate of soda, which may be "fished" out of the pan; the liquid will now be found to have a specific gravity of about 1·200, 1·250, or 1·275, according to the manner in which the "dissolving and boiling" down is conducted. The liquid, after remaining at rest 2 hours or so, may be run into the coolers in the usual way to allow the nitrate of potash to crystallise out of the liquor; of course all the nitrate of potash does not crystallise out. A quantity remains in the "mother liquor," a portion of which may be used for dissolving the raw materials. The mother liquor, however, should not be too strong, as neither the muriate nor nitrate dissolve well in strong mother liquor; about 15 Twaddell will be found about the strength. When the "mothers" begin to increase and become too many for "dissolving" purposes, they must be salted down and crystallised. This is done by placing them in an evaporating pan, and boiling them down to about 35°, or so, Twaddell; care must be taken to remove the muriate of soda as it falls to the bottom of the pan. This may be done by means of perforated ladles. There is sure to be a little nitrate of potash clinging to the muriate of soda, and the manner of separating this must be considered. Even when there is so much as 5 per cent. of potash mixed with the muriate of soda, it will scarcely pay to extract it, unless coals are very cheap; for the entire mass would have to be dissolved, and the liquor evaporated down to the crystallising point. I would recommend the following plan:—As the muriate of soda is taken out of the pans it should be placed in a strong tub with a kind of filter bottom—this tub to be provided with a tight-fitting top, through which a pipe passes for the admittance of steam. After the muriate of soda has been put in this tub and the top secured, the steam is blown through the mass for 15 minutes, and the liquor run off by means of a tap placed at the bottom of the tub. This liquor may be used for dissolving the raw material, and all over and above that required for this purpose must be evaporated down along with the mother liquor. By all means, the manufacturer, if he is not capable of making his own analyses, should have analyses made from time to time of his muriate of soda; he will then be able to form the best opinion of what he is doing, and thus avoid any unnecessary loss. Owing to the low price of saltpetre, very close working is required to make the business pay, and adulteration is coming much into vogue. The adulteration is practised by the manufacturer, and the saltpetre also meets with sophistication after it leaves his hands. The most glaring adulteration is common salt and alum. The muriate of soda from saltpetre making, is cleansed and mixed in the proportion of 2 cwt. to the ton of petre. In the process of refining the saltpetre, from 2 to 3 cwt. of alum is sometimes used. Nitrate of potash adulterated like this will not of course do for the gunpowder makers, but for many other purposes this adulteration often passes unnoticed.

ON SESQUISULPHATE OF IRON AND FERRIDCYANIDE OF POTASSIUM AS A TEST.

BY EDWIN SMITH, M.A.

HAVING occasion to explain to my class the deoxidising property of sulphurous acid, it occurred to me that the property in question might be illustrated by exposing a slip of bibulous paper, dipped in a mixed solution of sesquisulphate of iron and ferridcyanide of potassium, to the vapour rising from a bit of sulphur burning in air. For should the persalt of iron be reduced to a protosalt by giving up an equivalent of oxygen to the sulphurous acid, a blue reaction ought to take place with the ferridcyanide of potassium present in the solution. I tried the experiment, and the colour of the paper was instantly changed to a beautiful blue. A solution of sulphurous acid, or of a sulphite or hyposulphite, gives the same result; while only a very slight greenish tinge is imparted to the mixture by a sulphate, except in the case of protosulphate of iron. With this exception, a useful test seems to be afforded between sulphites and sulphates. I find the same test will discriminate a nitrite from a nitrate. To the mixed solution of sesquisulphate of iron and ferridcyanide of potassium add a few drops of nitric acid; then add a little of the solution to be tested. If the latter contains a nitrite, a greenish-blue precipitate will begin to fall, and quickly increase; if a nitrate, only a slight greenish tinge will be imparted to the test. Nitric oxide or nitric trioxide passed into the mixed solution throws down the same characteristic precipitate, which is produced by the decomposition of a nitrite in the previous case. Carbonic oxide will act in the same way, and if a slip of paper dipped in the test-mixture be held over the clear part of a bright coal fire, it turns blue with the carbonic oxide or sulphurous acid there given off. Again, if a bit of phosphorus is dropped into a little of the test in a porcelain dish, the phosphorus immediately becomes coloured a greenish blue, and on stirring about, gradually imparts the same tinge to the solution. Phosphorous acid may be discriminated from phosphoric acid, just as sulphurous acid was distinguished from sulphuric acid, by the blue reaction with our test. Thus also phosphites are distinguished from phosphates. A solution of phosphorous acid showed the reaction readily on being shaken up with the test. Lastly, if copper turnings are boiled in the latter for a few minutes, a greenish-blue tint is imparted to it, which becomes gradually deeper with the oxidation of the copper and the consequent reduction of the persalt of iron to the state of a protosalt.

ON A
COMPOUND FORMED BY THE DIRECT UNION
OF
ALDEHYDE AND ANHYDROUS PRUSSIC ACID.

BY MAXWELL SIMPSON, M.D., F.R.S., and A. GAUTIER, M.D.

The synthesis of alanin from aldehydate of ammonia, prussic and hydrochloric acids, and the formation of lactic acid by the action of the same acids upon aldehyde in presence of water, render it highly probable that an intermediate body exists, resulting from the direct combination of hydrocyanic acid and aldehyde. It is this body which forms the subject of the present memoir.

If one molecule of anhydrous prussic acid is added to one molecule of dry aldehyde, contained in a mattrass surrounded with a freezing mixture, the two liquids mix without combining chemically, and their chemical combination is not accelerated by heating at 100° C. If, however, we leave them in contact for ten or twelve days, at the ordinary temperature of the air, they gradually unite, forming a perfectly transparent and colourless liquid. On subjecting this to distillation it was observed that hardly a drop passed over at 100°, although we operated upon a large quantity of liquid (34 grms.), a small quantity between 160° and 174°, and the remainder of the liquid between 174° and 185° C. On re-distilling the latter portion, in order to fractionate it, it was found that the greater part passed over at about 183° C. Notable quantities of the liquid, however, came over between 40° and 60° C., consisting principally of the parent bodies which had been dissociated by the simple vaporisation of the liquid. On leaving these bodies thus dissociated once more in contact for some days the point of ebullition rose, as before, to 183° C. The fractions boiling at 180°, and between 183° and 184° C. gave, on analysis, the following results:—

Product boiling at	Product boiling between	Theory.
180° C.	183°-184°.	CNH,C_2H_4.
C 49·78	51·70	50·71
H 7·44	7·64	7·04
N 20·42	"	19·83

These analyses prove that the body in question results from the direct combination of one molecule of aldehyde and one molecule of anhydrous prussic acid, or at least of equal numbers of molecules of these bodies, and that its point of ebullition is intermediate between 180° and 184°. We have tried the above experiments on mixtures containing the two generating bodies in various proportions, but always with the production of the same body. The name we propose for this compound is cyanhydrate of aldehyde, which is simply founded upon its synthetical formation.

Properties.—The cyanhydrate of aldehyde is a colourless liquid, having a faint odour of its generators; it has a bitter and acrid taste; it does not crystallise at = 21° C., but becomes syrupy. It can bear the temperature of 150° for a considerable time without suffering decomposition; at 180°, however, slight dissociation commences, and the liquid must be rapidly distilled in order to avoid the loss of a considerable quantity. It is soluble in all proportions in water and alcohol. It may be heated with water in a sealed tube to 150° C. without suffering the slightest decomposition, and the entire liquid can be recovered by distillation. Caustic potash appears to separate it into its two generators, forming cyanide of potassium and resin of aldehyde. A little ammonia is also evolved, owing, probably, to the decomposition of the cyanide of potassium.

Gaseous ammonia is absorbed by this body, with the production of a base, which gives a precipitate with bichloride of platinum. Our analyses of this salt have not yet enabled us to ascertain the composition of the base.

A strong solution of hydrochloric acid acts with great violence at the ordinary temperature of the air upon cyanhydrate of aldehyde. If, however, the cyanhydrate is introduced into a balloon surrounded with a freezing mixture, and the hydrochloric acid added gradually, the two liquids mix without any reaction taking place. On removing the open balloon from the freezing mixture, and placing it in water at the ordinary temperature, the reaction soon commences, and proceeds gradually till the entire liquid becomes a mass of crystals. These were twice treated with absolute alcohol, and the alcoholic solution evaporated, in order to separate the chloride of ammonium which is formed. A syrupy liquid was thus obtained, which was saturated at 100° C. with pure oxide of zinc and filtered. The filtered liquor gave, on cooling, a mass of beautiful prismatic crystals. These were recrystallised, heated in an oil-bath to 150° C., and analysed. The numbers obtained prove that the body in question was the lactate of zinc, as will be seen from the following table:

Experiment.		Theory. $C_3H_5Zn'\Theta_3$.
C	29·84	29·63
H	4·52	4·13
Zn	26·77	26·75

The following equation explains the formation of this acid.

$$C_2H_4\Theta,HCN+HCl+2(H_2\Theta)=C_3H_6\Theta_3+NH_4Cl$$

The insolubility of this salt in alcohol, its non-decomposition at 150° C. (sarcolactate gives off vapours at this temperature), and its crystalline form, sufficiently prove that it is the ordinary lactate of zinc, and not the sarcolactate.

The cyanhydrine of aldehyde is, then, isomeric, and not identical with the cyanhydrine of glycol of Wislicenus, seeing that it gives ordinary lactic acid with hydrochloric acid, and that it is converted into a resin by potash instead of giving sarcolactic acid. Moreover, the cyanides of the glycols have no disposition to evolve prussic acid, and cannot be obtained in quantity and in a state of purity, whereas our body can be found in any quantity and perfectly pure.

We have endeavoured to obtain the vapour density of this body by Dumas' method, but without success. On heating the balloon containing our body till 210° in an oil-bath and removing it from the bath, we observed that the aldehyde had been converted into a resin. On deducting its weight from the weight of the balloon the density of the vapour approached very near that of prussic acid. It appears to us, however, to be sufficiently proved that this compound only contains one molecule of each of the parent bodies, from the facts that it gives lactic acid with hydrochloric acid, and that it separates by the action of heat into prussic acid and ordinary aldehyde, and not into aldehyde or paraldehyde.

It appears to us that time is a very striking example of an organic body which is dissociated by heat and reconstructed by time.

NOTE ON THE

ACTION OF PEROXIDE OF MANGANESE UPON URIC ACID.

BY C. GILBERT WHEELER.

The oxidising action of a peroxide upon organic substances varying to some extent according to the peroxide employed, I have investigated the action of peroxide of manganese upon uric acid.

If uric acid and peroxide of manganese are heated together with a like quantity of water, and sulphuric acid is added in small portions at a time until no further action is to be observed, the black pasty mass then filtered, and the filtrate evaporated to about one-fourth of its original volume, there is obtained, after considerable time, a quantity of large hexagonal crystals, which by analysis and characteristic reactions was found to be parabanic acid.

If uric acid is heated with a large quantity of water only, until the latter is brought to the boiling-point, and then peroxide of manganese added as long as evolution of carbonic acid occurs, and the mass filtered, there remains on the filter peroxide of manganese and oxalate of manganese, while the filtrate on being somewhat concentrated yields crystals, which if again dissolved and treated with animal charcoal may be obtained colourless and quite pure. They were tasteless, rather difficultly soluble in cold but readily soluble in warm water; the solution gave with chloride of mercury no precipitate, while a very voluminous one was obtained on adding the nitrate of the same base; nitrate of silver and ammonia gave a white glistening precipitate; on heating, cyanide of ammonium was evolved.

0·3595 gramme yielded on combustion 0·133 water and 0·398 carbonic acid; which relation indicated the substance to be *allantoin*.

	Found.	Theory.
C	30·13	30·4
H	4·09	3·8

The mother-liquor contained much urea; also an amorphous substance, the quantity of which was too trifling to admit of an analysis. The action of the peroxide of manganese may be explained by the following equation.

$$3\,(C_{10}H_4N_4O_6)+6MnO_2+8HO=2(C_8H_6N_4O_6)+2(C_2O_2H_4N_2)+4(MnO,C_2O_3)+2(MnO,CO_2).$$

If uric acid is heated with peroxide of manganese in the presence of but a small quantity of water there is formed urea, oxalic and carbonic acids, and but a very small quantity of allantoin; the action of peroxide of manganese upon uric acid resembles therefore very closely that of peroxide of lead.—*American Journal of Science, Sept.* 1867.

NOTE UPON A

METHOD OF VARYING WEIGHTS BY MINUTE QUANTITIES.

BY J. A. BROUN, F.R.S.

A notice of a Gravimeter, proposed by me, appeared in the *Proceedings* of the Royal Society of Edinburgh early in 1861. The instrument, with various modifications, was forwarded to me in India about three years thereafter, and was found to have several imperfections which could have been corrected only by my own supervision of the work as it proceeded. His Highness the Rajah of Travancore has been good enough to sanction a sum of money for the construction of a second instrument, with all the precautions experience has suggested.

There are two methods by which the observations may be made. One, by which there is a constant angle of torsion of a single wire giving a variable angular movement to the weight suspended by two wires (depending on the force of gravity at the place of observation). The other, by which the weight suspended is varied, and the angles of torsion of the single wire and movement of the weight are constant. This second method I had at first rejected, as experience had shown me the difficulty of changing the weights without affecting the stability of the instrument. I desire now to note that I devised, about a year ago, and communicated to different men of science, a method of varying the weight, suspended with the greatest delicacy, and without jar to the instrument. This method consists in suspending from the weight a metallic wire, which enters a piece of barometer tube fixed below the instrument: by means of a screw entering a cistern below the tube, mercury (or another fluid) can be forced into the tube so as to immerse the metallic wire in the fluid; the wire being properly chosen as to fineness, or as to its specific gravity compared with that of the fluid, and the height of the fluid being read to a thousandth of an inch as in the barometer, the weight suspended can be made, by turning the cistern-screw, to vary gently and gradually, by as minute a quantity as we please; while the eye is occupied with the coincidences of the telescope wire and its images, which indicate the constancy of the angles of torsion of the single and double wires.

Although the difference of the specific gravities is considerable, I propose to employ iron for the wire and mercury for the fluid.

It has occurred to me that this method of varying a weight might be of use in other branches of research.—*Proceedings of the Royal Society of Edinburgh, Session* 1866-67.

NOTES ON POWDERED CASTILE SOAP.

BY JOSEPH P. REMINGTON, BROOKLYN, N.Y.

The following notes are contributed to the present fund of knowledge on the subject of drug powdering:—

Exp. 1. 986 avoirdupois ounces of white Castile soap (Conti) were shaved into thin slices, by means of a common cabbage-cutter, then spread on shallow trays, and exposed to the air in a drying-room, temp. 84° F., for one week, transferred then to a drying-room, temp. 125°, and left there three weeks, at the end of which time it weighed 724 ounces avoir., thus losing 26·57 per cent. of water; it was then powdered in the ordinary chaser or Chilian mill, and lost 7 ounces more in the process of pulverising, making the total loss 27·28 per cent.

Exp. 2. 960 ounces avoirdupois of the floating variety of white Castile soap, after standing in a moderately dry room for fifteen months (losing 224 ounces, or 23·3 per cent.), on being further dried and powdered, in a similar manner, lost 56 ounces more, making a total loss of 280 ounces, or 29·16 per cent.

Exp. 3. 1,112 ounces avoirdupois of mottled Castile soap (commercial) treated precisely in the same way, lost 320 ounces, or 28·8 per cent. The average loss on five previous lots was 21 per cent., the amount of water present varying in each case, losing respectively 20, 11½, 18, 27 and 29 per cent. The lots which lost 11½ and 18 per cent. had undoubtedly been kept some time, and in the case of the lot losing 11½ per cent., half of the water which would help to form the ordinary loss was lost before it was sent to powder. The first and last experiments were made with soap recently imported. It will be noticed that in these experiments (which I may state were carefully conducted) the mottled Castile soap does not support its reputation for strength (its only credited merit over the white). According to the U. S. Dispensatory, good mottled Castile soap should not contain more than 14 per cent. of water; this contained, then, double the proper amount. The same authority states that white Castile soap should not have more than 21 per cent.; in both experiments with the white it was found to contain an excess of at least 6 per cent. of water.

There is an important reason for not putting the soap in the room of higher temperature at first, for it would then melt, instead of drying properly, and become unmanageable. The powder from the first experiment was fine, light, and very white; that from the second was not so white, owing, probably, to the fact of its being kept a greater length of time and the presence of a little sesquioxide of iron, the colour of the iron being masked when the soap was fresh, as in the first experiment, the iron being then in the state of protoxide; 21 cents per lb. was paid for the white soap (in Exp. 1) which was a low figure (Feb., 1867), and as the loss was 27·28 per cent., allowing 12 cents per lb. for powdering, the lowest cost of the powder would be 38 cents, and yet a powdered white Castile can be *bought* for that price; the inference regarding the character of such a powder is of course unmistakable. It would be an easy task for the pharmacist to prepare his own powdered soap, the only necessary outlay would be for that unscientific instrument, the cabbage-cutter, which, however, could be used with advantage in making tinct. sapon, camph., opodeldoc, soap plaster, etc., not to speak of its legitimate use; for small operations the soap could be shaved with a spatula. If a drying-room is not at command, the difficulty of drying it thoroughly may be overcome by shaving the soap very thin, then spreading on paper and setting in a warm place; the drying of course is hastened by a current of air. After it becomes dry and friable, it can be easily powdered in a mortar and sifted through a fine sieve, and the pharmacist has then the satisfaction of saving the difference in the cost of powdering and of furthering the cause of "Medicinæ Puritas."—*American Journal of Pharmacy.*

THE WATER SUPPLY OF CALCUTTA.[*]

The question of the water supply is now being agitated in Calcutta, and the first of these papers has been drawn up by Mr. Spencer, by order of the municipal justices.

The matter treated of in the report is taken under the following heads—subsidence, mechanical filtration, purification, nature of the purifying material. The Hoogly water, it appears, contains a large quantity of suspended matter, the greater part of which is very fine mud. The author thinks it would not be safe to estimate the amount of sedimentary matter at less than a cubic inch to a cubic foot of water. He therefore strongly insists upon the necessity of large reservoirs for the subsidence to take place in, so as to give the filter beds as little mechanical work as possible. There are some differences from the ordinary methods, and apparently advantages in the one recommended for the construction of the filter beds.

The water enters the beds considerably like a spring. It is supplied from a basin in the centre, and spreads gradually over the surface. The author says: "The water descends through the sand, not in straight, but in a series of conoidal lines, intersecting each other, and which converge below at the regulating apparatus, where the water is bent upwards, as in a spring, before it reaches the lateral drains at the bottom of the bed." The purifying material is magnetic oxide of iron impregnated with carbon; it is mixed with sand, and fragments of some coarser material are added to act as retarding media.

It is necessary that the magnetic oxide should be porous, and for this reason the native mineral is not suitable. The author explains that a compact crystalline ore cannot be as effective as the same material when porous; few would be inclined to dispute the fact. In the preparation of the artificial magnetic oxide to be used, either spathic iron ore or hæmatite is heated until the carbonic acid or oxygen is expelled.

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[*] "Report on the Purification of the Hoogly Water for the Supply of Calcutta," by Thomas Spencer, F.C.S.
"Experimental Investigations connected with the Supply of Water from the Hoogly to Calcutta," by David Waldie, F.C.S.

The heat required in the latter case must make it a rather expensive operation. The porous material remaining is impregnated with carbon.

The use of this purifying material, to which the name carbide has been given, is to destroy the organic matter in the water. The ordinary process of atmospheric oxidation proceeds more rapidly, we are told, by a magnetic attraction for oxygen, which the carbide possesses.

"The purifying operation which takes place in contact with the carbide, may be explained by stating, that the atmospheric oxygen in the water is attracted into the pores or cells of this substance, in virtue of its magnetic nature—and because oxygen itself is also a magnetic body. The consequence is, that this purifying gas, like all other magnetic bodies, becomes polarised in the presence of a similar body, by which a complete change of property ensues. In a word, from a state of comparative inertness, which does not allow it to combine very readily with organic matter, this gaseous body suddenly becomes a most active agent of natural purification, viz., ozone."

The author's theory is ingenious, but like many beautiful things it is, we are afraid, fragile.

It is mentioned in the report that large purifying works have been constructed at Wakefield in Yorkshire, upon the plan recommended, and have proved very successful. Still it is a curious fact that so little has been heard of this process during the seven years in which the author says he has had practical experience with it. In remarking this we have no wish to disparage Mr. Spencer's process: on the contrary, if the purifying material possess the virtues accorded to it by the discoverer, it is especially our duty to protest against anything less than the universal application of the process in this country. The destruction of the organic matter contained in water used for drinking purposes is a question of immense importance. Unfortunately, results obtained with any method by its discoverer, are apt to be viewed sceptically. There are many who believe such bodies as carbon, and magnetic oxide of iron, to act by the oxygen which has been condensed in their pores when dry, and that after being in contact with water for a time, the action becomes feebler, and finally disappears. We are not aware that any experiments proving the contrary in the case of the magnetic oxide of iron have ever been published if made. The subject is too important to allow of the passing over of facts, as if proved, without such has been shown to be the case by the most rigid tests. A few lines further on a quotation is inserted, containing an assumption which it is hardly possible to grant.

An examination of the Hoogly water suggested to the author the presence of decomposing animal matter in the water. Describing the odour emitted, he says:—"It was that peculiar odour which unmistakably betokens those gases which are generated by decomposing animal matters." Shortly afterwards this statement occurs, in reference to the water passed through a carbide filter. "Subsequent analysis showed that it contained (0·60) a very little over half a grain of organic matter to the gallon—a quantity which is practically harmless, if composed of vegetable matter." Has the author any reason to suppose that the decomposing animal matter would be oxidised before the vegetable matter? Most chemists would oppose such a supposition, but the author makes use of it as a *quasi* fact.

The amount of organic matter in the water of the River Hoogly, after separation of sedimentary matter, is given, as well as that of the other constituents, in an addendum: organic matter and loss together equal 1·96 grains to a gallon. This amount includes certain small amounts of phosphoric and nitric acids, which were detected, but were too small to bear quantitative estimation. In the present scheme there would be 8 filter beds, and 5,000 tons of carbide is the amount estimated as necessary for the proper purification of the water which would pass through.

Mr. Waldie has been engaged for some time in making a minute examination of the constituents of both the water and mud of the Hoogly, believing the research would possess some scientific interest. The investigation, he says, is far from completion, but as the subject is now attracting considerable attention in Calcutta, he publishes some of the results obtained—those bearing on the suitability of the river as a source of water supply for domestic purposes. This water has been found to be superior to most of the other waters supplied to the town. These other waters are the tank waters, which the author found to be harder, and to contain much more organic matter—two, three, or four times as much. The Hoogly water is rather hard during the dry season, but by boiling the hardness is reduced to a very small amount. As regards organic matter, the river water is better than any of the London waters. In January and November it in no case exceeded 1·05 grains per gallon. During flood tide the organic matter is about twice as much as during ebb tides. Yet the highest amount obtained was rather less than two grains per gallon. The quality of the organic matter does not seem very satisfactory. The author says that when partially separated from saline matter, the organic matter in the Hoogly water, during the rainy season, resembled, in its general properties, animal excrementitious matter; while in the dry season it more resembled urinous secretions. Certainly, water contaminated in this way must be very efficiently purified to make drinking it a harmless experiment. The soil of Calcutta is more or less penetrated by sewage water, all over the town; for the author found that the water from temporary wells, dug for the purpose, was "simply sewage water deprived of the greater part of its bad smell by passing through the earth."

Mr. Waldie considers the permanganate of potash a doubtful means of estimating the organic matter in water; the oxidation test appears to indicate only certain kinds of impurities. According to this test General's tank-water (considered the best for drinking in Calcutta) contained as much organic impurity as the water of the salt marsh to the east of the town. The water of the circular canal, too, which receives the greater portion of the sewage of Calcutta, required no more oxygen than that of the best tanks. He believes the determination by weight to be the most trustworthy method. In summing up, the author says—As regards the inorganic constituents, the water of the Hoogly is at least as good as any supplied to London. The organic impurity seems to be worst during the rainy season; the water is purest in this respect in the cold season, and it is doubtful whether during the very hot season the organic matter equals that during the rainy season.

THE ATMOSPHERE OF THE METROPOLITAN RAILWAY.

In this part of our paper we give a full report of the inquiry relating to the air of the Metropolitan

Railway. In the interests of the public the results of the scientific evidence here brought forward should not pass without comment. Considering the mode in which gas analyses generally, especially those of air, are performed, the work done by the chemical referees deserves high praise; but the inferences which the public are likely to draw from the condensed reports hitherto published are certainly not justified by a careful analysis of the whole of the evidence. For the sake of clearness we will here give a summary of the details. In the tunnels Professor Rodgers found—

1 of sulphurous acid in	40,789 vols.
" " "	23,913 "
13 of carbonic acid in	10,000 "
18·7 " "	10,000 "

The average amount of oxygen found was 20·17 per cent., in one instance sinking as low as 18·7 per cent.

At Box tunnel there were found to be 20·3 per cent. of oxygen; at Blackheath tunnel, 20·0; at the Crystal Palace tunnel, 19·7; at Birkenhead tunnel, 20·1 per cent.; at Wolverhampton tunnel, 20·3 per cent. These figures are contrasted with Pimlico air, which contains 20·9 per cent.

Such results are very remarkable, and we confess we were not prepared for such a deficiency of oxygen in tunnels. The actual loss of oxygen is known not to be of great importance in itself, but as an indication of impurity it is one of the most delicate tests. When the oxygen is removed in small quantities by simple absorbents, it is not missed, but when it is replaced by the ordinary products of combustion and of respiration, the minutest portions make the deterioration of air evident to the senses. The deficiency here, according to Professor Rodgers, is as much as 20·9—20·17, or 0·73 per cent.

The amount of oxygen found in the bowels of the deepest mines of Great Britain was found in an average of several hundred cases to be 20·26, or more than in the Underground Railway, whilst the air of a large city has been found to contain 20·947. Now we can scarcely look on the air of London as worse than that, and we may safely say that if the air of Pimlico had been taken more frequently, a higher average than 20·9 would have been attained, although we agree with Professor Rodgers that 20·9 will be the amount at some times and places. Without being too minute, we have therefore to record the existence in the Underground Railway of an atmosphere more hurtful than that of the average of metalliferous mines.

Besides this, sulphurous acid, one of the evils absent from the mines, is found in the Underground Railway. One volume in 23,000 we consider very high, if we accept, on Dr. Letheby's authority, that 1 in 100,000 would create coughing.

As to carbonic acid, the amount in London streets is less than 4 in 10,000. 18·7 is high for the Underground Railway, although Dr. Letheby is right in saying that crowded rooms contain sometimes more than this.

If we look at the analyses of Drs. Letheby, Bachhoffner, and Whitmore, we observe results different from those of Professor Rodgers. They do not give the oxygen, but say that sulphurous acid to the amount of 1 in 100,000 could not be detected. The carbonic acid found was about 5, although it rose in one case to 9, and in another to 12·7 in 10,000. It is, moreover, worthy of remark that only where the carbonic acid is low are the maximum and minimum shown. In all cases where the quantity exceeds 5·5 parts in 10,000, the air was taken when it was likely to be foulest (4 p.m.), and the *mean* only is given. An explanation why the *maximum* was not given in these cases is certainly due to the public. The oxygen is said not to be deficient, but results are wanting.

We have here two views of the case not agreeing very well with each other, although not in all points contradictory. Both, however, agree in drawing conclusions favourable to the air of the railway. For ourselves we are unable to draw any such satisfactory conclusions; the analyses are not clear. Are we to look on the average air as containing only five or six parts of carbonic acid per 10,000, or are we to consider that it has lost 7 or 8 tenths of a per cent. of oxygen, which would indicate a larger amount of carbonic acid? Or, again, are we to believe that there is too little sulphurous acid to do injury, or 5 times more than is needful to make us cough? All these questions still remain unanswered, and yet the Companies are believed by many to have made an excellent case.

If there is a loss of 0·73 per cent. of oxygen, where has it gone? We do not find it accounted for either in the amount of carbonic acid or sulphurous acid.

We do not say that this proves the analyses to be incorrect, because the air might have been collected separately for the two analyses, but it certainly proves insufficiency of evidence. The public ought not to be troubled with these discrepancies; the results ought to be laid before them clear and unquestionable: less than this implies imperfect work and deficient time. Chemical referees cannot fail to come to identical results if they work well and long enough.

We can see little good in partially corroborating evidence, or in opposing it with contradiction enough to admit of doubt. The question is not pushed to the utmost, and the public will think exactly as before. And what does it think? Why, that the air is really very bad, and that if chemists have failed to prove it so, so much the worse for the chemists, but the air is no better for all that. They think too that they care little for averages, because a traveller by the Underground Railway does not breathe there a mere average, but at times a real blast from the chimney, in an atmosphere already vitiated. What is the amount of carbonic or of sulphurous acid in that blast? That depends on the coal, and is easily calculated. They will believe, too, that although the air may not be capable of killing a strong man, it is less capable of supporting vitality than the air outside, and it is deadly to those who cannot bear so much impurity, exactly as the air even of London is to those who are so constituted that they must live in the country. To take a delicate organism as an instance; will anyone say that a lark could live in such air as was found? We cannot name the point at which the quality "deadly" begins; it is a matter of degree. There is no doubt that the air is of an inferior description, how bad we do not exactly know; but if it were really desired to be ascertained, numerous experiments should be made, and when there was a diversity of opinion we would have the matter investigated till every point was clear. It does not amount to a scientific inquiry when two sides throw down the dice and shout immediately who has won. The first experiments are generally useful merely to show the difficulties. Time and money are spent in obtaining scientific reports, and again more time and money in obtaining reports to contradict them; but the next step, namely, insisting that they shall agree, is too seldom taken. In this case they do agree in conclusions, we think without sufficient reason.

Perhaps in this case some of the weakness arises from a desire to protect the Company. It would be unwise to blame that body as if its conduct were reprehensible. The Directors must desire pure air more even than the public desires it, as it would increase both the comfort of the public and their profits. It shows great weakness on the part of the Company to appear at all afraid, or to seek to defend itself. It is enough if the Directors, have done their best, and are ready to improve as soon as a method is shown them. The railway is too valuable to be without defenders, and the appointment of persons by the Company to conduct the examination for them weakens their case in the eyes of the public. However honourable these men are, the public will say—"The Directors, after blowing smoke into our nostrils, attempt to throw dust into our eyes." It would be far better for them to make no defence at all, but improve the air of the tunnels to their utmost, never resting satisfied until the public ceases to complain.

An adjourned inquiry* before Dr. Lankester, coroner for the central division of Middlesex, relative to the death of Elizabeth Stainsby, who died suddenly at the King's Cross station of the Metropolitan Railway on the evening of the 28th of August, was concluded on the 30th of October.

In consequence of a suggestion at a former sitting that the impurity of the air in the tunnels of the line might have accelerated the death of the person in question, the Coroner directed a scientific examination of the air of the various tunnels. The result of that investigation was now laid before the jury.

Mr. Hawkins, Q.C., and Mr. Myles Fenton, General Manager of the Metropolitan Railway, attended on behalf of the Company.

The Coroner reminded the jury that, according to the evidence of Dr. Popham, the young woman, whose death they were investigating, had disease of the heart; but although a person suffering from that disease might be expected to die suddenly even under ordinary circumstances, the jury might very properly inquire whether any circumstance or condition to which the deceased had been exposed had accelerated death; and in the present instance it was more especially their duty to ascertain whether the condition of the atmosphere in the underground tunnels had had the effect of bringing about the deceased's death sooner than it would have otherwise occurred. Dr. Popham, who had made a *post-mortem* examination, said in his evidence that he did not know sufficient of the nature of the atmosphere of the line to be able to say whether or not it had hastened death in this case. Although it had been said by some persons that the atmosphere of the railway really produced no other effect than the external air, there seemed to be a general feeling at the former sittings that the air of the tunnels was different in its quality from the air in the open street; and the question arose whether that difference was due to anything which would hasten the death of any person. For this reason it was determined that an analysis of the air should be made, and he had requested Professor Rodgers to make such an analysis. The railway authorities had been very anxious to get at the truth of the matter, and they had themselves employed some scientific gentlemen to make a similar investigation. He (the Coroner) must say that he regretted that in such cases as the present the Coroner had not a competent official assessor to instruct and assist him, for when scientific men were employed by the parties interested in these cases it often happened that they were supposed to advocate the interests of the side which had sought their services.

At the request of Professor Rodgers, the Coroner read his notes of Dr. Popham's evidence as to the *post-mortem* appearances.

Mr. J. E. D. Rodgers was then sworn and examined. He said: I am lecturer on medical jurisprudence and toxicology at the London Hospital Medical College, and I lectured many years on Chemistry at the old St. George's School of Medicine. I am also a member of the Royal College of Surgeons, and Licentiate of Apothecaries' Hall. I have often been employed to give evidence at coroners' courts,—as much, I believe, as any toxicologist in England. I was especially so employed by Mr. Wakley. I hold the coroner's warrant for examining the air of the Underground Railway. I am acquainted with the fact that the illness of the deceased commenced at the Bishop's Road station. Dr. Popham has informed me that her stomach was full of food, and I have also been informed that the dress was remarkably tight round the waist.

The **Coroner**: We have had no evidence of that fact. Who undressed the woman?

Dr. **Popham**: I was present. The dress was certainly very tight—more than ordinarily so. I think it was I who cut the stay-laces. They compressed the lungs and chest a good deal. She had no belt, but she wore two dresses.

Professor **Rodgers**: I have carefully analysed and tested the air contained in the tunnels of the Underground Railway between the Bishop's Road and King's Cross stations. I have done this on four occasions—namely, September 4th, September 10th, October 2nd, and October 28th. With a view to compare the chemical constitution of the air of these tunnels with that of others, I have also subjected to experiments air taken from the Blackheath tunnel, the Gipsy Hill tunnel on the Crystal Palace line, the tunnel between King's Cross and Finchley on the Great Northern line, and the Box tunnel, the Wolverhampton tunnel, and the Birkenhead tunnel, on the Great Western Railway. I have also examined and tested the air of London in as pure a condition as I could obtain it, in order to ascertain the amount of deterioration that that of the tunnels had undergone. The atmosphere in a pure condition consists by volume of 79·19 measures of nitrogen, and 20·81 of oxygen, and every 10,000 measures of air contain from 3·7 to 6·2 measures of carbonic acid. On September 4th I visited the Metropolitan Railway between the hours of 3 and 5 p.m., and tested the general nature of the air, and collected certain portions for subsequent examination. In 17 cubic inches of air taken from each of the five tunnels between Bishop's Road and King's Cross, and tested for carbonic acid (with the exception of the air from the Gower Street and King's Cross tunnel, which contained a more notable quantity), only a slight trace of carbonic acid was indicated. The percentage of oxygen in the samples then taken was—

In the Bishop's Road tunnel	20·48
" Edgware Road "	20·6
" Baker Street "	20·3
" Portland Road "	20·1

In the air of the tunnel between Gower Street and

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* Specially reported for the Chemical News.

King's Cross, an opportunity having been watched to get some of the worst air, I found that the percentage of oxygen was 18·7. With this exception the air of the tunnels was not far below the external atmosphere as far as its oxygen was concerned. In travelling backwards and forwards the atmosphere between Gower Street and Portland Road was occasionally unpleasant, but still I felt no faintness or exhaustion from it. On my next visit, September 10th, between the hours of 10 and 11 p.m., I made an arrangement by which I could determine the quantity of carbonic acid and sulphurous acid contained in 775 cubic inches of the air taken from the tunnels during my transit backwards and forwards from King's Cross and Bishop's Road. The result was that the average sample of air contained 13 measures of carbonic acid in 10,000, and one measure of sulphurous acid in 40,789—nearly 41,000. On this occasion also the air taken from the Gower Street and King's Cross tunnel gave clear evidence of the presence of carbonic acid in 17 cubic inches. On my next visit, October 2nd, my object was to ascertain the quantity of carbonic and sulphurous acids in the air of the Gower Street and King's Cross tunnel only, and I found 18·7 measures of carbonic acid in 10,000, and one measure of sulphurous acid in 23,913.

The Coroner: You were getting worse then?

Witness: Rather worse. Then I had the air of that one tunnel in its worst possible condition. On my last visit, October 28th, I tested the air in all the tunnels between the hours of 8 and 9 p.m., and found traces of carbonic acid evident in the 17 cubic inches, the air of Gower Street, on this occasion, containing the minimum, and that of Portland Road the maximum. I brought away samples for the purpose of ascertaining the percentage of oxygen, and the results of my analysis I will now give you.

Percentage of Oxygen on October 28.

In Bishop's Road tunnel	20·5
" Edgware Road "	20·2
" Baker Street "	20·5
" Portland Road "	20·0
" Gower Street "	20·4

On October 2nd I found in the Gower Street tunnel the percentage of oxygen was 20·1.

The witness then submitted the following results of observations, which, for the sake of comparison, he had made on the air of different railways:—

Percentage of Oxygen in Air.

Blackheath tunnel (Sept. 28th)	20·0
Crystal Palace tunnel (Oct. 2nd), taken in the midst of traffic	19·7
King's Cross and Finchley tunnel	20·5
Box tunnel	20·3
Birkenhead Tunnel	20·1
Wolverhampton tunnel	20·3
Open air of Pimlico (Sept. 21), going from Battersea Park by the S. W. Railway	20·9

The Coroner: Is that the general average?

Witness: To get the exact determination of oxygen in the air is a matter of considerable difficulty. It would take perhaps many months to determine it exactly.

The Coroner: But that is in accordance with general analyses?

Witness: It is. My object in making these analyses was fairly to contrast the air of the metropolitan tunnels with that of other situations and other tunnels. The highest point of carbonic acid which I found is 18·7 measures in 10,000. That occurred in the Gower Street and King's Cross tunnel. That is the worst quantity on that line. I must here observe that the quantity of carbonic acid in different airs has been determined by other observers. Dr. Roscoe found that in the atmosphere of a crowded theatre four feet above the stage there was no less than 23·37 measures of carbonic acid in the 10,000; and going a little higher (34 feet from the stage) he found 32·12 measures in the 10,000. In the Wellington Barracks it was determined that 2 feet 6 inches from the floor there were 12·42 measures of carbonic acid in the 10,000.

The Coroner: Have you found any fluctuation with regard to the sulphurous acid?

Witness: No, I have not.

The Coroner: Do you think the deficiency of oxygen likely to act injuriously upon the health of delicate persons?

Witness: Only if they continued there for some time. Not if they passed rapidly, as they do in the trains.

The Coroner: After all, the deficiency of oxygen appears to be very slight?

Witness: It is a very slight deficiency.

The Coroner: Except in that one case of Gower Street, where there was only 18 per cent.

Witness: At that time there had been trains rapidly passing, and there was one passing at the time I took the air.

The Coroner: You do not think, then, that the deficiency of oxygen would hasten the death of a person who had already constricted disease of the aorta?

Witness: I would answer that question in this way, Mr. Coroner. She has diseased heart; she has eaten considerably; she is tightly laced; she begins to be faint at a station where the air is unexceptionable—that is, at Bishop's Road. I do not think that under those circumstances the deficiency of oxygen hastened her death.

The Coroner: I will just say to the jury (I do not know whether they understand it) that the deficiency of oxygen is one point to be considered. But here are also two other things,—a redundancy of carbonic acid, and also the presence of a gas, which, I suppose, does not exist in the atmosphere at all, namely, sulphurous acid. [To the witness.] With regard to the carbonic acid, do you think that that gas would be likely to act injuriously upon a person under those circumstances?

Witness: Not in the quantity found. It must be borne in mind that under an attack of that kind the respiration would be more feeble, and consequently less air would be taken in, and it would be more slowly taken in.

The Coroner: Then there is the sulphurous acid. Would that be likely to produce any injurious effect?

Witness: No, not in that quantity. The proportion would not be so great as if an ordinary sulphur-match containing a few grains of sulphur were burned in this room.

Q. Did you perceive the odour of sulphurous acid gas there?—*A.* In the Portland Road and the Gower Street tunnels, and occasionally on the passing of some of the trains.

Q. What was the highest quantity that you found of sulphurous acid gas?—*A.* In the Gower Street tunnel, 1 in 23,000.

Q. And you do not think that would be more than would be given by a match lighted in this room?—*A.* That would be rather more.

Q. There are about 3,000 cubic feet in this room. Now, if found in that quantity, would the sulphurous acid produce any feeling of depression upon persons subjected to its effects?—*A.* I think not.

Q. Then when persons feel this gas so oppressive, do you think that arises from any peculiarity or idiosyncrasy in them, or is there more gas at those times?—*A.* It might arise from more gas at those times and the heat.

Q. You think there may be more sulphurous acid gas at one time than another?—*A.* Certainly.

Q. Supposing, then, there is less oxygen, more carbonic acid, and more sulphurous acid, at one time than another, do you think there is sufficient ventilation to prevent dangerous results?—*A.* I think that during my last two visits, there was unquestionably sufficient ventilation to prevent such an accumulation of those gases as would be injurious to the public health.

Q. You would not mind going up and down any number of times a day?—*A.* No.

Q. Have you seen any of the servants of the railway at all, and questioned them as to the effect of the air upon them?—*A.* Yes. They made no complaint.

Q. They all knew you were examining for the Coroner's court?—*A.* They did: and I would here mention that I have had every facility given to me so that the examination should be perfect.

Q. Would you mind staying all day in one of those tunnels if you were handsomely paid for it? (Laughter) *A.* Oh, that becomes another question.

The Coroner: I really want to know what is your opinion with regard to the effect of the air upon health. There is a very decided feeling that persons suffer from it. I have had a high pile of letters about it, and amongst other things it is stated that servants of the Company have suffered and gone away. I want to get your opinion as to whether they would be likely to suffer, and to test in your own person whether you would like to stay in those tunnels all day long for a handsome sum.

Witness: My opinion of the air is that it is not an air that you would choose for a dwelling, to be constantly residing in. But I speak of the line simply as a tunnel for travelling; and, comparing it with other tunnels and crowded places, I think that the air of the Metropolitan Railway tunnels will now bear comparison with any. It is not an air that ought to be chosen as one to dwell in constantly.

Mr. Hawkins: You would not take the air of this room to dwell in?

Witness: Certainly not.

The Coroner: Can you tell the jury whether it is known that sulphurous acid has an injurious effect upon health?

Witness: Long continued exposure to it unquestionably has. I recollect the experiments of Dr. Turner and another on the influence of sulphurous acid on plants. The mixture in that case was much stronger, being one part of sulphurous acid in 5,000. That atmosphere killed plants. They withered in twenty-four hours. That points to the injurious effect of burning gas in greenhouses. I do not think that animals are so susceptible as vegetables to the effects of sulphurous acid. I do not think that in the hot summer weather, when there is less interchange of air between the tunnels and the outside atmosphere, there would be more carbonic and sulphurous acids and less oxygen than in the months when I examined the tunnels. It seemed to me that the trains passing backwards and forwards ventilated very completely and caused a change of air. I do not think that the tunnels would be more oppressive in hot than in cold weather, because the temperature of the air in the tunnels is rather lower than that of the external air in the summer, and rather higher in the winter. There would always be a difference. When I made my experiments the temperature of the air was 72 outside the tunnels and 69 inside. I cannot say whether there had been any removal of the glass or the windows at Baker Street and Gower Street stations before I took the first atmosphere, which was on the 4th of September. Unquestionably the removal of that glass would make a difference in the ventilation.

A Juror: I should like to know whether the tests were made in a compartment similar to that in which the deceased died.

Witness: I do not know in what class she travelled. For the purpose of convenience I travelled in a third class carriage.

At the request of the Coroner, the witness exhibited and explained the apparatus which he used for the purpose of testing the air. It consisted of three small flasks connected by tubes and communicating with a large bottle of water to which a syphon was attached. The flasks contained baryta in solution; and the tubes were so arranged that when water was drawn off by the syphon from the large bottle, a corresponding bulk of the external air would enter at the flask most remote from the syphon, and pass consecutively through the two other flasks and ultimately into the large bottle, there taking the place of the water drawn off by the syphon. The bulk of the water so discharged was equal to that of the air passing into the flasks. The bottle was of known capacity, and thus the experimenter knew how many cubic inches of air he operated on.

Mrs. Annie Pembridge, who was in the railway carriage at the time the deceased was taken ill, deposed that she did not feel the air of the tunnels oppressive on that evening, and she heard no one complain of its being so. The carriage was a third class. Witness entered the carriage at Portland Road. The deceased was already there. Witness did not hear the deceased say, "What a stink!"

The Coroner asked if any stranger in court wished to give evidence as to the condition of the tunnel atmosphere, but no person came forward for the purpose.

Drs. George Henry Bachhoffner, Henry Letheby, and John Whitmore were then sworn. They presented in evidence a printed copy of a joint report of an examination of the air of the tunnels made by them at the request of the Directors of the railway.

The report was read by Mr. Hawkins, and was as follows:—

Report of an Examination of the Air in the Tunnels of the Metropolitan Railway.

Having received instructions from the Directors of the Metropolitan Railway Company, through Messrs. Burchell, their solicitors, by letter addressed to Dr. Bachhoffner, to examine and report the state of the atmosphere in the different tunnels on their line, and, on the sanitary condition generally of the stations and tunnels, we beg to present the following as the result of our investigations:—

We proceeded in the first instance to obtain samples of the air in the tunnels, and we collected them on three separate occasions, namely:—First, immediately after the trains had ceased running at night; secondly, just before they commenced running in the morning; and, thirdly, in the afternoon between 4 and 5 o'clock, the period of the day when there is generally the largest amount of traffic.

The samples, twenty-eight in number, were taken at different places in each tunnel, and at different altitudes; some near the crown of the arch, some near the ground, and others on a level with the heads of the passengers. These samples were analysed for sulphurous acid, carbonic acid, carbonic oxide, coal gas, and oxygen.

The presence of sulphurous acid was sought for by the most delicate chemical test with which we are acquainted; namely, its action upon iodic acid and starch, which we have ascertained is capable of showing the presence of one part by volume of sulphurous acid in 100,000 parts of air, but we could not in any case discover by such test the presence of this acid; from which we conclude that its volume was less than the above in the tunnels. The proportion of carbonic acid by volume in 10,000 parts of the air in the several tunnels and stations was as follows:—

		Max.	Min.	Mean.
Tunnel between Bishop's Road and Edgware Road	2 a.m. Sept. 3	4·1	4·1	4·1
Tunnel between Edgware Road and Baker Street	1 to 3 a.m. September 3	5·2	4·3	4·8
	2 to 4 a.m. September 6	5·4	4·7	5·0
	4 p.m. September 7	—	—	5·7
Baker Street Station	4 p.m. September 10	—	—	6·2
Tunnel between Baker Street and Portland Road	1 to 3 a.m. September 3	6·0	4·6	5·1
	2 to 4 a.m. September 6	4·5	4·2	4·4
	4 p.m. September 7	—	—	6·9
Tunnel between Portland Road and Gower Street	1 to 3 a.m. September 3	6·0	5·1	5·5
	2 to 4 a.m. September 6	6·1	4·5	5·1
	4 p.m. September 7	—	—	12·7
Gower Street Station	4 p.m. September 7	—	—	5·7
Tunnel between Gower Street and King's Court	1 to 3 a.m. September 3	5·4	4·4	4·9
	2 to 4 a.m. September 6	5·2	4·3	4·6
	4 p.m. September 7	—	—	9·1

The amounts of carbo-hydrogen (coal gas) and of carbonic oxide present were so small as to be barely discoverable by the most delicate processes of analysis. Lastly, we ascertained that the amount of oxygen in the air of the tunnels and stations was not in any case deficient.

These results prove that in no instance was the air found to be vitiated to any material extent, although it will be seen that the air taken in the afternoon was less pure than that taken at night. The researches of Regnault, Bunsen, and other eminent chemists, and more recently those of Dr. Angus Smith, show that what may be termed "model or normal atmospheric air" in cities and large towns consists in every 10,000 parts by volume of

Oxygen	2,096
Nitrogen	7,900
Carbonic acid	4
	10,000

It is the last constituent which when in excess renders the air impure, and, in proportion to its increase, so is the air made unfit for respiration. Experiments conducted by Dr. Bernays and Dr. Angus Smith have shown that in several of our London theatres at about 10 o'clock p.m., in many other places of public resort, and especially in some of our law courts, the quantity of carbonic acid in the atmosphere of those places varied from 10 to 32 parts per 10,000; and from the Army Report (vol. v. page 272) it appears that in some fairly-ventilated barracks at Aldershot the quantity of carbonic acid at midnight amounted to 6·42 per 10,000 of air, and at 5 p.m. it amounted to 7·59 per 10,000; and in Wellington Barracks, from 11·89 to 14·18. Even in the streets of Manchester, in foggy weather, it has amounted to 8 parts per 10,000 of air.

In order to determine the atmospheric conditions of these tunnels by comparison with the condition of the air in the tunnels of other lines of railway, we took samples of the air from several tunnels near London; and from these, which we designate by numbers only, we obtained the subjoined results:—

Tunnel.
No. 1—4·7 Carbonic acid per 10,000 of air by volume.
" 2—12·1 " " "
" 3—4·6 " " "
" 4—4·3 " " "
" 5—7·8 " " "
" 6—4·5 " " "
" 7—5·3 " " "
" 8—4·3 " " "
" 9—4·2 " " "
" 10—5·1 " " "
" 11—4·3 " " "
" 12—4·2 " " "
" 13—4·6 " " "

Our inquiries were next directed to the quality and quantity of the fuel used in the engines, and to the mode by which its combustion is effected. The plan adopted (with which we cordially agree) is to diminish as far as practicable the combustion of the fuel during the passage of the trains through the tunnels and stations. The steam in the boiler is raised in the open air to a temperature and pressure which, by experience and daily practice, is found sufficient to work the trains through the tunnels; and, when the trains come again into open space, fresh steam is then generated sufficient to propel the trains through the next journey, when the process is again repeated; by which means the engine driver is enabled, when passing through the tunnels and stations, to close the fire-box and damper so as merely to keep the fire in such a condition that it may be easily revived at either end of the journey.

The evolution of the products of combustion is thus almost entirely confined to that portion of the journey when the trains are passing through the open spaces.

The coke is of a superior quality, being made from a coal which is known to be more than usually free from iron pyrites, and it is burnt in the ovens for twenty-four hours longer than the ordinary coke generally used upon railways. In addition to which a staff of eight men and a foreman are constantly employed in examining and selecting the coke, so as to ensure that none but the best quality of coke is transmitted to London for the use of the Underground Railway.

To determine the percentage of sulphur in the coke, thirteen samples were submitted to chemical analysis, and these gave an average proportion of 0·26 per cent. of sulphur, which is about one-fourth the quantity found in ordinary coke. As regards the coke, therefore, we see nothing to which we can take exception, but, on the contrary, we are of opinion that the best available means are used for obtaining a fuel as free from deleterious matter as possible, in addition to which the combustion of the same is conducted with the view of preventing as far as possible the escape of offensive gases.

The presence of sulphur, or, more correctly speaking, of sulphurous-acid gas, in the tunnels and stations, which at times is appreciable both to taste and smell, more particularly on those days when the external atmosphere is unusually dense, must not be taken as an indication that this gas exists in dangerous quantities, for as little as one part of this gas in 100,000 parts of atmospheric air is strongly perceptible both to taste and smell; and paper moistened with a solution of iodic acid and starch becomes tinged with a blue colour when exposed for a few minutes to air having the above proportion of sulphurous acid. On several occasions we have exposed this delicate test to the air in the tunnels while passing through them, both in the carriages and on the engines; and although the quantity of air thus brought into contact with the test has been considerable, yet it has only been during the time of active traffic that the test has shown the presence of sulphurous acid, and then in an insignificant degree. In addition to the above, we beg to point out another cause which communicates to the air, more particularly in the stations, a pungent smell, which, although disagreeable, cannot in the slightest degree be regarded as injurious to health; we allude to the partial combustion of the wood forming the breaks when acting upon the tires of the wheels in checking the speed of the train as it approaches the stations.

The number of trips made by the trains through the tunnels daily amounts to 358, of which 284 are by the narrow-gauge trains, and 74 by the broad-gauge. Each of the narrow-gauge trains occupies 20,000 cubic feet of space, and those of the broad-gauge 23,000 cubic feet. The length of time occupied by each train in passing through the tunnels and stations is ten minutes. There are numerous openings communicating with the external atmosphere above, amounting in the aggregate to 3,164 square feet, and distributed in the following manner: namely, Baker Street station, 1,362 square feet; Portland Road station, 863 square feet; Gower Street station, 939 square feet. The western end of the tunnel at Edgware Road opens into a large area called the yard, and at the eastern end of the tunnel at King's Cross an opening has been made directly into the atmosphere, 40 feet in width, in addition. By an extensive series of thermometric observation, we find that there is an average difference of about 1·7° Fahrenheit between the temperature of tunnels and that of the external atmosphere; the mean outside temperature being 70° Fahrenheit, while the air in the tunnels had a mean temperature of 68·3° Fahrenheit, so that it was 1·7° Fahrenheit colder than the external atmosphere. During the winter months this condition will possibly be reversed; but in either case there will be a rapid change of air by an ascending and descending current. Having regard to the cubical volume of the trains, the short time occupied by them in passing through the tunnels and stations, the large volume of air which they displace, and the increased impetus given to the horizontal movement of the air by the rapidity of their transit, we are of opinion that the vitiation of the atmosphere cannot be of a serious character, and this accords with the results of our analysis.

A careful inspection of the tunnels has also shown that they are well constructed, and are generally dry and free from infiltration of liquid or other matter prejudicial to health, with the exception of a portion of the tunnel between Portland Road and Gower Street; to this we directed the attention of Mr. Fenton immediately after our first inspection; and we are happy to be able to add that the defect was at once attended to, and is now in a perfect sanitary condition.

We find on inquiry that the general health of the employés is such as to afford unquestionable proof of the sanitary condition of the air in the tunnels. From a statement furnished to us by Mr. Fenton, it appears that the percentage of sickness and mortality of these persons is considerably less than that of the employés on the Great Western Railway. To this fact we may add the results of our own personal inquiries, which fully confirm it, as many of the engine-drivers and guards have been in the service of the Company since the opening of the line. They are to all external appearance robust healthy men, and they have assured us that since they were first appointed they have scarcely had a day's illness.

(Signed) GEO. H. BACHHOFFNER, Ph.D., F.C.S., etc.
HY. LETHEBY, M.B., M.A., etc., Professor of Chemistry in the College of the London Hospital, and Medical Officer of Health for the City of London.
J. WHITMORE, M.D., etc., Medical Officer of Health and Chemical Examiner of Gas for the Parish of St. Marylebone.

Dr. **WHITMORE** stated, in answer to the Coroner, that the glass was not removed from the windows of the Baker Street station until after the first samples of air had been taken for examination. The numbers given in the report were the result of a large number of experiments made on the days stated.

Mr. **MYLES FENTON** stated that the glass from the Gower Street station was removed at the beginning of the summer, and long before the death in question. The removal was not due to any complaint he had received. Had had no complaint of the atmosphere officially previous to the death. At the opening of the line, when it was worked by the Great Western Railway Company, some of the porters were taken ill at the Portland Road station. The reason was that there was no special provision made for securing the most perfect coke, and the drivers were not so skilful in the working of their engines as they are now. When the Metropolitan Company took the working of the line into their own hands they took means to secure the best coke in the country. They believed they had obtained it, though at very great cost. The atmosphere of the line was as good as they could make it, and the proportion of sickness among their men was smaller than among the Great Western Company's men.

FREDERICK GIBBONS, an inspector on the Metropolitan Railway, stated that there had been no complaints of the air on the part of the men employed on the line since the Company had used their own engines and employed good coke. The train in which the deceased travelled was the last but two that night.

Mr. **HAWKINS** stated that twenty-five millions of people travelled by the line every year, and that seventy-two millions had travelled by it from the time of its opening.

The **CORONER** remarked that there was a slight discrepancy between the examination made by Mr. Rodgers and those made by Drs. Bachhoffner, Letheby, and Whitmore, with regard to the sulphurous acid present in the tunnel air. The latter gentlemen stated that they could not detect it at all.

Dr. **LETHEBY** said sulphurous acid could be readily detected when a large volume of the air was caused to play upon the test-paper; but when only 70 or 80 inches of air was collected, they in every instance failed to discover sulphurous acid. The nose, however, was more sensitive than any chemical test. One part of sulphurous acid in 100,000 of air would create coughing. Persons who had an irritation of the bronchial membrane and of the throat would be more susceptible of its effects than others.

The **Coroner** said that that would account for some persons complaining very constantly of the effect of sulphurous acid in the tunnels. The evidence on the whole had been very re-assuring to the public.

Dr. **Letheby** said that the smell of the wood-breaks, which became charred as the trains were being stopped, would be more evident to the passengers than the sulphurous acid. Probably the gas evolved at such times was what persons found most oppressive. It would be a carbo-hydrogen, and probably there would be a little acetic acid.

The **Coroner**: Would that carbo-hydrogen produce an irritating effect?

Dr. **Letheby**: Yes. When wood is submitted to heat it gives off vinegar and an empyreumatic oil which is very irritating.

The **Coroner**: It has an irritating effect upon the mucous membrane of the lungs?

Dr. **Letheby**: Yes.

The **Coroner**: Do you think that any person with congestion of the lungs, we will say an abnormal congestion from disease of the heart, would be likely to feel this?

Dr. **Letheby**: There is no doubt there is a difference in the sensibility of the nerve of smell. and also in the sensibility of the mucous membrane of the lungs; but although there may be differences in the recognition of it, I do not think that under any circumstances there would be sufficient to produce any dangerous effect.

The **Coroner**: The differences amongst persons in perceiving it will account for some persons telling me that they have been obliged to give up travelling by that line, in consequence of a sense of irritation after leaving the train.

Dr. **Letheby**: I have calculated the quantity of sulphurous acid which would be given off by the burning of the coke now used, and I find that if they were to shut the tunnels up from end to end, the burning of the coke for 18 hours would not produce enough sulphurous acid to be dangerous to the most susceptible persons. Sulphurous acid is used as a disinfectant in a much larger proportion than would be present under such circumstances. It enters into the composition of a disinfectant which is used over and over again, and I have never heard of any bad effect from it.

The **Coroner**: And you know there are cases in which persons live in an atmosphere pervaded by it, as at Luton, where they bleach straw by it, and at other places.

Dr. **Letheby**: Yes, and it is used also for bleaching blankets.

The **Coroner**: How much coke is consumed in the tunnels a day?

Dr. **Bachhoffner**: Two tons.

Dr. **Letheby**: The coke contains 0·261 per cent of sulphur. 6lbs. of coke are consumed per mile in the tunnels and stations. The length of the tunnels and stations is 11,056 feet, that is 2·09 miles. The cubic contents of the tunnels (without the stations) is 4,606,792 feet. The number of trains passing up and down in the day of 18 hours is 358. If you work that out it will come to this,—that supposing the whole space to be hermetically sealed, and that the whole of the sulphur is burned in a closed atmosphere, the quantity of sulphurous acid produced by the sulphur of the coke in that cubic space of air will amount to 2·95 parts in 100,000 by volume. The sulphur burned amounts to 11·717 lbs. in the 18 hours. That will produce nearly 136 feet of sulphurous acid. That going into a volume of 4,606,792 cubic feet of air gives 2·95 parts of sulphurous acid in 100,000 volumes of air.

The **Coroner**: That is supposing no change of air during the eighteen hours?

Dr. **Letheby**: Yes. Well, I say that quantity is quite incapable of doing mischief.

In reply to the **Coroner**, Dr. Letheby stated that the apparatus he used for ascertaining the proportion of carbonic acid in the air was a contrivance of Dr. Angus Smith, invented for use in mines and crowded places where chemical apparatus could not be conveniently used. It gave quantitative results immediately. The apparatus consisted of an india-rubber ball of known capacity, by means of which the air to be tested was blown into a known quantity of baryta water. Account was kept of the number of ballfulls of air required to produce turbidity in the water, and the proportion of carbonic acid was ascertained by reference to a calculated table accompanying the apparatus. The air of the inquest-room was found to contain 44·4 volumes of carbonic acid in 10,000.

The **Coroner**: Well, we are very much worse than the tunnel, then. (Laughter.)

Dr. **Letheby**: It is three times as bad as we have ever found it in the tunnel. (The witness then put in the subjoined table.)

Amounts of Carbonic Acid per 10,000 *of Air in different Places.*

1. *Cities and Towns*—				
London	from	2·8 to 4·3	Average	3·4
Manchester	"	4·9 to 15·0	"	5·4
Munich				5·0
Madrid	"	3·0 to 8·0	"	5·2
Paris	"	3·6 to 5·1	"	4·9
2. *Places of Public Resort*—				
Court of Chancery (doors closed)				19·8
" " (doors open)				4·8
Chamber of Deputies, Paris				25·0
Theatres (London)		7·6 to 32	"	14·9
" (Manchester)		10·2 to 27·3	"	14·8
" (Paris)		23 to 43	"	33·0
3. *Dwelling-houses by Day*—				
From		5·4 to 12·7	"	7·8
4. *Dwelling-houses by Night*—				
In a study near table				11·8
" " ceiling				15·6
Bed-room at night				28·0
" " window open				8·0
5. *Dormitories*—				
At Saltpetriere				80·0
Another at ditto				58·0
Workhouse Ward				125·0
Lodging-house in City				100·0
6. *Schools by day*—				
Various in France	"	27 to 47	"	36·0
" in Germany		20 to 56	"	39·2
" in England		9·7 to 31	"	21·5
7. *Mills and Workshops*—				
	"	28·3 to 30·0	"	29·1
8. *Barracks at night*—				
	"	11·9 to 14·2	"	12·8
10. *Cornish Mines*—				
Average of good				8·0
" of bad				190·9

11. *In Expired Breath*—350 to 500. 425
12. *In Room with Chafing Dish*— 1400

The **Coroner** asked Professor Rodgers whether he could account for finding so much more sulphurous acid than the other experimenters.

Mr. **Rodgers** replied that both he and a gentleman who accompanied him had noticed that some of the trains which passed them seemed to emit no sulphur at all. He had not the remotest doubt of the accuracy of Dr. Letheby's experiments, but he must say that he (Mr. Rodgers) should not have found sulphur if it had not been there. He first obtained sulphite of baryta by means of the sulphurous acid in the air, and he afterwards converted the sulphite into sulphate by means of chlorine. He then estimated the quantity, and finally applied nitro-prusside of sodium as a test to show the existence of the sulphur compound. The weight of the sulphite subtracted from the gross weight of the precipitate showed the weight of the carbonate.

The **Coroner**: Do you see any objection to that plan, Dr. Letheby?

Dr. **Letheby**: No; but there is a very small quantity to work upon.

Mr. **Rodgers**: The balance I used turns at the thousandth of a grain.

The **Coroner**: Would the quantity of sulphurous acid you found do any harm.

Mr. **Rodgers**: No.

The use of the iodic acid and starch test for sulphurous acid was then illustrated to the jury by Dr. Letheby. A decided blue tint was produced on the test paper by the fumes from a burning sulphur match.

The **Coroner** briefly summed up the evidence.

After a few minutes' consultation, the jury returned the following verdict: "That the deceased, Elizabeth Stainsby, on the 28th August was found dying, and did die, from the constricted disease of the aorta, and the jury say that the said death arose from natural causes."

ON THE

FORMATION OF SUCCINIC ACID FROM CHLORIDE OF ETHYLIDENE.

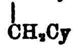
BY MAXWELL SIMPSON, M.D., F.R.S.

Some years ago* I ascertained that when bromide of ethylene is successively treated with cyanide of potassium and caustic potash ordinary succinic acid is formed. This reaction has since been confirmed by M. Geuther,† who, however, employed chloride instead of bromide of ethylene.

It occurred to me that it would be interesting to ascertain whether the chloride of ethylidene would, when subjected to the same treatment, produce the same or an isomeric acid. One would naturally be inclined to expect the latter result, seeing that the constitution of the chloride of ethylidene is different from that of the chloride of ethylene. The following formulæ will make this intelligible, and show the probable constitution of the isomeric acid:—

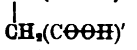
$$\begin{array}{l}CH_2Cl\\ |\\ CH_2Cl\end{array}$$

Chloride of ethylene.



$$\begin{array}{l}CH_2Cy\\ |\\ CH_2Cy\end{array}$$

Cyanide of ethylene.

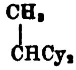


$$\begin{array}{l}CH_2(C\Theta\Theta H)'\\ |\\ CH_2(C\Theta\Theta H)'\end{array}$$

Ordinary succinic acid.

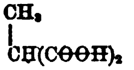
$$\begin{array}{l}CH_3\\ |\\ CHCl_2\end{array}$$

Chloride of ethylidene.



$$\begin{array}{l}CH_3\\ |\\ CHCy_2\end{array}$$

Cyanide of ethylidene.



$$\begin{array}{l}CH_3\\ |\\ CH(C\Theta\Theta H)_2\end{array}$$

Isomeric acid.

It is to be observed that, in the transformation of cyanide of ethylene into ordinary succinic acid, the group $C\Theta\Theta H$ takes the place of each equivalent of cyanogen. In the transformation of cyanide of ethylidene, it is to be supposed that the cyanogen is replaced in a similar manner, an isomeric acid being formed.

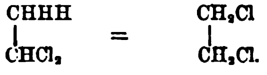
In order to determine this point I made the following experiments:—

A mixture of one equivalent of pure chloride of ethyle *chloré*, which is identical with the chloride of ethylidene, two equivalents of pure cyanide of potassium and a large quantity of alcohol was exposed in a sealed mattrass for twenty-seven hours to a temperature ranging between 160° and 180° C. I had previously ascertained that a high temperature was necessary in order to determine the reaction. At the expiration of this time the mattrass was opened and its contents filtered. The filtered liquor was then treated with solid potash and afterwards exposed to the temperature of a water-bath till ammonia ceased to be evolved. When this was observed the alcohol was distilled off, and nitric acid added in excess to the residue. Finally this was evaporated to dryness at a low temperature, and the liberated organic acid taken up by alcohol. By dissolving in absolute alcohol and crystallising from water, it was obtained quite pure. The quantity I obtained was small; dried at 100° C. it gave the following numbers on analysis:—

Theory. Succinic acid.			Experiment.
		Per cent.	
C_4	48	40·67	40·86
H_6	6	5·10	5·55
Θ_4	64	54·23	
	118	100·00	

It had, therefore, the composition of succinic acid. That it was the ordinary acid was sufficiently proved by the following properties and reactions:—It melted at 179° C. and sublimed in the form of needles on the application of a higher temperature. The vapour produced, on being inhaled, instant coughing and a painful sensation in the nostrils. The neutralised acid gave an abundant brown precipitate on the addition of perchloride of iron. This test was tried both before and after the body in question had been treated with nitric acid, and with the same result.

The only explanation I can give of the formation of ordinary succinic acid in this case is, that the chloride of ethyle *chloré* was, in presence of the cyanide of potassium, partially converted, by the high temperature to which it had been subjected, into chloride of ethylene, one equivalent of hydrogen changing its place with one equivalent of chlorine—



$$\begin{array}{l}CHHH\\ |\\ CHCl_2\end{array} = \begin{array}{l}CH_2Cl\\ |\\ CH_2Cl.\end{array}$$

Since the above was written, I perceive that M. Wichelhaus* has formed the isomeric acid from cyanpropionic acid. The difference between it and the ordinary acid is well marked. Its melting-point is 40° lower, and it does *not*, when neutralised, give a

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* Philosophical Transactions for 1861.

† Annalen der Chemie und Pharmacie, xxx., p. 268.

* 'Zeitschrift für Chemie,' Neue Folge, iii. Band, s. 247.

precipitate with perchloride of iron. These results correspond with the researches of M. Caventon, who has shown that ordinary glycol can be obtained from the bromide of ethyle *bromé*.

PRELIMINARY NOTICE OF

RESULTS ON THE COMPOSITION OF WHEAT GROWN FOR TWENTY YEARS IN SUCCESSION ON THE SAME LAND. (ABSTRACT.)

BY J. B. LAWES, F.R.S., ETC., AND J. H. GILBERT, PH.D., F.R.S.[*]

The results had reference to the produce of a field in which wheat had now been grown, on some plots without manure, on one with farm-yard manure, and on others by different artificial mixtures, for twenty-four years in succession (1843-4 to 1866-7 inclusive). At the Cheltenham Meeting of the British Association, in 1856, the authors treated of the effects of season and manures on the composition of the crop as illustrated by the results of analysis relating to the produce of some of the plots during the first ten years of the experiments. At the Manchester Meeting, in 1861, they recurred to the subject; the analytical results, which then extended to the produce of some of the plots for sixteen years, were, however, chiefly applied to the illustration of certain points in connection with the exhaustion of soils. At the Nottingham Meeting, in 1866, they treated of the accumulation of the nitrogen of manure in the soil of the same experimental field. The results adduced on the present occasion showed the effects of season and manuring on the composition of both the grain and the straw during twenty years of the experimental growth.

The particulars of composition given are the percentages of dry substance, of mineral matter, and of nitrogen, and the constituents of the ash of both grain and straw, more than 200 complete ash-analyses being brought to bear on the subject; and, side by side with these, as indicating the general characters of the produce of the different seasons and plots, are given the proportion of corn to straw, and the weight per bushel of the corn.

In the case of the plots without manure, with farm-yard manure, and with ammonia-salts alone, every year, the ash of the grain last 16, or more, and of the straw of the last 16, of the twenty years, had been analysed; and in the case of 9 differently manured plots (including the above 3) the ash, of both corn and straw, of the first, the last, and two intermediate seasons (one bad and one good) of the last 12 of the 20 years had been analysed. It was the intention of the authors to publish the results of the investigation in detail before long; and on the present occasion they confined attention to a few of the most prominent effects of the respective manures on the composition of the crop, when thus applied for so long a continuance, year after year on the same plot.

It is first pointed out as remarkable, though fully established by their results from the commencement, that variation in manure, even though maintained for many years in succession, and resulting in great variation in amount of produce, affects comparatively little either the proportion of corn to straw, or the weight per bushel of corn, excepting, indeed, in a few extreme cases of abnormal exhaustion or repletion. Nor do the percentages of dry substance, of mineral matter in dry substance, or of nitrogen in dry substance, vary much under the direct influence of variation in manure, unless again in very abnormal cases. Very different, however, is the effect of season; the variation in the character of the produce, in every one of the above particulars, being much greater in different seasons with the same manure than with different manures in the same season.

Consistently with these broad facts, the composition of the ash of the grain is found to be pretty uniform under a great variety of manurial conditions in one and the same season, only in a few extreme cases of special interest varying in any material degree. The same may be said in some, though in a much less degree, of the composition of the ash of the straw, which is obviously much more directly affected by the character of the supplies within the soil.

The general result is that (excepting in a few abnormal cases) the variation in the composition of the ash of the grain is limited to the slight variations due to differences of development and maturation, which, in their turn, are much greater with variation of season than with variation of manure. The composition of the ash of the straw, on the other hand, much more nearly represents the total mineral matters taken up by the plant, and much less the character of development of its own more fixed and essential constituents. In other words, whilst there may be considerable range in the composition of the matters taken up by the entire plant, the tendency in the formation and ripening of the ultimate product, the seed (whether produced in small quantity or large) is to a fixed and uniform composition, the deviation from which is little directly affected by the character of the supplies within the soil, but much more by the various influences of season.

The deviations from the point of fixed and uniform composition, thus due primarily to variations in climatic circumstance, are, however, when considered in relation to other characters of the grain, sufficient to show the general connection between the comparative predominance of individual constituents and that of certain general characters of development. A few illustrations were given; but the fuller treatment of the subject, in its bearing on these as well as on other points, was reserved until the results could be considered in the detail necessary to their proper elucidation.

One point of interest prominently brought out by the results relating to the composition of the straw-ash was, that a high percentage of silica was almost uniformly associated with a bad, and a low percentage with a good condition of the produce—a fact to which the authors had on former occasions called attention, but which, as was remarked by the President, was quite inconsistent with the generally accepted views on the subject.

VERIFICATION OF THE LAW OF HENRY AND DALTON FOR THE ABSORPTION BY LIQUIDS OF CARBONIC ACID GAS.

BY M. KHANIKOFF.

The absorption of gases by liquids was known to natural philosophers at the end of the seventeenth century, but the first important observations on this subject were made by Cavendish and Priestley.

At the beginning of this century, in the year 1803, Dr. Henry formulated a law of absorption of gases which is very simple—namely, he concluded from his experiences that the absorption is directly proportional to the pressure and inversely to the temperature.

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[*] Read in Section B at the British Association Meeting, Dundee.

Nevertheless, it was evident that in the expression of the power of absorption possessed by liquids, in so simple a manner, the pressure and temperature could only be rough approximations, and that in reality a phenomenon so intimately connected with the molecular structure of the liquids could not be expressed in such an uncompounded form. For if this law were admitted without limitations, an unlimited absorption of gases must also be admitted,—already impossible for all gases, especially for coercible ones. Dr. Henry, by the nature of the apparatus that he constructed for his researches, could not come to any other conclusion. His apparatus consisted simply of a glass bell, in which he introduced the absorbing liquid and the absorbable gas. This bell was connected with a manometer by a tube of india-rubber, and after the establishment of the required pressure was separated from the manometer and shaken by the observer a long time, so as to produce the total absorption. This construction has two great imperfections—firstly, a pressure of more than three atmospheres forces the joint, and, secondly, the long contact of the hands of the observer with the vessel containing the gas, makes the temperature of the gaseous volume very uncertain, both before and after the absorption.

Saussure repeated the experiments of Dr. Henry without changing considerably his apparatus, and came naturally to the same result.

Nearly forty years after the experiments of Dr. Henry, M. Bunsen, of Heidelberg, made a valuable series of experiments on absorption of gases at different temperatures; but the ingenious apparatus he invented for this purpose could only be employed under the ordinary pressure of one atmosphere, and left untouched the relation established by Dr. Henry and Dalton between absorption and pressure. More recently, Messrs. Roscoe, Ditmar, and Simms have made very interesting investigations on the absorption of the gases of hydrochloric acid, ammonia, and sulphurous acid; they proved the law of Henry and Dalton to be only exact at an elevated temperature—viz., 40° Centig. for the sulphurous acid gas and 100° for the ammoniacal gas. For this reason my friend Mr. Longuimine and I resolved to undertake a new series of experiments on gases not so absorbable as those investigated by Messrs. Roscoe, Ditmar, and Simms.

Before all it was necessary to construct an apparatus which should not be liable to the above-mentioned imperfections of the apparatus of our celebrated predecessor, Dr. Henry. It was evident that it must consist of a glass vessel exactly gauged, and arranged in a manner to be easily put in connection with a large manometer, and separated from it in a very short time. Secondly, the absorption must be produced, not by shaking the apparatus with the hands, but by moving it mechanically in a space with an invariable temperature.

The first question was easily solved by an iron tube with a cork, and the second by the observation that the contact of the absorbing liquid and the absorbable gas would be very perfect, by revolving the glass vessel containing the liquid and the gas in a great mass of water maintained constantly at the same temperature. These are the two principal differences between our apparatus and those of our predecessors, and without entering into more details on our experimentation executed at the College of France, in the laboratory of M. Regnault, and published in the last number of the *Annales de ph. et de ch.*, I pass directly to the results we obtained for the carbonic acid gas at the temperature of 15°.

If we designate by L, the co-efficient of absorption of a given gas under the pressure of P, and by L_2 the co-efficient of the same kind, but under a higher pressure P_2, by the law of Henry and Dalton we must have,

$$L_2 : d = P_2 : P1, \text{ or } \frac{L_2}{L_1} - \frac{P_2}{P_1} = 0$$

But not only are these differences never *nil*, but they are constantly increasing with the pressure, so that this discrepancy with the law mentioned cannot be ascribed exclusively to the inevitable errors of observations.

From the moment that carbonic acid gas was liquefied it was evident that the co-efficient of its absorption by liquids must be *nil* for two different pressures, *first*, for a pressure *nil*, or nearly *nil*, and second, for the pressure which reduced the gas at a given temperature to a liquid state.

It was evident, also, that the expression of a relation between the co-efficient of absorption and the pressure could not be a lineal function of this variable, but that this co-efficient a could be nearer expressed by the equation:

$$a = A + BP + CP_2$$

For $a = 0$,—the equation $A + BP + CP_2 = 0$ must have two positive and real roots, and also $a = A \times BP - CP_2$ and $B \} A$, and $C \{ B$. Applying to this equation for the different values of a and P, obtained by our experiments, the method of least squares, we found for A, B, and C, the following values:

$$A = \pm 0{\cdot}01520946$$
$$B = \pm 0{\cdot}01393995$$
$$C = \pm 0{\cdot}00283004$$

The values being put in the equation $a = 0$ gives us the two numerical expressions of P, which renders $a = 0$, namely,

Atmos.
P=010983 and P=61 at 1439

The first of these two values is manifest, and requires no special commentary, but the second merits close investigation.

Our observations were made at the temperature of 15° Cent., and we have no direct experiment on the pressure necessary to coerce carbonic acid gas to a liquid at this temperature; but, taking the observations of M. Regnault on the point of ebullition of the liquefied carbonic acid gas at different temperatures, we obtain the following table:—

Temperature.	Pressure in Atmos.
73°3	1·8
56·7	5·3
40·0	11·3
28·9	16·3
12·2	26·8
1·1	37·2

so that the pressure increases for every degree of the Centigrade thermometer

In the first interval of 16°1 by 0 at 2,108
In the second " of 16°7 by 0 at 3,473
In the third " of 11°1 by 0 at 4,684
In the fourth " of 17°7 by 0 at 6,287
And in the fifth " of 11°1 by 0 at 9,369

so that without supposing that our last value of P=61 at 1,439 is exact; but, admitting only that for the interval of 16°1 coming next after 1°1, the increase follows the same law, this increase must be for every degree of the thermometer of 1 at 4,845.

But this value multiplied by 161 gives 23 at 23·9+76·2=61·1.

More importance than they deserve must not be attached to these numerical results, which cannot be strictly exact, being deduced from a too limited series of experiments.

I have mentioned them only to show that our method of experimentation can give us in a comparatively easy way—1st. The value of pressure required for liquefaction of gases; and, 2nd. The numerical value of the maximum of absorption varying only with the nature of the gas and with the temperature.

EXPERIMENTS WITH NITROGLYCERINE.

BY C. A. RICHTER, OF FREIBERG.*

The following is an account of the results of a comparison of the effects of nitroglycerine and the nitrate of soda gunpowder, which is used in this neighbourhood.

The first experiment was made under the guidance of one of the inventor's agents in the year 1865.

Beihilf shaft, which was being sunk thirty feet long by eight feet wide, was chosen as a suitable place for the experiments. The shaft was being sunk in the "country" (*i. e.*, not on the vein), which consisted of hard grey gneiss; now and then only it had a few joints, which rendered the work easier. This happened to be the case on the day of the above-mentioned experiments, and partly explains the extraordinary effect produced by the nitroglycerine. The effect was indeed extraordinary, because bore-holes, placed so as to give them twice as much to do as usual, and even more, did their work perfectly, and, indeed, more than sufficiently, for they caused such an accumulation of stuff in the shaft that for three days no more boring could be done, and the men had to devote themselves entirely to winding up the stuff. The holes were bored partly single-handed, and then one inch in diameter and twenty-seven to thirty inches deep, partly by two men, and then two inches in diameter and thirty-six to forty-eight inches deep. The holes were charged in the mode originally adopted by the agent. As all the holes looked downwards, the nitroglycerine could be poured in by means of a tin funnel. Upon the top of it a small wooden cartridge, three inches long, containing a little powder, was let down by means of the Bickford's fuse, to which it was attached, and then the hole was filled up by hand, without using any tool at all, first with mud and then with sand or small stuff.

It appeared from these first experiments with nitroglycerine that, without any exaggeration, its power was four or five times greater than that of the powder hitherto in use. From this it naturally followed, as the advantages of a powerful explosive material would be most felt in large workings in close little-jointed rocks, such as sinking a shaft in the "country," that such workings could be carried on much faster than had previously been the case. This advantage which nitroglycerine afforded could only be looked upon as most important.

Besides, other advantages were apparent, which, it is true, did not seem so great as the first, but which, nevertheless, promised to exercise a decidedly favourable influence on the economy of mining. They may be summed up as follows:—

1. Fewer men are wanted for working out a certain sized piece of ground, and fewer holes have to be bored than at present. A dearth of miners may to a certain extent be remedied in this manner, and less steel and iron will be used than hitherto.

2. Nitroglycerine does not take fire easily, and when lighted burns but does not explode, and goes out as soon as the flame with which it had been brought in contact is taken away.

3. The holes can be tamped easily, quickly, and without danger.

4. The amount of smoke after a blast is small compared with that of powder, and workmen can go back at once to the place where they have blasted without trouble. This is a considerable advantage in places where there is but little draught, and holes can be bored and fired singly, which was hitherto almost impossible in consequence of the all but impenetrable smoke, and had to be avoided as much as possible.

5. Holes that have missed or only partly torn can be retamped and shot off, which, with the present arrangements, is either impossible or accompanied by great danger.

Against these advantages must be set the following disadvantages:—

a. The gases formed during the explosion of the nitroglycerine have an injurious effect on the organs of sight and respiration.

b. Nitroglycerine explodes on being struck smartly, and easily freezes.

c. The masses of rock which it removes are mostly very large, and considerable time has to be spent in breaking them up.

With regard to the first of these disadvantages, it should be remarked that during the first day's experiments, scarcely any signs of pain in the eyes or head were remarked, although the bottom of the shaft was not particularly well ventilated; later they grew more and more marked, so that it became gradually more apparent that where nitroglycerine was used every effort should be made to secure good ventilation. In the course of time, however, the workmen seem to have become accustomed to the smell, and this disadvantage of the nitroglycerine was no longer looked upon as one which need restrict its employment.

The dangerous property of nitroglycerine of exploding from a smart blow cannot be denied, but this is not more dangerous than the property of ordinary gunpowder of taking fire readily and exploding; and again, the fact of its freezing must be looked upon rather as an inconvenience than a danger.

We must allow that the last of the three disadvantages also exists; holes blasted with nitroglycerine throw down large masses rather than small. These may easily be broken up with a sledge, or, if necessary, be blasted. At all events, it is no greater disadvantage than what happens so often with powder; the rock is blown into small pieces, which are sometimes thrown to a great distance, and may perhaps do damage, not only to the workings but also to the miners. On the contrary, it would seem to be rather an advantage than a disadvantage that the rock should be thrown down gently and without danger, because the workmen, the timbering, and masonry are not so liable to be injured as in blasting with powder.

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* "Berg- und hüttenmännische Zeitung," 1867.

All the results of the first trials with nitroglycerine were so favourable, that they naturally instigated us to obtain further and more certain proofs of the possibility of practically employing nitroglycerine underground. A comparative experiment was made between the nitrate of soda powder in use here and nitroglycerine at Segen Gottes mine, in sinking a shaft, in driving a level, and in stopes. The nitroglycerine was tried first; 226 holes were bored, in all 5,043 inches (English) deep. Of these holes, 180, or 80 per cent., tore perfectly, 40, or 17 per cent., only half, whilst 6 or 3 per cent., did nothing. 9·302 cubic fathoms (English) of ground were removed; the smith's cost was £1 6s.; blasting materials cost 17s. 1¼d.; nitroglycerine, £11 11s. 6d.; wages, £23 12s. 9d.; so that the cubic fathom cost £4 0s. 4d.; the end, on account of its small dimensions, costing comparatively the most, and the sinking of the shaft, for the opposite reason, being the cheapest work. The experiment with the nitrate of soda powder was then made; 559 holes were bored, in all 9,249 inches deep, or 333 holes, with a depth of 4,206 inches more than in the previous experiment. 315, or 57 per cent., tore perfectly; 225, or 40 per cent., only half; and 19, or 3 per cent., not at all. By means of these holes, 6·036 cubic fathoms of ground were removed, in which the end and the stopes do not stand anything nearly so far behind as in the first case. The smith's cost was £1 18s. 2d.; blasting materials, 9s. 7d.; powder, £3 13s.; wages, £24 7s. 10½d.; so that the cubic fathom cost £5 0s. 9¾d. With powder, therefore, 3·266 cubic fathoms less ground were removed, though the wages (in spite of one case of loss of wages) were 15s. 1½d., and the smith's cost 12s. 2d. more; on the other hand, on account of using the needle in blasting, the blasting materials cost 7s. 6¼d. less; the powder also cost £7 18s. 6d. less than the nitroglycerine. Taking all together, the cubic fathom with powder cost £1 0s. 5¾d. more than it did with nitroglycerine.

These experiments show that the employment of nitroglycerine, especially in large workings, already offers great advantages over ordinary powder, and that these advantages lie in the fact that with fewer holes, and in a shorter time, a greater amount of ground can be removed than by the present mode of proceeding. Besides, in working out any given quantity of ground nitroglycerine is found to do the work much cheaper, on account of the extraordinary force with which it blasts the holes, and the smaller quantity of iron and steel used up. Lastly, the holes can be tamped much quicker and without danger, as, if they are loosely filled with sand, any small stuff, or even water, they can be considered as thoroughly well tamped. But even a stronger tamping, such as is in use in the Hartz, has, up to the present time, been entirely exempt from danger, and has doubtless caused a greater effect, under certain circumstances, as may be readily understood. In the Hartz, the cartridges, made of well-glued paper, are filled with sand in order to make them stiffer, and especially to allow their being longer, and thus to spread the explosive force over a greater area; in other words, to give the explosive force a greater leverage, and thus increase the effect. The proper quantity of nitroglycerine for each hole is then poured into the cartridge by means of a little can with a spout, until the sand is more than saturated, and the whole of the nitroglycerine forms one single mass; on the top a little sand is put so as to close the cartridges better, and then the upper part is pinched up just as in the cartridges filled with powder. Where nitroglycerine is used alone, without sand, the cartridges are made long and narrow for the reason explained above; they are closed with a cork. The cartridge in either case is carefully let down into the hole, or pushed in with the tamping bar, or scraper. Upon the top of it comes a paper cartridge, about two or three inches in length, not particularly strong, and filled with good powder, such as sporting powder: it has the ordinary iron needle stuck in it, but without the reed; a little clay is stuck on the top of the cartridge and round about the needle. The tamping employed is clay-slate beaten up fine, and made into a soft mass with water; this is moulded into lumps like pieces of peat, and when dried is ready for use. This tamping is forced in with the iron tamping bar, the wooden one being discarded; the first blows are gentle, and then gradually harder and harder until the mass rings. The hammer, however, is not used, the tamping is simply rammed in with the iron bar. When the hole is tamped it is clayed over, the needle drawn out, and instead of the reed filled with powder, a paper fuse is stuck in and the hole fired off.

The results obtained by the experiments described above would probably have been greater had the workmen been as thoroughly accustomed to the use of nitroglycerine as they are to that of powder. This may be inferred from the fact that far fewer holes were bored in the experiments where nitroglycerine was used than in those where powder was the blasting material employed. For though it must be conceded that holes blasted with nitroglycerine brought about more delay, and caused more time to be spent in winding stuff, and thus caused a loss of time, still the difference in the number of holes bored is so great that we may assume that the men would have bored more holes if they had had more experience in the mode of procedure adopted with nitroglycerine.

The same thing no doubt happened when gunpowder was first introduced, and probably less work could be done with it, and much more danger accompanied its employment, than has since proved to be the case.

In speaking of the many advantages which, according to these experiments, nitroglycerine possesses over gunpowder, it may be added that still further progress has been made with regard to its introduction, and people have not been stopped even by two accidents which have occurred from using it. In one case walls to keep up the attle heap were being built out of some large pieces of rock brought up from the Beihilf shaft, and which had been lying out in the air for some time. These pieces had to be trimmed a little with the hammer, and during this work a small explosion occurred, slightly injuring the mason in the eye.

The explosion was probably caused by some nitroglycerine which had escaped decomposition and remained sticking to the rock. In the second case a hole in the mine did not tear the rock properly, but simply split and loosened it. As the miner was removing these loose pieces an explosion occurred from undecomposed nitroglycerine which remained in the cracks. Luckily, the man was but slightly injured.

The accidents can only have occurred from the nitroglycerine having been used alone without any cartridge, or from the hole not having been properly clayed, so that the nitroglycerine found means of getting into joints and cracks and escaped decomposition. The consequence has been that nitroglycerine is not so often poured straight into the hole, but is enclosed in a cartridge of paper well joined with glue, and in order to give the cartridges greater strength, and the explosive material a greater area to act on, the cartridge is first of all filled up to a certain height with sand, or, as I have since tried, it is at once filled with common powder.

Now, although it has been remarked that a hole containing free nitroglycerine does more work than one in which the blasting oil is contained in a cartridge, which sometimes hinders the quickness of its decomposition, it must not be assumed that the real reason has yet been hit upon, and before a final decision further evidence must first of all be obtained.

Further experiments were made in the above-described manner in sinking a shaft in clay-slate. During a period of three months the men were paid for having worked 372 shifts, £13 15s. 9½d., or, adding in the money paid for extra work, £2 10s. 0¾d., altogether £16 5s. 10¾d. 251 holes, with a total depth of 7,520 inches, were bored, and 11·972 cubic fathoms of ground were removed. Of these holes 229, or 91·2 per cent., tore perfectly; 18, or 7·2 per cent., only half; and 4, or 1·6 per cent., simply blew out, but could be used again on being recharged. In each shift, then, ·67 of a hole, or 20·2 inches were bored, and ·0321 of a cubic fathom of ground removed, which cost 9d., or, including the extra wages, 10½d. The smith's cost was 9s. 4d.; blasting materials, 16s. 5d., and the expense of 99·67 pounds of nitroglycerine £17 18s. 9¾.; so that the total expenditure was £35 10s. 5d., or £2 19s. 3½d. per cubic fathom.

The results of these experiments are still more favourable than those obtained previously, and the reason of this lies in the fact that the experiments were confined to a shaft which was being sunk of greater length and breadth than the previous one, and consequently, the full effect of the nitroglycerine was obtained. The sinking of the shaft in question has been consequently continued with the aid of nitroglycerine, and with excellent results, for the holes do quite three times as much work as they did with gunpowder. The sinking of course proceeds more rapidly; the complaints about headaches caused by the nitroglycerine have ceased, and no other inconveniences have manifested themselves. It must not be denied that this favourable result is partly owing to the length and breadth of the shaft, the tight nature of the ground, as well as to the porous nature of the clay-slate, and the wetness of the sinking. Still some means should be discovered to lessen or prevent entirely the injurious effect which nitroglycerine has on the health of the workman. If further experience does not bring any other disagreeable qualities to light, there is no doubt that nitroglycerine will be more generally introduced in certain workings to which it is specially adapted, and then further improvements may bring about more important results than those which have already been obtained in certain cases.

ON THE DEVELOPMENT OF IDEAS IN NATURAL PHILOSOPHY.*

BY JUSTUS VON LIEBIG.

THE history of natural philosophy teaches us that the final object of our knowledge of matter and phenomena is the material and intellectual acquirements of mankind.

Nature has denied to man the means of resistance against external injurious effects which constantly endanger his existence; and it is in the first place the pressure acting upon him from without, which challenges his dormant mental powers to a combat. He gains from nature whatever he requires for the purpose, as a protection against the influence of climate and against his enemies, as a means of supporting life or restoring health, and thus originates the acquaintance with innumerable substances, and their qualities, and with the processes by which they are made suitable to his purposes. In a former lecture I took occasion to draw attention to the peculiar power of the imagination to call forth the relationship between different pictures raised by impressions upon our senses, and to draw inferences, which depend upon each other in a similar manner as the ideas which guide the understanding in its combinations, only with this difference, that the inferences of imagination are likewise pictures. A word taken as the sign of an idea is the same to the understanding, as an impression upon the senses to the imagination.

The word "tar" is very likely without the slightest reaction upon the imagination of most people, whereas the smell of ship's tar will awaken in the imagination of an individual the picture of a ship or a sea-port visited by him years ago.

The husbandman, the shepherd, the hunter, stand in direct communication with nature. The first learns by simple observation of the senses, how sunshine and rain act upon the growth of his plants, how the seed germinates and developes itself into a plant, how the latter blossoms and bears fruit; in the same manner the shepherd collects a mass of experience on the maintenance and propagation of the animals he tends, he becomes acquainted with their diseases, and through them with nutritive and venomous plants, he constructs himself a time-piece on the starry sky, he learns to know the course of the heavenly bodies and how they move with the seasons. The priest who dissects the animals of sacrifice learns to know their internal parts and their relative functions. A collection of such facts leads those who observe them to draw inferences about the existence of other facts. The shepherd searches for medical herbs for his animals, and applies them to man; from the changes produced by diseases in the organism of animals, the sacrificer draws inferences as to the nature of human diseases. Thus the shepherd becomes the first therapeutist, the priest the first pathologist.

The processes of manufacturing leather, soap, glass, wine, oil, bread, and cheese, have been invented by deductions of a similar nature; they are very ancient, as are also the application of woollen and vegetable fibre to textiles, the art of dyeing, smelting of copper, tin, and iron ores, the extraction of gold and silver.

The superiority of man over other animals chiefly depends upon his capacity to produce inventions which meet his requirements, and the sum total of these inventions in a population expresses the meaning of its civilisation. By means of the inventions of men in art and manufacture, in medicine, mechanics, and astronomy, those facts are acquired which are indispensable to the subsequent development of science; they lead to the knowledge of the phenomena of motion in the firmament and upon the surface of the globe, of the constituent parts of the earth and of the animals and plants on the same; they lead to the discovery of the effects of fire and of the natural forces,—but the experimental science which leads to inventions

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* A Lecture delivered at the Meeting of the Royal Academy of Science, at Munich.

does not seek any solution of the nature and essence of matter and natural phenomena, for this is quite beyond its range. The scientific study of nature aims at different results; it springs from the intellectual requirements of man, from the impulse of his mind to account for the world in which he lives, and for the objects and phenomena which daily engage his senses.

Now, at the commencement of this inquiry, man does not know anything of the nature of his senses: not that the origin of things is inaccessible to them; the senses, intended to assist him in comprehending the outer world, are to him implements the handling of which he does not know; he sees and hears, but he knows nothing of light or sound, he does not know whether he looks with his eyes at certain objects or whether these objects look into his eyes, nor that the temperature he feels is his own.

History teaches us that the popular notions of substances and events in the outer world develope themselves much in the same way as the mind of a child, which becomes acquainted only gradually with the impressions of his senses. By continued and repeated testing things with the hand, the eye, or the tongue, the child learns to recognise and distinguish their shape, colour, and condition, the resistance offered, solid from the liquid, the cold from the warm, the dry from the wet; and the child's further development chiefly depends on his capacity to reproduce within himself the already observed without calling in further assistance of his senses. The pictures retained by memory gradually increase in number, and the human mind begins unconsciously to put questions to the senses; it compares and discovers analogies and distinctions; it notices that under certain conditions cold becomes warm, liquid solid, solid liquid; but a long time elapses before it learns the characteristics of every substance. The idea of motion connects itself with a hand which lifts, pushes away, or draws something towards itself.

With ideas of this kind the investigation of nature began, and its further expansion took place as in an individual, only the senses and minds of many participated in testing matter and in considering processes; every man takes his own point of view, every one observes in the object or phenomenon a different face and profile, and thus it gradually becomes known from all sides; later on, when the details become more distinct, many phenomena are found to have parts, to be compounds, and things are discovered to be present which escaped the simple observation of the senses, our former trust in the impressions of the senses is lost, and measures are adopted to prove their veracity.

In this manner we gradually succeed in acquiring of matter and of processes definite ideas which are applicable to mental operations; with their accumulation the number of their combinations naturally increases, as also the mastery of the mind over the senses;—instead of unconscious questions he now asks positive ones, instead of one a number of questions, the perceptions grow into conscious observations.

No one will maintain that in former times an obstacle had existed in the senses of men preventing them from seeing and perceiving everything in the same manner as we now see and perceive. Want of facts is also not the reason of the difference in our recent and former views of many phenomena; true, we now know more facts than formerly, but those relating to the most common phenomena—to air and fire, vapour and rain, heat and cold—were just as well known and observable to men a thousand years ago as they are to-day, and no one will imagine that before the discovery of oxygen people were the least doubtful as to the necessary presence of air for burning and respiration, or of a strong current of air for the production of high degrees of heat. Our better understanding does not lie in our senses nor in our higher mental capacity, for in regard to the latter the great philosophers of antiquity who endeavoured to gain information on the essence of matter and phenomena serve still at the present day as models unsurpassed.

The true reason is that we have grown richer in ideas; but the ideas of things, or, what is the same, the acquaintance with sense-observed things, their peculiarities and agencies, man does not bring into the world with him; they must be acquired by experience, must be developed in his mind in a totally different manner from the animal, whose faculties are developed to the highest perfection attainable without his co-operation, in consequence of natural laws acting within him.

All these conceptions originated with or sprang from impressions of the senses, and as natural phenomena are always composite, and their conditions or parts are again things which likewise produce definite and invariable impressions of their own, it is clear that a mental conception of a thing or phenomenon must include all these characteristics in itself.

We speak of carbon as a constituent part of plants and animal bodies, without calling to our mind either diamond, coal, charcoal, or lamp-black; the same with phosphorus or iodine, which as such do not even exist in nature. These are all abstract notions which once settled, raise in all cases where their characteristics are perceived, the idea of carbon, phosphorus, or iodine.

The natural phenomena are linked together like the meshes of a net, and the investigation of individual phenomena results in showing that they have in common certain conditions, which as stated above are active realities; and as the totality of the conditions or parts of all phenomena is limited and relatively small, we succeed at last in reducing all natural phenomena to conceptions.

This is the problem of science,—its progress depends upon the accumulation of facts, but it is not in proportion to their number, only to the sum total of mental material deduced from the facts. A thousand facts *per se* do not alter the position of science, whereas a single one which has become comprehensible outweighs in course of time all others in importance.

These views of the development of the ideas derived from experience, or, to use a shorter expression hereafter, of *derived ideas*, may perhaps assist in leading to a more correct estimate of the different epochs in the comprehension of natural phenomena than has hitherto been the case.

The explanation of a natural phenomenon being a logical process, our intellect is *à priori* enabled to lay down the proposition, *i.e.* the logical conditions, which must combine in its comprehension or explanation. This has been done by Aristotle, who says the road of philosophy is that of all other sciences—"One must first collect the facts, and then learn to know the things on which the facts originate; not the mass of facts all at once, but every one must be viewed singly and separately, and the inferences drawn from it; as soon as we have the facts it becomes our business to settle their combination.

"These facts are acquired by observations of the senses; if these latter are imperfect, the knowledge depending upon them will be so likewise."

"We cannot have any general theoretical propositions unless by induction, and induction can be made only by perceptions of our senses, for these have to deal with the individual."

These are the chief principles of investigation bequeathed to us by the great philosopher of antiquity, and they have still the same import which they had two thousand years ago.

On studying his explanations of natural phenomena and those of the whole successive series of natural philosophers, down to our own time, we find that at all times the opinion obtained, that the conceptions were in harmony with the facts, and indeed the definitions always corresponded with the logical laws, but the later are always in opposition to the earlier; what had been held to be right is found to be wrong at a later period, and thus the subsequent definitions annul the former ones, and this goes on for centuries, hence it becomes evident that the truth of the definitions does not depend upon the principles of logic alone.

But if we consider, on the other hand, the derived ideas of Aristotle and subsequent investigators, we find at once the reason why the most highly developed intellect and the keenest logic of themselves, do not suffice for a correct explanation, because this depends entirely upon the extent of the derived ideas.

At the beginning the facts which an idea includes are undefined, and their number and extent is unknown; hence it follows, *eo ipso*, that the first explanations cannot be defined or exhaustive, and that they must change in the same proportion as the facts become better ascertained, and the unknown facts, being part of the ideas, are discovered and are embodied in the idea; the earlier definitions therefore are only comparatively wrong, and the latter are more correct merely because the extent of the idea of things has become larger, clearer, and more definite. This development takes place in a certain succession.

No subsequently developed idea can precede in order of time an earlier one, and if it is ineffective, this happens because it is wanting in extent. With the earlier idea the development of all subsequent ones is bound up.

The definitions of natural phenomena by the Greek and subsequent philosophers prove the extent and comprehension of their "derived ideas," and nothing more; and from this point of view they offer a peculiar interest for the history of the development of ideas in natural philosophy, inasmuch as we discover in them the first outlines in the elevation of our ideas. Aristotle distinguishes the solid from the liquid and the æriform. All solid bodies are to him varieties of *one* solid; it can be perceived that transparent bodies have something in common with water, but language does not suffice to define the other varieties of solid bodies, such as form, colour, or hardness; only what can be produced from them or what results from them are definable. One white stone yields in the fire lime, another white stone fuses to glass, one red stone produces iron, another mercury, a grey stone tin, a black one lead. "The essence of things," Aristotle says, "lies in the form." *This is the first conception of chemical analysis.*

Daily experience teaches us that solid bodies cannot float "in the air or in space without being suspended by something; and as the stars are seen behind the moon, and the moon is nearer to the earth than the sun, these celestial bodies, being solid bodies, must be fastened to transparent rings or discs which move round the earth together with the celestial bodies."

"A freely falling stone moves towards the earth with increasing velocity: sense and mind are quite incapable of recognising that the earth has any part in the falling; it is evident there must be a desire within the stone to return to the place assigned to it by nature." *This is the commencement of the idea of gravity, or of an attractive force.*

These ideas of the Greeks were perfectly in harmony with their experience, and correct so far as they could not have any others. The idea of time, embodied in the compound idea of velocity, was developed and incorporated with the latter 1500 years after Aristotle. Watches or measures for short intervals of time the Greeks did not possess. At the commencement of investigation of nature the compound phenomena of rain, of the rainbow, of burning and breathing, are taken simply as a matter of course, for nothing is known of their parts; at a later period it is discovered that the rain is preceded by a formation of clouds, that without sun no rainbow is formed, and that without air, burning and breathing cannot take place. That part of the phenomenon which is observed at a later time is always looked upon as the cause, the sun as the cause of the rainbow, the air as the cause of breathing and burning, just in the same way as the course of the moon is thought to be the cause of ebb and flow of the tides.

And in this respect the tracing out and defining of the manifold relations of water by Thalso, of air by Anasimenes, of fire by Heraclitus, belong to the greatest discoveries, for these philosophers thereby created the field for all questions connected with the most important occurrences on the surface of the globe, with animal and human life—questions which have occupied us up to the most recent times.

From the acute analyses of words by the Greek philosophers, we learn with great precision the sum total of the ideas included in the words which they used in their mental operations, and it might suffice to compare the contents of one of these words—of the word "air," —to obtain a clear conception of the "derived ideas" of those times and of their development.

The Greeks knew that air inclosed in a bladder resists pressure, and that a glass inverted into water does not fill with the water. Air was looked upon as a space filling, resisting matter, as an element, and next to fire, *i.e.*, smoke ascending into the air, as the lightest element. Up to the beginning of the 16th century, air was considered to be convertible into water,—in the middle of the 16th century as not convertible into water. It was found to contain water in a gaseous form; in the year 1630 the idea obtained that it was heavy, *i.e.*, ponderable matter; in 1643, that it pressed with its entire weight upon all bodies on the surface of the globe; in 1647, that the invisible particles of air press also upon each other and are elastic, that the lower air strata are denser than the upper ones; in 1660, that different kinds of air or gases, elastic like common air, could be artificially produced by chemical processes; in 1727, that such like gases were also in plants, animal matter, ores, and metallic calxes, not as products, but as educts, some inflammable, some stifling fire. In 1774, it was found that among these gases was one in which inflammable substances burnt more briskly than in common air; in

1775, that atmospheric air consisted chiefly of a mixture of two gases, one of which supported combustion, the other not, also that it contained variable quantities of aqueous vapour; at the end of the 18th century, that it contained carbonic acid; in the 19th century, that it also contains ammonia, nitric acid, and, lastly, that all kinds of fungi were floating in the air. Our position in regard to the notion of "air" has been acquired by the labour of hundreds of the most clear-sighted men during an interval of more than 2,000 years, by constantly enlarging, separating, and limiting the first notions; and this constitutes the difference between all notions of matter and of phenomena formerly entertained, and those we have to-day. By-and-bye I shall take occasion to show that the discovery of facts which were added to the notion of air, and which gradually enlarged and defined more completely its extent, was precisely the conception of the facts, *i.e.*, that they were first "conceived" and afterwards discovered.

It will easily be observed that most of our notions in philosophy, and especially in jurisprudence, have been traced out and developed in an exactly similar manner, and that our present ideas of the words "state" or "church" convey a different meaning to what they did 100 years ago. "The idea of divinity" changes and developes with the notion of "force."

Every one of our present notions is the result of time and of infinite labour and mental effort, and if our speculations are less bold than those of the Greeks, it is exactly because their example has taught us that the highest flight of imagination and the most acute logic do not alter our position, and that they are without influence upon the regular course of the development of the "derived ideas." Euclid, with his powerful mathematical mind, thought we looked out of the eyes by means of optical rays. Descartes, one of the greatest philosophers of all times, could not raise himself to the idea of an attractive force.

Very widely spread is the opinion that a gap exists between the Greek and modern philosophy up to the 15th century, and the historians designate the middle ages as the period of stand-still, and the 15th century as that of the re-awakening of science.

This view if applied to Europe is only partly correct, and it cannot hold good for the western parts of Europe, for Germany, England, and the present France. In these countries Greek and Roman culture could not be extinguished in the middle ages, simply because it found entrance into them only at a much later period. We must recollect that at the time of the Academies of Athens Western Europe was inhabited by half savage races who were clothed in hides, that under Charlemagne most of the dignitaries and the most powerful barons of the empire could not write their own names, that in the 13th century Rome was still the centre of the Christian slave trade, and that large slave markets existed at Lyons and in the coast towns of the German Ocean and of the Baltic.

The endeavours of the great Emperor to impart mental cultivation into the rude and ignorant clergy of his time, by the foundation of schools, could not be crowned with success, because the ground on which culture spreads had not been prepared by cultivation. The development of culture, *i.e.*, the enlargement of the sphere of the human mind, depends upon the increase of inventions of the populations, which modify the progress of their civilisation; for by these inventions new facts are gained from nature which are absolutely indispensable for the augmentation of the "derived ideas," or the matter of human thought. But other conditions are yet required for the development of science, whose parent is Culture. Science depends upon the rise of a social class which devotes its energy to the cultivation of the mental domain to the exclusion of every other purpose. The men who apply themselves to this task do not produce any articles which they can realise in exchange for the necessaries of life, like goods in the market, and therefore such a social class cannot spring up until a certain surplus of wealth has accumulated amongst the populations not necessarily required by its possessors to meet their material needs. Only with the introduction of such a state of things are the mental demands of any avail, and the proprietary class exchanges part of its wealth for the sake of cultivating the mind.

Although an uninterrupted traffic without obstacle to the diffusion of Byzantine learning existed in the middle ages between the Eastern Roman Empire and Italy, this learning did not pass over into the Western countries until the 14th century, because the intellectual class had not yet been formed, and with it the conditions of the cultivation and development of learning were still wanting. Naturally Greek culture could only grow up in Western Europe in the same ratio as the civilisation of the populations approached that of Greek antiquity.

It can easily be proved that the civilisation of European populations continually increased after the decline of the ancient Greek states; but owing to peculiar circumstances, to which I will presently allude, it remained for a time without influence upon the progress of culture, *i. e.*, of its mental domain, and hence apparently a gap.

In reference to the share which inventions bear in the development of the conceptions and ideas in natural philosophy, it will be sufficient to draw attention to the facts that the true view of the motion of the earth and the planets originated with the invention of the telescope, and that all progress in astronomy depended upon the perfection of optical instruments. The invention of the telescope was preceded by that of colourless glass; the further improvement of optical instruments depended upon the invention of flint glass, and of the achromatic lens, which Newton thought impossible. By means of Galileo's instruments Uranus and the satellites of Saturn could not have been discovered. Copernicus looked upon his view not as "true," but as more simple and more beautiful, in the same manner as we take the notions of a physiologist of "good" and "beautiful," not in the meaning of true, as it is true that 2 × 2 = 4, but as "suitable," "profound," or "exhaustive." Chemical analysis resulted from the manipulations of the metallurgists, mineral chemistry from pharmacy and from chemico-technical manufactures, organic chemistry from medicine. The theory of heat has been amplified by the steam-engines, the theory of light by photography.

In astronomy the Greeks accomplished the utmost they possibly could with a simple, single sense; they discovered the law of reflection of light, the arithmetical laws of sound, the centre of gravity, the law of leverage and of hydrostatic pressure, and whatever could be deduced from these laws and astronomical observations by means of mathematics; any further progress was excluded by the degree of their civilisation. The source of the trade, wealth, and power of the Greek states in their most flourishing period was a highly developed, extensive industry; Corinth produced what we might call Birmingham and Sheffield goods; Athens was the centre of the products of manufactures now spread over Leeds, Staffordshire, and London, such as woollen textiles, dye-stuffs, ceramic wares, gold and silver articles, and ship-building. The citizens were manufacturers on the largest scale, ship-owners and merchants who had their offices and factories on all coasts of the Black Sea and the Mediterranean; the men of science were the sons of citizens, and well acquainted with manufactures, industry, and commerce. Socrates was a stonemason, Aristotle an apothecary (compounder of medicines and a medical man), and Plato and Solon were no strangers to trade. The men of science spoke and wrote in ancient Greece the same language as the trades-people; in cultivation of the mind the latter were on a level with the philosopher, the difference consisted only in the direction of their knowledge; democratic governments and institutions united both in an intimate personal intercourse, and indeed the thirty-eight chapters of the "Problem" appear to be nothing else but questions of trades-people, artists, musicians, architects, or engineers, which Aristotle tried to solve as far as his "derived ideas" permitted.

No other land in the old world combined in the same degree as Greece, up to the time of Pericles, in her social arrangements, in the close connection of the productive and the intellectual class, the necessary elements for the origin of science. But Greece was a slave state, and slavery was the bane which confined Greek civilisation within certain limits, and made them impassable. All products of the Greek manufactories were the result of slave labour. At the time when Athens flourished there were nearly 2000 slaves for every 100 citizens, which number gives us an idea of the extraordinary development of the Athenæan industry.

Now a tradesman or artisan is not able to produce by himself alone more value than he requires for the very necessaries of life for himself and family, but he must be able to command at will the strength of 20 or more people if he will realise an excess of products of industry large enough to satisfy the requirements of part of the population of the land in which he lives, and all tradespeople in the land put together must produce a very much larger excess if their products are to become articles of export. This last proportion exists in all industrial commercial states, and it did exist in Greece, for the accumulation of wealth in precious metals in the country had not been effected by plunder, but by exchanging products of Greek industry in other countries for the people of which they had more value than gold or silver.

The progress in Greek civilisation chiefly depended upon the transition of the slave state into a free state, which cannot be imagined without the utilisation of natural forces by means of complicated tools or machinery which do the work of slaves.

It is evident that by the invention of machinery, which converts a given natural force, the weight of falling water for instance, into power of labour, and thereby performs the work of 20 people, the inventor may become rich and the slaves free men, and the natural consequence of the introduction of machinery is an increase in the productive class, and thereby in the number of inventors and an enlarged production of the country. But in a slave state the application of natural forces and the substitution of slave labour by machine work is almost impossible, for the profit and wealth of the possessing class in such a state consists in the slaves, and every single citizen sees his property *de facto* endangered by the introduction of machinery; and if the citizens, as then in Greece, form part of the authorities, government and people combine to make the existing state of slavery permanent, the government with the apparently wise intention of securing to the labouring population their livelihood.

The free man only, and not the slave, has the inward impulse and an interest to improve his implements or to design new ones, and thus the workman who builds up a complete machine generally participates as co-inventor. The regulator and other most important parts of the steam engine are inventions of workmen.

Any improvement in the methods of once established routine and manufacture by slaves (themselves working machines) is out of the question.

Liberty, *i. e.*, the loosening of all bonds which prevent man from employing the powers bestowed upon him by God to his own best advantage, is the foundation and the principal condition for the progress of the human race in civilisation and culture.

A glance at China suffices to comprehend the influence upon a gifted people, resulting from simple exclusion of the natural forces in the execution of human labour by machinery; her high civilisation has thereby become stationary for the last 2000 years.

In England, and especially in the United States of North America, where antiquated institutions and laws, grown up from ignorance, do not impede the free employment of human forces, we see on the contrary a continuous increase in wealth, power, and civilisation, and we can scarcely entertain a doubt that in the free states of North America all the elements exist to lead them to the highest scale of culture and civilisation attainable by men.

A modern state, in which free trade does not exist, in which the establishment or extension of a business depends upon the pleasure of ignorant officials, in which the free man is not allowed to choose the place he considers most suitable for the exercise of his powers, and in which he requires the permission of his masters to contract marriage,—this is the ancient slave state, in which the *élite* of the people is poor and without susceptibility for mental or moral improvement, and whose wealth and power form a deceptive varnish, easily rubbed off by slight friction.

We observe the influence of wealth upon the mind of the productive class in those commercial states whose trade originates with industry; the sons of the mechanics and merchants abandon the occupation of their fathers, the source of their wealth; their aim becomes the acquisition, not of money, which they possess in abundance, but of honour and distinction,—they devote themselves to science, to the service of the government, of the army, or the church, and in this manner from the productive rises the intellectual class.

In Europe a manufacture does not go down to the third generation, and, in like manner, mercantile houses pass in the second generation into other hands; therein consists in a free state the renovation of the whole industrial population with each generation, and the perpetual revival of industry,—the industrial grown rich makes room to the man without means, who aspires, and who produces new inventions, and thus a circulation is established in the state, by which its power and wealth constantly increase.

In Greece these relations were shaped in a totally different manner; there, as everywhere, wealth created the intellectual class of society, whose subsistence must be secured by the productive class; but the latter did not renovate and recruit itself in Greece; the free man without means was obliged to emigrate, he might possibly invent a machine, but he could not invent slaves, and without slaves he was debarred from acquiring wealth by industry in his country; the way of trade remained open to the minority only.

As soon as the circulation which preserves industry ceased in the state, and the power of production in the population, upon which its progress depends, Greece had arrived at the limit of her civilisation and culture. The rich people did not produce any further inventions, and with the absence of new facts gained from nature, dried up the source of the ideas indispensable to the enlargement of the domain of the mind, *i.e.*, the culture. The trade in home productions must by degrees pass over into the trade in products of other countries; by this means the accumulated capital could still for a time be saved, but the sinews of life of the slave state withered, for centuries before its decline became visible by outward marks.

The civilisation of the Greeks travelled by the Roman Empire and by the Arabs into all countries of Europe, and its continuous development became manifest throughout the middle ages in the increase of inventions. At the end of the 15th century we already meet with higher algebra and trigonometry, the decimal divisions in calculations, the improved almanac, and in the field of medicine a complete revolution prepared; we find admirable progress in mining and in the metallurgical processes, in dyeing, weaving, and tanning, in the art of making glass, in engineering and architecture, and especially in the sphere of chemistry, paper, the telescope, fire-arms, watches, knitting with knitting-needles, table-forks, horse-shoes, bells, fire-places, and chimneys, the arts of wood-cutting and copper-engraving, wire-drawing machines, the manufacture of steel, plate-glass, tinning of mirrors by lead and tin amalgam; wind, saw, and crushing mills were discovered, and corn-mills and the loom were improved.

These inventions give an idea of the progress of civilisation in Western Europe, and with these and the geographical discoveries all acquisitions in the realm of mind in the 15th century are closely connected; we find a flourishing trade which embraces the whole of Europe from Geneva, Pisa, and Venice, to the coast towns of the German Ocean and the Baltic, and which connects Europe with the East, Arabia, and India, and as its foundation we find an extensive industry in the thriving Flemish, Italian, German, and English towns; we observe in these towns a free opulent citizenship rise in increasing ability, and out of the civic elements the intellectual class of society naturally rises in consequence of accumulated wealth. From here commences the development of Greek and Roman culture.

At first the faculties of the newly originated learned profession were expended in endeavours to take possession of the inheritance of the intellectual treasures bequeathed by antiquity, and as long as the scholars had to learn and were disciples themselves, and the Greek and Roman culture was not yet alive in them, *i.e.*, not capable of being further developed, so long they could not fulfil their avocations of becoming teachers of the people. They even turned away, and not without reason, from the people and their language, for the literature of their country scarcely offered anything worthy to attract and engage their mind filled with the models of antiquity.

The position and the occupations of the scholars of that time co-operated in excluding them from intercourse with the productive classes, and the literature of that period therefore does not give us any information as to the degree of civilisation and culture of the people; the knowledge circulating in the population and penetrating into their thoughts, and developing itself from the better acquaintance with the physical laws and in proportion to the sum total of their more correct ideas of things and their relation to each other, had not yet been collected in books, and was totally unknown to the scholars.

The approximation of the intellectual and productive class was scarcely retarded by the seclusion of the learned profession, because the trading and industrial population up to the 14th century were without the necessary medium in the but little accomplished language of books. In lieu of the scholars the master singers worked successfully in their schools of music for the development and diffusion of the language by voice and pen amongst the civil classes; hitherto the productive class was limited in the exchange and increase of their experience exclusively to personal intercourse by travelling, they were a wandering class of society; but with the acquisition of the language of books the facts and experience gained by them were collected and became diffusible, and writing and reading, hitherto unknown arts, were recognised by the population as highly necessary means to exchange and increase their knowledge,—at first in the towns whose industry was not compatible with a wandering population. In these towns the first public schools were founded; the ardent desire to spread the treasures of antiquity by schools was, with the learned class, just as strong as the longing for instruction with the productive class. Both circumstances combined increased the demand for books, and the difficulty to meet this demand by copyists called forth in the middle of the 15th century the invention of printing.

A century before it would have been without the slightest influence upon the development of the mind; from the time at which it took place dates a new era in the history of culture.

In looking over the literature at the end of the first century after the printing of the first book with moveable letters, one is filled with astonishment at the extent and importance of what was accomplished in natural science and medicine, and at the extraordinary mass of facts and experiences which the middle ages had acquired and handed down in astronomy, technics, engineering, in handicraft and industry, and which were now collected by those intellectually educated scholars of the high schools, which stood nearest to the productive classes, the medical men. In the 16th century the medical men were the founders of the modern natural sciences. They were the media of the intellectual education of the people. But again a century and a half elapsed before the knowledge collected and acquired by them was sufficiently arranged —sufficiently extensive and complete to become effective as a means of instruction in the universities; until then the foreign language in which their knowledge was deposited, and which was familiar to all scholars of Europe, had the advantage, which cannot be too highly estimated, of combining for the solution of their high problems all men of all European countries who devoted their energy to the promotion of science. Without the Latin language in common, their fruit-bearing co-operation would have become impossible. Only towards the end of the eighteenth century, fell, with its exclusion from science and literature, the last barrier which had separated the intellectual class from the productive. Both spoke again, as in ancient Greece, the same language, and understood each other, for science, school, and poetry co-operated to spread an equally high degree of education in all classes.

With the extinction of slavery in the Old World, and the combination of all elements further to develope the human mind, advancements in civilisation and culture were initiated, which are without end, indestructible, and imperishable.

In natural philosophy a change has taken place in the course of its cultivation. For some time this science had been supplied with all facts from which it formed by mental labour the derived ideas, by metallurgists, engineers, apothecaries, the industrial class generally, and natural philosophers had reduced their inventions to ideas which the manufacturing class received back in the shape of explanations and turned to profitable account.

The antipathy of the practical class to theory died out. The artisan, manufacturer, agriculturist, the medical man, consult, as formerly in Greece, the scientific theorists. A new impetus was given to science as soon as the scientific investigator—the teacher of medicine—had acquired the technical skill and dexterity of the practical class, and when, on the other hand, the productive class had adopted the laws and scientific principles laid down by the scholars. Thus the man of science has become an inventor, and is independent in the prosecution of his studies. The manufacturer, the agriculturist, has become an independent investigator, an intellectually free man.

A picture full of life, of endless activity, wide in results, unfolds itself before our view into the future. The past now appears to us in a different light. We look back with indifference at the conflicts between mediæval scholastics and ecclesiastics upon natural philosophy; their opposition was based upon the incapacity of distinguishing at that time between a hypothesis and a fact. All the ecclesiastical and secular power united could not prevent the invention of the telescope and the compass, and the discovery of oxygen, and could not suppress their reaction upon the human mind. A book may be burned, but not a fact.

With the proof of the earth being a small planet revolving round the sun, the former conception of "Heaven," and with the explanation of fire, the conception of "heat" lost its significance; with the discovery of atmospheric pressure, the belief in witchcraft and sorcery lost its foundation, for with the "dread" of a vacuum, nature lost her "will," her love, her hatred. With these discoveries man began to feel his power and his position in the universe.

If Aristotle and Plato had risen alive from their graves and had become teachers in the scholastic institutions of the middle ages they could not have furthered the progress in science for want of increase in derived ideas. The logic of the scholastics and the intellectual gymnastics raised upon it, were only adapted to their times and those which succeeded them; their hostile position to natural philosophy had no influence upon its progress.

And if the whole church and state power had been in league with the natural sciences, still the latter would not have advanced a single step, and would not have developed itself either sooner or in a different manner.

If we were to calculate the influence upon our time and our position which Luther effected, in conjunction with the great discoveries in the field of nature, and also the influence which these discoveries would have had without Luther, we should arrive at a peculiar result. We now know that the human conceptions are organically developed according to certain laws of nature and of the human mind, and we see the tree of human knowledge planted by the Greeks grow in the soil of civilisation, and by our care of the soil we see it develope itself without interruption, and blossom in the sunshine of freedom and bear fruit in proper time. We have learnt that by external force its branches may be bent but not broken, and that its delicate and numberless roots lie so deep and hidden as to withdraw their silent working altogether out of the reach of human will and pleasure.

The past history of nations informs us of the impotent endeavours of political and ecclesiastical powers to perpetuate the bodily and intellectual slavery of men. The history of the future will record the victories of liberty gained by men by inquiring into the essence of things and into truth—victories gained with weapons not stained with blood, and in a combat in which morality and religion will take part only as feeble allies.

ON THE

COMPOSITION AND METALLURGY OF SOME NORWEGIAN IRON ORES.

BY DAVID FORBES, F.R.S., ETC.

The iron ores here under consideration were all strongly attracted by the magnet, and, with the exception of traces of iron pyrites and pyrrhotine, all the iron contained in them was found to be present in the state of magnetic oxide (Magnetite).

The percentage of metallic iron, besides being calculated from the amount of sesquioxide of iron obtained in the course of the analysis, was also determined in a separate portion of the ore by the bichromate of potash volumetric process, after its previous solution and reduction to the state of protoxide by metallic zinc; the oxygen combined with the iron was estimated from the loss in analysis.

The portion of the mineral employed for determining the amount of sulphur was dissolved in nitrohydrochloric acid, filtered from the insoluble siliceous matter, and the filtrate evaporated nearly to dryness in a water bath, so as to expel the excess of free acid (which otherwise might augment the solubility of the sulphate of barytes), and after having been precipitated by chloride of barium, the sulphate of barytes was estimated as usual.

Phosphorus was sought for in a larger amount of the ore, both by Abel's and Spiller's processes (Chem. News, vol. vi., p. 133, and vol. xiii., p. 170, *Eng. Ed.*), and also bythe molybdate process recommended by Eggertz.

The manganese was separated from the iron by carbonate of barytes, and the other constituents, carbonic acid, lime, magnesia, and alumina determined as usual.

The ores No. 1, 2, and 3 occur as veins in the limestones and calcareous shales of the Silurian formation lying to the west of the river Dram in Norway, and have long been worked for the supply of the charcoal blast furnaces in the vicinity.

These deposits of magnetite are in some places greatly disturbed by the intrusion of eruptive granite and trappean rocks; of these the granites are the oldest; breaking through the Silurian strata they frequently dislocate both these and the iron veins, and often send out dykes cutting through the masses of iron ore and dividing it into sections, or, as it were, chambers, separated from one another by walls of granite of varying thickness; the quality of the iron ore does not appear, however, to have been affected or deteriorated at these points.

The trappean dykes, being of still later age, traverse alike both the Silurian strata, iron veins, and granitic intrusions, and in general are found to have a very injurious effect upon the iron ore in immediate contact with them, causing it (sometimes to the distance of several feet) to become more or less strongly impregnated with sulphur, which, combining with the iron, shows itself as pyrites and pyrrhotine, both of which minerals are usually found in greater or less quantity diffused throughout the substance of the rock of the dyke itself.

No. 1. Magnetite from the Aaserud mine, about 5 miles north-west of the town of Drammen.

The specimen analysed was extremely compact in texture, strongly magnetic, and was intersected by minute veins of carbonate of lime; some traces of a greenish silicate were also visible, but otherwise the ore appeared quite free from impurity; it was broken out at a depth of 122 feet from the surface.

The specific gravity was found to be 4·56.

The chemical analysis afforded the following percentage composition:—

Iron, metallic	58·24
Oxygen (or loss)	22·20
Protoxide of manganese	0·14
Carbonate of lime	7·44
" of magnesia	2·48
Alumina	8·00
Silica	1·35
Sulphur	0·15
Phosphorus	0·00
	100·00

In the metallurgical treatment of this ore, owing to its extreme compactness in texture, it requires a prolonged roasting in order to render it as porous and permeable to the reducing action as possible, since, when but lightly roasted, the ore always contains hard unaltered kernels, and is found to be much more irregular and refractory in the furnace.

The ore, in lumps of the size of a fist, is roasted in (somewhat conical) cylindrical kilns fired by the waste gases from the blast furnace, brought down from a depth of about 16 feet below the top of the furnace. It remains in the roasting kiln about from twenty-four to thirty hours; the small amount of sulphur contained in the ore seems to be entirely removed during this operation.

Beyond being charged into the furnace, the roasted ore (and also the limestone used as a flux) is broken up by rolls or stamps to the average size of a hazel-nut, and the charges of ore, limestone, and charcoal are not allowed to exceed at a time about 600 pounds of the calcined ore along with from 30 to 60 lbs. limestone, and 34 cubic feet of fir and pine charcoal; the whole being carefully distributed over the area of the furnace mouth. The blast is obtained from three double-acting horizontal boxes driven by a water-wheel and working into a regulator, from which the furnace is supplied by two tuyeres, the blast being previously heated in an apparatus at top of the furnace fired by the waste gases. The dimensions of the blast furnace are as follows:— Diameter of hearth, 2 feet 6 inches; diameter greatest, 7 feet; height from bottom of hearth to tuyeres, 1 foot 6 inches; do. from line of tuyeres to greatest diameter, 7 feet 6 inches; do. from greatest diameter to top of furnace, 37 feet; consequent total height from floor of hearth to top of furnace, 45 feet.

The bottom stone of the furnace is made of Newcastle sandstone, on which the first eight feet in height of the interior is formed of English (Newcastle) firebrick; above this the entire lining of the furnace is built of slag from the furnace itself, cast in the form of large brick moulds. These slag bricks have been proved to stand excellently; in some cases even for more than four years, during which the furnace has been in continuous operation. They appear to undergo a change similar to the conversion of glass into Reaumur's porcelain. The exterior of the furnace is built entirely of the rough unhewn stone of the neighbouring hills.*

When in good trim, and smelting the ore from the above mine (which, however, only averages about 44 per cent. iron on the large scale) for the production of first-class charcoal pig for conversion into Bessemer steel, the burden and yield of the above furnace is about as follows:—

Ore smelted weekly, 68 tons; limestone (Silurian) used as flux, 5 to 6 tons; charcoal consumed, 10·750 English cubic feet; yield of pig iron, 30 tons, or 44 per cent. of the weight of ore.

The furnace is tapped three times per 24 hours, at 8 a.m., 4 p.m., and midnight respectively, and the cast iron run into chills weighing each about 75 pounds.

The charcoal used is made from the spruce and Scotch firs, and burnt (in heaps) in the open air; as the weight of a given volume of charcoal differs greatly according to the season of the year, the quickness of burning, the length of time it has been burnt before weighing, and the humidity of the atmosphere, it is considered better to use volume instead of weight in such calculations; since, however greatly the weight may alter under these circumstances from the absorption of moisture, the bulk remains comparatively the same.†

When white iron is obtained (instead of the dark grey black) this consumption of charcoal is much diminished, and much more ore can be run through the furnace in the same time.

The slag is glassy, and of a light yellow or brownish tinge, containing only a mere trace of iron; when in thin splinters it is both transparent and colourless.

Until the last two years the whole of this iron has been manufactured with charcoal in Lancashire hearths into hammered bars of a quality equal to the finest Swedish brands, and as this iron has shown itself particularly adapted for conversion into steel, it has been chiefly employed for that purpose from as far back as the middle of the last century.

Since the introduction of the Bessemer process the pig iron from this ore has shown itself equally adapted

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* The iron works here referred to are respectively situated at Eidsfoss, on the Lake Ekeren, and at the mining town of Kongsberg. The author has been connected with the same in the capacity of consulting engineer ever since 1847, first to the Norwegian proprietors, and now to the present owners, the Norwegian Charcoal Iron Company.

† Two different managers of these works considered one English ton of charcoal as equal to 242 and 255 English cubic feet respectively. The consumption of charcoal according to the above data will consequently be about 158 cubic feet per ton iron ore smelted, or 358 cubic feet per ton pig iron produced.

for conversion into Bessemer steel of admirable quality, and the whole production is now employed for this purpose; the pig exported for conversion shows a dark black grey brilliant crystalline fracture, with the lower part chilled to the extent of half an inch or more; both parts, however, being very uniform in appearance.

No. 2. Magnetite from the Saasen mines, about four miles from the east side of the Ekeren Lake.

This ore is compact, strongly magnetic, and contains an admixture of a green silicate, probably epidote, along with a little carbonate of lime; the sulphur present in the ore is evidently in the form of pyrrhotine, as specks of this mineral are seen disseminated in the ore. Its specific gravity was 4·22. On analysis its composition proved to be as follows:—

Metallic iron	61·88
Oxygen (or loss)	20·57
Protoxide of manganese	0·51
Carbonate of lime	3·24
Alumina	2·69
Lime	1·12
Magnesia	0·29
Silica	6·50
Phosphorus	trace
Sulphur	0·25
	100·00

This ore, although not quite so compact as that from the Aaserna mine, contains more sulphur, and consequently requires a prolonged roasting to get rid of this deleterious ingredient.

The roasted ore melts well, and requires about the same amount of charcoal for its reduction as the last described ore, affording a strong light grey iron slightly chilled; there was an evident tendency to the production of white iron.

The slag was nearly free from iron, of a dull yellow colour, and crystalline; it frequently showed streaks of a blue tint, which is commonly found when smelting ores containing sulphur, especially when irregularly roasted.

When converted with charcoal in the Lancashire hearth into bar, it afforded a good strong and tough bar, but in quality rather inferior to the iron from the Aaserna ore; the quality of this, however, would doubtless be much improved if more attention were paid to the sorting and roasting of the ore previously to smelting.

No. 3. Magnetite from the Narverna mine, about three miles from Drammen.

This ore was very strongly magnetic, and possessed a granular texture; it invariably contained particles of iron and copper pyrites disseminated throughout the entire substance of the vein mass; the only other mineral occurring in it appeared to be allocroite, which was frequent. The specific gravity was 4·39.

In the analysis, the copper was precipitated from the solution of the ore in hydrochloric acid by sulphuretted hydrogen, and determined subsequently as oxide; the composition of this ore was found to be:—

Iron, metallic	57·59
Oxygen (or loss)	23·22
Copper	0·79
Protoxide of manganese	0·65
Lime	2·85
Magnesia	0·14
Alumina	3·55
Silica	10·25
Phosphorus	0·00
Sulphur	0·96
	100·00

On account of the amount of copper and sulphur contained in this ore, the pig iron produced in the blast furnace was not convertible into malleable bars when treated with charcoal in the Lancashire hearth.

As the ore itself on the large scale contained an average of over 50 per cent. iron, and the deposit was capable of yielding an immense supply at a nominal price, it became of some importance to inquire whether some means might not be found to eliminate the sulphur and copper, and thus enable the ore to be utilised for malleable bars.

For this purpose several experiments were made (both on the small and on the large scale) in roasting the ore in lumps in kilns as well as in coarse powder in a reverberatory furnace heated by the waste gases of the furnace, sometimes with the assistance of a jet of steam playing upon the red hot ore during the calcination. In some instances the ores thus treated were at once reduced in the blast furnace, but in others they were spread out in the open air, and occasionally sprinkled with water in order to facilitate the oxidation and dissolve out any sulphates of iron or copper formed. After well washing out they were again roasted and melted. Although much better results were obtained by so treating the ores, the bars were never sufficiently good for the charcoal iron market, and the results of these experiments were conclusive only as showing that, on the large scale, the ores could not, practically, be sufficiently purified to fit them for the production of malleable bars of good quality.

The pig iron produced from this ore, even when roasted in the ordinary manner, is well adapted for foundry use, and is employed with advantage in making the finest ornamental castings. For this purpose a small admixture of common English pig (about 20 per cent.) is usually added, and is considered to render the metal softer.

In order to examine whether the ore could be smelted to advantage with coke imported from England, instead of with charcoal, a trial smelting of some weeks' duration was made with Newcastle coke in the same furnace (the dimensions of which have already been given) which was employed for smelting the ore with charcoal.

The furnace was evidently not well adapted to coke. The hearth (of stamped quartz powder) did not stand well; the blast appeared too weak for the heavier coke.

The results, however, possess considerable interest, since both smeltings were carried on under similar circumstances.

Not taking into consideration the irregularity of the actual smelting trials, the results of these experiments may be stated approximatively as follows:—

	Smelting with charcoal.	Smelting with coke.
Iron ore smelted per week	70 tons	80 tons
Cast iron produced "	36 "	41 "
Charcoal consumed "	44 " (10,800 c. ft.)	— "
" per ton iron ore smelted	0·63 "	— "
" " cast iron produced	1·22 "	— "
Coke consumed per week	— "	44 "
" per ton iron ore smelted	— "	0·55 "
" per ton cast iron produced	— "	0·55 "

In both cases the percentage of cast iron obtained from the ore was about the same, or an average of 51·3 per cent.

Other experiments with coke make it probable that under conditions more favourable to its employment, much more advantageous results would have been obtained.

No. 4. Magnetite from Ringkjern mine, Sandsvoerd, near Kongsberg.

The lode containing this ore was situated in granite gneiss, cutting this rock at a nearly vertical angle.

It contained small veins and fragments of white and nearly pure quartz associated with it, as well as occasionally traces of violet fluor spar, and, besides these minerals, it also had numerous specks and large blotches of iron pyrites, from which it could only be separated by careful breaking and hand-picking.

The magnetite itself presented an appearance totally different to that of this mineral in general, and would be at first sight taken for iron glance or specular iron ore, since it consisted entirely of an aggregate of radial plates or scales of the mineral, of a bright black metallic lustre. It gave, however, a black to brownish black streak, and was very strongly magnetic.

The specific gravity was 4·16: the results of its chemical analysis were found to be as follows:

Metallic iron	67·73
Oxygen (or loss in analysis)	25·81
Protoxide of manganese	0·32
Lime and magnesia	traces
Alumina	0·75
Silica	5·00
Sulphur	0·39
Phosphorus	traces
	100·00

This ore, owing to its peculiar foliated texture, roasts with great ease, and also reduces well in the furnace, but always yields a white pig, which has only been employed for castings. The slag produced was clean, and of an opaque yellow colour, frequently crystalline, and always having blue streaks, apparently due to the sulphurous ore.

The amount of silica contained in this ore accumulated a much larger amount of limestone as flux than was required when smelting the three before described iron ores. Some experiments made on this pig iron converted with charcoal in a Lancashire hearth produced hammered bars of inferior quality, being red-short. They, however, possessed no trace of cold-shortness, and were of an extremely tough fracture when broken cold, so that it is probable that a better sorting and calcination of the ores would enable the ore of this mine also to be employed in the manufacture of hammered bars for the market.

ON THE SEPARATION OF

CERIUM FROM DIDYMIUM AND LANTHANUM.

BY M. M. PATTISON, ANDERSONIAN, GLASGOW; AND JOHN CLARKE, PH. D.

The following method has been found to be very effective for the separation of cerium from didymium and lanthanum:—

It is based upon the fact that when chromate of cerium is evaporated to dryness and heated to about 230° F., it is decomposed, and the oxide of cerium remains as an insoluble powder, whilst the chromates of didymium and lanthanum, when subjected to the same treatment, remain unchanged.

The mixed oxides of cerium, didymium, and lanthanum are subjected to the action of an aqueous solution of chromic acid, aided by heat till solution is complete. The chromic acid need not be entirely free from sulphuric acid. The solution obtained is evaporated to dryness, and the residue heated to about 230° F. Hot water is then added, which dissolves the lanthanum and didymium and leaves the oxide of cerium, which is then separated by filtering. Thus obtained, the oxide of cerium is a yellowish-white powder which is almost completely insoluble in acids, but is rendered soluble when fused with the acid sulphate of potassium.

This process may also be employed for the quantitative determination of cerium, as it was found by careful trial that not a trace of cerium could be detected by the best known processes in the solution, after its separation, as above described.

ON GAS ANALYSIS.

BY DRS. GRANDEAU AND TROOST.

I. Mixture of Oxygen, Carbonic Acid and Nitrogen. —O, CO_2, N.

1. Introduce a portion of the mixture into a graduated tube over the mercury trough, and note accurately the volume.

To estimate the carbonic acid, pass into the tube, by means of a curved pipette, a small quantity of a concentrated solution of potash, and agitate several times until there is no further variation in the level of the liquid; the carbonic acid will be absorbed by the potash. To obtain the volume very accurately, transfer the tube to a vessel of water, so as to allow the alkali to fall out; then retransfer the gas to another tube and determine its volume, saturated with moisture.

To estimate the oxygen, first introduce into the tube a concentrated solution of potash, then a little pyrogallic acid. Upon agitation, the oxygen is absorbed, and the nitrogen remains. The amount of the latter gas may be obtained by taking the same precautions as in the former instance.

After having determined the volume of carbonic acid, phosphorus may be employed to absorb the oxygen. The experiment may be performed in two ways.

a. In the Cold.—In the tube containing the gas (over mercury) pass up a long stick of moist phosphorus, the sides of the tube being at the same time moistened. The oxygen combines with the phosphorus, giving phosphorous acid, which dissolves in the water. At the end of an hour the absorption is completed. It may be known by the absence of white fumes on the stick of phosphorus. Remove the latter, dry the gas, and measure its volume; it will be the nitrogen.

b. With Heat.—The analysis is effected much more rapidly in the following manner:—In a curved tube containing the mixture of oxygen and nitrogen standing over water, introduce by means of an iron wire a small piece of phosphorus, so that it rests on the upper curved portion of the tube; then remove the iron wire and heat the phosphorus, at first carefully, to volatilise the water which remains in the bend of the tube, and then rapidly, so as to inflame the vapour

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of phosphorus. A greenish flame will be seen to advance, gradually absorbing the oxygen of the air. When it has descended to the level of the liquid it disappears, and the experiment is terminated. Allow it to cool, and determine the volume of the residual nitrogen.

2. The analysis of a mixture of carbonic acid, oxygen, and nitrogen may be effected with a little more accuracy in the following manner:—The dry mixture being contained in a graduated tube standing over mercury, introduce a piece of caustic potash fixed to the extremity of a platinum wire, and slightly moistened. When the carbonic acid is absorbed, withdraw the piece of potash, and a simple observation gives the residual volume of the mixed oxygen and nitrogen, perfectly dry.

This residue is introduced into a mercurial eudiometer. This consists of a glass tube about 2 centimeters in diameter, having two platinum wires melted through the upper part terminating exteriorly in a loop, and curved inside in such a manner as to have their extremities opposite to each other, and one or two millimeters apart, across which the spark passes.

Add to the mixture double its volume of hydrogen, and pass the electric spark. Water will be produced by the combination of the hydrogen and oxygen in the proportion of two volumes of the former to one volume of the latter. One-third of the diminution in volume represents, therefore, the volume of oxygen. The volume of nitrogen is obtained by difference. It is the excess of the original volume of the mixture over the sum of the volumes of oxygen and carbonic acid.

The estimation of oxygen by the eudiometer is not exact unless this gas is present in tolerable quantity in the mixture. If there is only a very small proportion, it is necessary, in order to ensure complete combustion, to take the precaution to introduce into the mixture a sufficiently large quantity of oxy-hydrogen gas, obtained by decomposing acidulated water with three or four Bunsen's elements. The gas should be passed through concentrated sulphuric acid in order to dry it.

II. Mixture of Oxygen, Hydrogen and Nitrogen.—O, H, N.

1. After having measured the volume of the mixture, absorb the oxygen by potash and pyrogallic acid, or by phosphorus as described in section I.

Pass the remainder into a curved tube over mercury and introduce into it a piece of compact oxide of copper,* and heat it for about twenty minutes, all the hydrogen is then absorbed; the residue will be nitrogen; it may be transferred to a graduated tube, and its volume measured.

After the absorption of the oxygen the hydrogen may be estimated by introducing it into the eudiometer with half its volume of oxygen. Two-thirds of the diminution of volume occasioned by the passage of the spark represents the volume of hydrogen. The nitrogen is given by difference.

2. The analysis may also be effected entirely by the eudiometer. Introduce the original mixture into the eudiometer with twice its volume of hydrogen, and pass the spark. The volume of hydrogen which enters into combination will be two-thirds the diminution of volume, the oxygen being represented by the other third. This first experiment will therefore give the amount of oxygen. In order to ascertain the amount of hydrogen in the mixture, add to the residue of the first explosion half its volume of oxygen, and pass the spark a second time. Two-thirds of the diminution of volume will be hydrogen. The excess of the sum of the volumes of hydrogen burnt in these two experiments, over the volume of this gas introduced into the eudiometer, represents the volume of hydrogen found in the original mixture. The nitrogen will still be given by difference.

III. Mixture of Hydrogen, Carburetted Hydrogen, and Nitrogen.—H, C_2H_4, N.

Introduce the mixture into a mercurial eudiometer with twice its volume of oxygen, and pass the spark. The free hydrogen, and that in the carburet, combine with the oxygen to form aqueous vapour which condenses. The carbon becomes carbonic acid. The residue is therefore a mixture of nitrogen, oxygen, and carbonic acid.

Pass these gases into a graduated tube, and after having observed the volume, absorb the carbonic acid with potash. The diminution of volume gives the volume of carbonic acid, which is precisely equal to the volume of the carburetted hydrogen, as shown by the equation:

$$\underbrace{C_2H_4}_{\text{4 vols.}} + 8O = 4HO + \underbrace{2CO_2}_{\text{4 vols.}}$$

If a little pyrogallic acid is then introduced into the potash, the rest of the oxygen is absorbed, and the volume of nitrogen is obtained as a residue.

As to the hydrogen which existed in the free state in the original mixture, its amount is obtained by taking the excess of the volume of the original mixture over the sum of the volumes of nitrogen and carburetted hydrogen.

(To be continued.)

ON SOME POINTS IN CHEMICAL NOMENCLATURE.*

BY A. V. HARCOURT, M.A., LEE'S READER IN CHEMISTRY.

One of the dangers against which the advancing sciences need to be always on their guard is that of mistaking verbal for real questions. An example of this well-known truth is furnished by a controversy, which has been carried on among chemists in an intermittent fashion during several years, and which has recently broken out afresh.

The subject of the controversy is the question, What is an acid? As to the meaning of the name there is a tolerable agreement, though no chemist would attempt to state it with scientific precision. The only appeal is to the popular use of the word. By an acid, I suppose we mean a sour, corrosive substance, which changes vegetable colours, and effervesces with carbonated alkalies and is neutralized by them. At any rate an acid ought to have most of these properties. It has happened, however, that by a process very common in the history of scientific nomenclature, many substances have been likened, on other grounds, to those to which this description applies, and have thus received the name of acid though devoid of these properties; while, on the other hand, many substances having all these

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* M. H. Ste-Claire Deville prepares this compact oxide by fusing 2 parts of oxide of copper with 1 part of oxide of lead. The fused mass is run on to a plate of copper, then broken into pieces and preserved in bottles.

* Proceedings of the Ashmolean Society, New Series, No. 1.

properties are by common consent excluded from the acid class. But the controversy does not originate in this border land. It relates to some of the most typical and best known acids. Different chemists apply to different substances the familiar names of sulphuric acid, nitric acid, carbonic acid.

Formerly these names, which may be taken as examples of the class, were applied to particular oxides of sulphur, nitrogen, and carbon. But the founders of the unitary system of chemistry, laying stress upon the facts, (1) that these substances exhibited their characteristic properties only in the presence of water, (2) that some well-defined acids while having hydrogen in their composition were devoid of oxygen, transferred the name of acid from these oxides to their actual or supposed hydrates, and assigned to the oxides thus dispossessed the name of "anhydrides." This system having met with general acceptance among scientific chemists, the change of nomenclature has also been widely adopted.

Quite recently one of the foremost of English chemists, Dr. Williamson, in a manual published at the University Press, has reverted to the ancient practice, objecting to the name of "anhydride" as unmeaning, and insisting on the advantage of co-ordinating, in each series, hydrogen salts with metallic salts instead of separating them under a distinctive name. This retrograde proposal has naturally been controverted by the adherents of the prevailing system, and thus it is now more uncertain than ever what substance a chemist means when he speaks, for example, of sulphuric acid. All discussion, Gerhardt says, remains necessarily sterile when the disputants are agreed as to facts and differ only about the meaning of words. In this case, I venture to think the best solution of the controversy may be to discontinue the use of the word "acid" as a specific name for a particular class of substances, and to apply it only as a descriptive term to any substance having the properties which the word connotes. The name of oxide appears perfectly applicable to, and sufficient for, all those substances which have been called "anhydrous acids," and it appears positively mischievous to distinguish the acids from the other salts of a series. In order to determine what particular name these oxides and salts should receive, it is necessary to decide some much more general questions of nomenclature, as to which there is at present great difference of opinion among chemists.

I should exceed the limits I have undertaken to observe, if I attempted to offer a criticism upon the various names that have been proposed for these classes of substances, but I shall venture to state what names appear to me to be the best, and shall endeavour to offer some reasons for my conclusions. The name of "carbonic acid," for the substance whose symbol is CO_2, is perhaps too well established ever to be changed, but for a systematic name I should prefer "carbon dioxide;" and similarly for anhydrous sulphuric acid and anhydrous nitric acid I would substitute "sulphur trioxide" and "nitrogen pentoxide." To the salts of these acids, as they are called, I would give such names as "sodium carbonate," "nickel sulphate," "silver nitrate;" and the hydrated acids I would, upon the same principle, call "hydrogen sulphate," "hydrogen chloride," "hydrogen phosphate," etc.

Two sets of names are more familiar than these: (1) "binoxide of carbon," "sulphate of nickel," etc.; and (2) "carbonic binoxide," "nickelic sulphate," "hydric chloride," etc. To the first it has been objected that the use of the preposition "of" is incorrect, being in fact a too literal translation from the French. "*Carbonate de soude*" no more ought to have been rendered "carbonate of soda," than "*table de bois*" should be translated "table of wood." Accordingly chemists are now accustoming themselves to give up this usage which had acquired a formidable prescription. As to the second set of names, I see no advantage in adding the adjectival termination except where it is desired to prefix a numeral or to distinguish a class of salts by one or other of the terminations "-ic" or "-ous." In these cases, which are comparatively few, such terminations might be used without rendering it necessary to employ them in other cases. It is to be objected to them that they necessitate the use of Latin names in many cases instead of English, as "argentic" for "silver," or are tacked on barbarously to words which are not Greek or Latin, as in "nickelic," "zincic," etc. Further, the special use of these terminations, to indicate which of two classes of salts contains the larger proportion of metal, causes some confusion when they are applied without any such meaning. For example, "calcic sulphate" corresponds in constitution to "ferrous" and not to "ferric sulphate." I would rarely attempt to indicate the formula completely by numerals introduced into the name, but would give as a rule the simple name of "chloride" or "oxide" to that compound which is normal according to the atomicity of the particular metal. Thus I prefer "sodium dioxide" to "disodium dioxide" for Na_2O_2. In a few cases it may be convenient to express more; thus the magnetic oxide of iron might be named "terferrum tetroxide." For the parallel series of tin, mercury, and iron salts respectively, the convenient designations of "stannous," "stannic," etc. may be retained, while each pair would naturally be represented by the simple name of the metal. Thus we might speak of the two "iron sulphates," "ferrous sulphate" and "ferric sulphate." A complex salt such as microcosmic salt might be called "sodium, ammonium, and hydrogen phosphate." It appears to me clearly better to have a descriptive name of this kind thus divided into several distinct words rather than to attempt to form such a single word as "hydro-sodio-ammonic phosphate." I will only add that names with a numeral appear to me generally better than such vague terms as "peroxide" and "suboxide," and that as far as possible Latin numerals should be joined to Latin words, and Greek numerals to Greek words.

PARIS EXHIBITION OF 1867.

(From our Special Correspondent.)

Messrs. Hopkin and Williams, of New Cavendish Street, London, have a most remarkable display of rare and valuable chemicals. It is not too much to say that these gentlemen have, by the articles they have exhibited, shown themselves to be second to none in their profession for chemical skill. The collection of specimens illustrating the chemical history of thallium and its compounds is absolutely unrivalled, and casts into the shade everything that has been done by the French chemists. This series of compounds is far too remarkable to be cursorily passed over; we shall, therefore, describe them *seriatim*, as briefly as is compatible with doing justice to the subject.

Metallic Thallium.—This is shown in the form of a large bar, weighing 2 lbs. It has a dull leaden colour, owing to a film of oxide. It is only necessary, however, to wash the bar in water to obtain it with a brilliant metallic lustre, The water becomes strongly alkaline, and is in fact a solution of oxide of thallium.

Oxide of Thallium.—This substance yields a colourless solution when dissolved in water, it is strongly alkaline, and precipitates solutions of nitrate of silver, etc., as if it were a solution of hydrate of potassium. On concentration of the aqueous solution of oxide of thallium, it deposits yellow crystals of the hydrate; these crystals lose water very readily, yielding the dark coloured mass of dry protoxide of thallium as exhibited. If dried *in vacuo* so as to prevent the formation of traces of peroxide the salt would be colourless.

Suboxide of Thallium.—When the metal is fused in the air a very mobile, dark coloured liquid is formed upon the surface of the melted metal; upon cooling it can be separated; it is insoluble in water, and has entirely different properties to the peroxide.

Peroxide of Thallium.—This specimen was prepared by adding a solution of hypochlorite of sodium to a boiling solution of protoxide of thallium, the peroxide is precipitated under the form of a brown powder.

Sulphate of Thallium.—A superb specimen of large and fine crystals about two pounds in weight.

Nitrate of Thallium.—A large specimen, colourless and well crystallised.

Chloride of Thallium.—About two pounds weight crystallised from twenty gallons of boiling water.

Perchloride of Thallium.—A very fine specimen produced by the action of nitric acid on a boiling solution of the chloride.

Bromide of Thallium.—This salt is less soluble than the chloride, and was consequently prepared by precipitation.

Iodide of Thallium.—Almost insoluble in cold or hot water; a cold solution of bromide is precipitated by iodide of potassium.

Periodide of Thallium.—Made by partially precipitating a boiling solution of the perchloride by iodide of potassium. The salt is dark brown, and if in the course of preparation an excess of iodide of potassium is used, it rapidly becomes converted into ordinary iodide.

Silicate of Thallium.—Prepared by decomposing a solution of nitrate of thallium with silicate of sodium; the solution is boiled, and on cooling deposits the silicate in crystals.

Phosphate of Thallium.—Produced in the same manner as the silicate, substituting phosphate for silicate of sodium. The phosphates of thallium require investigation.

The molybdate, vanadiate, sulphantimoniate, sulphocyanide, cyanide, benzoate, oxalate, borate, chromate, bichromate, are also exhibited, but do not call for any special remarks.

Sulphide of Thallium.—This compound may be produced by fusing thallium and sulphur together. Sulphide of hydrogen does not precipitate the solutions of the sulphate, nitrate, etc., but an abundant precipitate is produced in the solution of the acetate, and as thus prepared it can be washed and dried, but if precipitated by sulphide of ammonium, and the precipitate be washed, etc., when approaching dryness it will take fire even at a very moderate temperature. In fact, Messrs. Hopkin and Williams informed your correspondent that they could not succeed in preparing a permanent sulphide of thallium by this process.

Carbonate of Thallium, upwards of 1lb. in weight, and in good and white crystals, is also an interesting salt.

There were also exhibited the acetate, chlorate, bitartrate, and tungstate of thallium. Among the double salts we observed thallium alum, platino-chloride, sodio-tartrate, and the tartrate of thallium and antimony (thallium tartar emetic.)

It would be impossible to estimate too highly the skill, perseverance, and chemical knowledge shown by this firm in the preparation of the extraordinary and unrivalled collection of preparations of thallium contained in their case.

In addition to the above there is an admirable collection of fine chemicals, not mere showy crystalline salts, but preparations requiring far more than the average amount of scientific information for their production. The bottle of cantharidin contains nearly 5 ounces of superb white crystals of this substance. The specimen is the finest of its kind in the Exhibition, and is decidedly better than that in the case of M. Menier, although the latter is very good. It would be improper to pass over without notice the bromides of potassium and ammonium, and the iodide of ammonium.

The specimen of pyrogallic acid has evidently met with an accident, and been subsequently put into a dirty bottle; this is unfortunate, as this firm for many years were by far the largest makers of it.

The crude oxalate of cerium, which is still used as a remedy in cases of obstinate vomiting, does not call for any very special remark. The same applies to the permanganate and cyanide of potassium.

The glacial phosphoric acid is very fine and pure. Large quantities of this substance are shown in the German department, and sometimes in sticks. In the latter state it has a very pretty appearance, but the German acid found in commerce is generally far from pure, containing soda, and almost invariably more than traces of lime and ammonia. Your correspondent is informed by Messrs. Hopkin and Williams that the syrupy phosphoric acid yielded by the action of nitric acid on phosphorus will never yield a solid brittle phosphoric acid by direct heating; no matter how elevated the temperature to which it is subjected, the product always remaining is a soft adhesive mass, and, at an elevated temperature, either partially sublimes, or, at all events, yields a volatile product.

The scale preparations of iron—for example, the ammonio-citrate, the ammonio-tartrate, the potassio-tartrate, and the citrate of iron and quinine—look very well, especially when their long exposure to light and heat is considered. These articles, although invented in France, have become enormously used in England, and are made upon a very large scale by several manufacturing chemists. The price in England is three or four hundred per cent. cheaper than in France or Germany. The scale preparations in Messrs. Hopkin and Williams' case have certainly kept better than any others we observed, excepting, perhaps, the ammonio-citrate of bismuth which has turned black.

Taking it all together, we consider that Messrs. Hopkin and Williams' case is in the very highest degree creditable to them. It is quite evident that they do not allow themselves to fall behind any of their contemporaries; and considering the efforts that our chemical manufacturers are obliged to make to compete with the skilled labour and low wages which are such immense helps to their German rivals, this is saying a great deal.

We are glad to find that they have been awarded a silver medal.

FOREIGN SCIENCE.

(From our own Correspondent.)

Paris, Oct. 29, 1867.

Animal Miasma—Spectrum of the Bessemer Flame—Meteoric falls in Greece—Silurian System of Bohemia.

Dr. J. Lemaire continues his researches on the nature of miasma exhaled by the body of a healthy man. He finds that it is on the periphery of the body and outside the organs on which develope the microphites and the microzoaires on a man in good health. The deposit, vulgarly termed "dirt" or "scurf," that the perspiration, mingled with atmospheric and other dust contained in the linen, produce on the skin, and which accumulates every day, if the body be neglected, gives rise to myriads of microphites and microzoaires. They are the more numerous according as the deposit is more abundant. This coating contains an albuminoid matter capable of coagulation, furnished by the perspiration, which also maintains it in a humid state.

The contact of the air and the mean temperature of the neighbouring body, about 37° C., are the cause that this crust is in the most favourable condition for fermentation to take place, and the infusoria to be disengaged and developed. In studying the effects on men and women from 30 to 70 years, whose cleanliness had been neglected for a week or two, there were remarked a fetid odour in various parts of the body, and a slightly acid liquid containing transparent spherical, ovoidal, and cylindrical bodies similar to those found in the confined air of the Eastern fort; thousands of bacteria (*Bacterium termo, Bacterium catenulæ* formed of 2, 3, 4 and 5 articulations, *Bacterium ponetum*), vibrios, etc.

In the former experiments the presence of entirely developed animalculæ six hours after the condensation of the aqueous vapour of the barrack-ward, can be explained by the elevated temperature of the human body and the existence of a great quantity of vapour in this air. They are no doubt furnished by the elevated temperature of the climate and the animal heat in adults, which produce miasma so fatal in tropical climates to man and beast.

Professor A. Lielegg has continued his researches on the spectra of the flame in which the melting is carried on according to the Bessemer system. This flame being only carbonic acid gas in an incandescent state, and the spectrum of this gas being yet unknown, the observations of M. Lielegg have served to fill up a gap in the series of spectra produced by the gases in combustion. The apparition and the disappearance of some of the luminous fixed lines is closely connected with the metallurgical operations. At the moment when the decarburisation of the iron is nearly terminated, these spectral lines assume essential modifications. The apparition of a group of lines and of an isolated line in the violet-blue portion of the spectrum, marks a particular movement of the period during which the soft iron is being formed, and these lines disappear sooner than all the others; their appearance and disappearance serve, therefore, to indicate the termination of the process.

In a letter addressed to M. Haidinger, M. Jules Schmidt gives some notes upon some igneous meteors observed in Greece. The meteoric fall of Nauplia was observed the 17-19 June, 1850, about ten o'clock at night, by M. A. R. Logothetis. In a perfectly clear sky, the meteor, like a swarm of falling stars, seemed to travel from East to West, and fell at the North of Nauplia near Tyrinthia. Shortly after its appearance a loud explosion like thunder was heard. The search made on the following day gave no trace of meteoric substances. It was only subsequently that a fragment of black stone was brought to M. Logothetis. This fragment, lost by carelessness, was about twice the size of an egg, and had the aspect of a metallic substance subjected to the action of fire, presenting here and there reddish golden specks. It evidently contained metallic iron and sulphur. A meteor of the first magnitude was observed at Athens on May, 1857, at 11h. 46m., at 20° N.W. of the zenith, having its radiating-point situated between Scorpio and Sagittarius. It was bright green, with a beautiful red trail. A sudden explosion accompanied the traject, which only lasted a second. An explosion similar to that of a heavy cannon was heard about a mile and a half fifty-three minutes after its extinction. The conclusion is, that the detonation took place fourteen geographic miles from Athens, at the height of about thirteen miles above the plain of Thebes. A similar meteor but much smaller was observed the 16th May, 1862, at 8h. 24m. The catalogue of M. Schmidt states the occurrence, for the 17th May, of eleven great meteors, four of which were terminated by showers of stones; on the 26th May, date of the fall of the meteoric stone of Agram (1751), seven meteors, two of which were accompanied by showers of stones, and one by the fall of meteoric iron. An enormeous one passed over Greece on the 27th May, 1867, at 2¼ to 3h. in the morning.

M. J. Barrande published in July a new continuation of his magnificent work on the Silurian system of the centre of Bohemia, a volume in 4to, containing 179 pages and 16 plates, treating on Pteropode mollusca (69 species, included in 8 genera) the remains of which are included in this formation and others, such as Columates, Trochocystites, Chittons, and Rhombifera.

F. Moigno.

Paris, Nov. 5, 1867.

Tar Water.—Leaden Electrodes.—Production of High Temperatures.—Father Secchi and the Roman Observatory.—Self-registering Barometer.—Paraffin Lubricants.

M. Guyot, an apothecary in Paris, has made a sort of concentrated tar-water, which he calls tar-liquor, calculated to render great service to the medical world. The great advantage of this preparation is to be able to furnish water more or less charged with tar, according to the affection to be treated. This preparation is free from any quackery and has given the best results.

The employment of tar-water is of old date. The discovery of its medical properties is due to Bishop Berkeley. This intrepid missionary embarked at the end of the 18th century for Rhode Island. The ship, having remained becalmed for several days in the midst of the ocean, was attacked by a terrible epidemic which decimated the crew. Some of the sick, lying at the bottom of the hold, the prey to a terrible fever, burning with thirst, drank the bilge-water of the ship, which was impregnated with tar. It was well they did so, for all those who drank of the water were rapidly cured. Berkeley, a skilful and profound observer, remarked at once that it was the tar-water which cured them, and by drinking it abundantly himself he was preserved from the contagion. On his return to Europe he experimented incessantly, and hastened to publish to the world, in a very interesting volume, the virtues of tar-water.

M. Planté has forwarded to the Society of Encouragement a plan by which platinum electrodes can be replaced by leaden ones. A short time after, he proposed this substitution in order to prove the superiority of the secondary current produced with lead over the secondary current furnished by platinum. He showed to the Academy of Sciences, at the meeting of March 26th, 1860, the powerful effects obtained with a secondary pile with leaden plates of great surface. M. Leon Foucault wrote on this subject on 7th June, 1860:—

"M. Jacobi employed secondary piles in platinum, but M. Planté employed, almost at the same time, lead, as being preferable to platinum, not on account of the economy, but certainly by reason of the secondary reaction. And to show in a striking manner the superiority of lead for this purpose, M. Planté has constructed at very little cost a secondary pile of great power, which can become in the hands of physicists an instrument of value. Lead, flexible and supple, is found in commerce, laminated in thin sheets, etc. Experiments have proved that two simple leaden wires polarised were able to produce a sufficiently intense secondary current to destroy residual magnetism that could not have been neutralised except by a platinum battery of a great number of elements."

On 13th May, 1861, M. Planté presented to the Academy of Sciences, in a sealed letter, the substitution of lead for platinum, in order to obtain a greater quantity of ozone in the decomposition of water by the pile. He subsequently, being attached to the establishment of M. Lenoir, when the process of round casting is carried on by electro-decomposition, essayed with perfect success cores of lead placed in the moulds.

By the invention of the copper soluble anode, M. Jacobi has happily completed his discovery in electro-metallurgy.

By the employment of the insoluble platinum, M. Lenoir has been able to succeed in the galvanic reproduction, in a single piece, of solid objects.

Introducing in his turn the lead anode, M. Planté has made an important improvement in the same direction.

In metallurgical operations in crucibles, M. Douenne has succeeded in diminishing the length of the operation and reducing the cost of fuel without having need of more blast. It also completely does away with smoke. Steam, admitted in the form of a jet, is decomposed into the elements of which it consists, oxygen and hydrogen, by which the oxygen combines with the carbon and hastens the combustion at the base of the crucible: the hydrogen, disengaged, being in a medium of higher temperature, rises around the crucible, when it is burnt by the air. The necessary temperature of fusion is thus produced at the proper point by the combustion of the gases resulting from the excessive temperature.

We have received from our illustrious colleague, the Reverend Father Secchi, Director of the Observatory of Rome, and who has just quitted Paris to return to his post, a letter which grieves us much. We cannot forget that in 1848 his predecessor, Father Vico, author of some great discoveries, was driven from Rome by violence and went to live in America, for which place we procured for him a free passage! We hope that the same lot will not befal Father Secchi, and that he will be free to illuminate the Pontifical Observatory with new discoveries. M. François Arago was also instrumental in the protection granted to the learned and excellent Father Vico, who died, not in America, but at London, 15th November, 1848.

M. Brequet, the well-known clock and scientific instrument maker, has exhibited at the Champ de Mars a new self-registering barometer called the *Barometrograph*, giving indications every six hours, by diagram, of the pressure of the atmosphere. It consists of four metallic boxes, the upper and lower of which are undulated (the usual aneroid barometer); a vacuum is made in each of these boxes separately, and they are attached to a chain the movement of which is four times greater than that of a single box for the same variation of pressure. A steel spring of great strength acts upon these boxes in a contrary direction to the atmospheric pressure, and communicates with an indicating lever. The registration is effected on a cylinder which revolves by means of an ordinary clock; it makes a complete revolution in a week, and carries a glazed paper, which is covered with lampblack by being held over the flame of a candle; the extremity of this lever, very fine and pointed, traces a line of variations in a white streak. The periods (4 times a day) are represented on the diagram by vertical lines, and the barometric readings by horizontal lines placed a millimètre apart, the arm of the indicator being so arranged as to mark the variations on the same scale as a common mercurial barometer. This instrument has none of the errors of the common aneroid barometer resulting from the great number of pieces, levers, articulations, gearing, connecting chains, and springs.

A new application of paraffin has been made by M. Monnet for lubricating machinery. The great difficulty was in procuring a lubricating substance that would not melt at a lower temperature than from 300° to 400° Centigrade, and cheap enough to be employed at Lyons on a large scale. Now, the class of paraffins furnishes a substance called melen ($C_{40}H_{60}$), insoluble in water, soluble in fatty oils, volatile without decomposition, and only boiling at above 370°, while at the ordinary temperature it has the consistence of wax, and floats freely on water. Its degree of softening at the temperature of the hand, from 15° to 20° C., is already sufficient to form between surfaces in contact a thin sheet of melen, and according as the heat increases the substance becomes softer, until it acquires a complete liquidity which is uniformly kept up.

The following are the advantages arising from paraffin or *melen* lubrication:—

1. During the working of the machine the lubricating substance is very fluid, oily, and unalterable. The melenic particles, carried by the steam, clot together on the surface of the condenser, and can be removed without difficulty.

2. When the motion has ceased the paraffin remains fixed and becomes solid much quicker than lubricating oils commonly in use, which are fluid at ordinary temperature.

3. When the machine is set in movement, the paraffin adherent to the surface to be lubricated is melted at once, while the steam gives its heat to the mass of metal of the receptacle before it acts upon the piston. The high temperature of the elastic fluid soon equalises the temperature, and the fusion of the paraffin takes place.

F. Moigno.

Paris, Nov. 12, 1867.

Improvements in Automatic Telegraphy.

Since the 11th September, 1867, the Directors of the telegraphic lines have made use, in the service between Paris and Lyons, of a new system of rapid transmission invented by MM. Chaudassaignes and Lambrigot, telegraph clerks. This telegraph acts automatically, transmitting the despatches between the two towns at the rate of 120 or 180 despatches per hour by a single conducting wire, a velocity three times as great as that obtained by other systems, and capable of being augmented proportionately to the diameter of the wire.

The transmissions are made by a band of metallic paper on which the signals composing the despatch are traced in insulating ink. The reproduction is obtained on a band of unsized paper, the centre portion of which is impregnated with a chemical liquor necessary for the formation of the characters existing on the metallic band.

In order to obtain regularity of execution in the different operations, such as the composition, transmission, and reception, they pass through several hands according to the requirements.

One instrument in communication with the line is composed of—1. A clock-work movement. 2. A double roller which sets at work either the metallic or the chemically prepared paper. 3. A ringing apparatus for calling the attention of the correspondent. 4. A "Morse" manipulator of ordinary construction for the exchange of the conventional signs necessary for setting in movement or stopping the rollers.

The clock-work movement is set at work by a weight easily wound up by means of a pedal; it serves to maintain the rollers in movement. Near the roller round which the metallic band passes, is a point which represents the extremity of a conducting wire. The roller communicates with the electric pile. When the band is drawn into movement by the rotation of the roller, the point is placed sometimes on one of the metallic parts of the band, and sometimes on the written parts of the despatch where the isolating ink is, so that the conducting wire marks the message by the alternate passage, and breaking of the current. Near the roller, on which is coiled the unsized paper, is placed a cup filled with a solution of nitrate of ammonia and ferrocyanide of potassium. In the middle of this cup is a small roller which dips into the liquid in its lower portion, and the upper portion of which rises a little higher than the edges of the basin and supports the band of unsized paper which, drawn by the rotation of the two rollers, turns the small dipping roller and becomes impregnated with the solution.

A point of iron representing, like that of the metallic band, the extremity of the conducting wire, leans, slightly inclined, resting by its own weight upon the damp paper band, and is in communication with the earth. The voltaic current decomposes the wet portion, and leaves a coloured deposit which represents the signals of the despatch.

The working of this apparatus is entirely mechanical. The transmission and the reception of the despatches take place automatically; one clerk superintends the machine. In order to compose the despatches into conventional signals on the metallic band, another instrument, called the compositor, is employed, similar to that of Morse, the signals of which are employed. The band of metallic paper unrolling itself is raised by a lever so as to touch a thick roller covered with a resinous preparation in fusion, which cools suddenly as soon as it is applied to the metallic band. One clerk can prepare alone 35 to 40 despatches per hour; the telegraphic staff acquainted with the Morse apparatus can, without any study, compose despatches. For the service between Paris and Lyons three compositors suffice completely for the transmissions. The despatches reproduced on the band of chemically prepared paper are handed over to other clerks, who translate them for the printed despatches distributed to the public.

The result is that two composing clerks, two translating clerks, and a superintendent of the machines of reception and transmission, do as much work by aid of a single conducting

[English Edition, Vol. XVI, No. 414, pages 243, 244; No. 415, page 256.]

wire as six clerks with three wires by the ordinary telegraphic system. A composing apparatus furnished with electro-magnets has been established on a line from London to Paris. When the *employé* in London wishes to transmit a telegram to Paris for the Lyons line, the only line in which this rapid service is installed, he manipulates as for the ordinary transmissions of the Morse apparatus; the letters or conventional signs are printed on a metallic band, and a few seconds afterwards are transmitted to the chemically prepared paper.

Thus we have before us a great improvement in modern telegraphy. Up to 11th September last the service of the Lyons line was carried on by aid of two or three Hughes' apparatus; each apparatus occupies two clerks and three batteries. By the new system five clerks do all the service with one line only. The new system works admirably and without a single hitch, and we can affirm that the invention of MM. Chaudassaignes and Lambrigot is destined to render great service to the telegraphic service. The economy of installation, and the saving effected in the number of clerks, the maintenance, wear and tear, &c., are marvellous.

By decree of the 16th October, M. Bertin-Mourot was named director of the scientific studies of the Normal College, in the place of M. Pasteur. M. Balard was named inspector-general of the order of sciences in place of M. Dumas. M. Pasteur undoubtedly will become professor of chemistry at the Faculty of Sciences of Paris in place of M. Dumas. Under the name of Bertin-Mourot the learned world will recognise M. Pierre Auguste Bertin, the skilful and zealous physician who has left so happy a *souvenir* in the Faculty of Sciences of Strasburg.

F. Moigno.

Paris, Nov. 19, 1867.

Curious Terrestrial Globe.—Organisation of the Imperial Observatory.—Report on Mr. Fryer's Concretor.—New Optical Instruments.

During the revolution of 1793 a curious terrestrial globe existed in the royal Chateau de Bellevue. Becoming national, it was sold and bought by one named Testu. The latter person, after having lost his fortune, and not having room for it, deposited it in the Royal Library of the Rue de Richelieu. Later, M. Jomard, conservator, engaged M. Sanis to purchase this curious work, assuring him that, after being restored, the Government would purchase it for one of the museums. M. Sanis treated with M. Testu, and M. Jomard put him in possession of this globe so remarkable in an archæological point of view, and he has had it since 1846. The globe has remained, after being restored, so as to have lost nothing of its primitive character, in the hands of M. Sanis, who wishes to yield it to a museum in any country. It can be seen at No. 22, Rue des Fossés, Saint Jacques, Paris.

As to the origin of this globe, the works of Guache gave the idea to Edme. Montell of constructing a globe which would represent at the same time the natural and political divisions of the earth. In order to accomplish this double project, the inventor proposed to trace on an ordinary globe, three feet in diameter, all the details of the inequalities of the surface of the globe. The King, Louis XVI. (so says the "Universal Biography of Milan"), ordered the execution of this project at the expense of £1,200, defrayed by his Majesty.

This instrument consists of—1. A great outer globe divided into two hemispheres. The convex portion gives the political geography, and the concave portion the state of the heavens. 2. A globe in relief, a mètre in diameter, rolls in the interior of the first, and represents continental and submarine mountains, basins, etc.

The decree for the organisation of the Imperial Observatory, dated January 30, 1854, contained the following:—"Every two years, at least, the minister receives a report stating the actual scientific situation and requirements, drawn up by a commission composed of two members of the Admiralty Board, a member of the Institute, two members of the *Bureau des Longitudes*, an inspector-general of superior instruction, and the director of the observatory. This excellent arrangement has remained a dead letter, the result being the enormous abuses whose extent can be known from the following facts. 1. The number of titular astronomers, calculators, and clerks, etc., who have passed through the Observatory without remaining in it between the years 1854 and 1867, exceeds a hundred. 2. Among those who have quitted the observatory there are many celebrated scientific men. 3. Several titular astronomers, the salaries of which ranged between £240 and £360 per annum, do not perform any regular service. 4. That the amount of the salaries was fixed in the most arbitrary manner. 5. That the salaries of many astronomers were kept back even more arbitrarily. 6. That the future career of 6 or 7 pupils, of great merit, proceeding from the Ecole Normale of France, was seriously compromised by the engagements towards them not being carried out, etc. The minister of public instruction, being struck by this state of affairs, has named a commission to report on the state of the Observatory. Among the questions to be debated is the demolition of the present building and the construction of a new one.

M. Dubrunfaut has just made a report on his examination of the products obtained by the concretor of Mr. Fryer of Guadaloupe. He states that the mass is sufficiently ripe, and the crystals sufficiently detached to allow of refining processes being adopted, either by moulds or by the turbine. Its colour is inferior to the 4th class of good quality; and, observing the facility with which the syrup is displaced in the mass, he asks if the specimen which has arrived in Europe is the same as that which left Guadaloupe, or, in other words, if it has not been subjected to some purification which had sensibly altered its constitution. This species of cooked mass, in fact, does not appear homogeneous, and he thought it useful to reduce it to fragments, so as to mix them all together, and thus form a mass representing the true average.

The reporter first examined a colonial sugar classed as good 4th (No. 12 of the samples); the results are as follows:—

	Percentage.	
Saccharimetric	91·35	
Sugar (uncrystallisable)	3·65	
Water	3·63	
Ash corrected by the factor, 0·9	0·85	
Organic and non-saccharine matters	0·55	
		100·00

The return of this sugar by refining is accomplished in the following manner:—Admitting 3·73 for the saline co-efficient, as we also do for the indigenous sugar, and taking unity for the glucosic co-efficient, as several refiners of Paris adopt—

$$0{\cdot}85 \times 3{\cdot}73 = 3{\cdot}17 \text{ sugar}$$
$$3{\cdot}65 \times 1{\cdot}30 = 3{\cdot}65$$

Sugar remaining in the molasses = 6·82

Sugar (refined).... 91·35 − 6·82 = 84·53

Thus we have—

Crystallisable sugar	6·82	15·47
Glucose	3·65	
Water	5·00	
Refined sugar		84·53
		100·00

As the molasses are delivered by the refiners at 42° Beaumé, and at 50 per cent. in all sugars, the return in molasses would be greater. But the difference compensates the loss, and the refiners ordinarily count upon cent. per cent. of return.

We can estimate the value of the sugar, in refining, as follows:—

	f.	c.
84·53 loaf sugar at 1·25 f. the kilo	106	66
15·47 molasses at 26 f.	4	02
	110	68

To be deducted

Duty on No. 12, less the drawback of 39f. Expenses of refinery 9·75 the 100 kilos., taking into account the return in molasses—8.25	47·25
Leaving........	63·43

for the value in refinery, not including the profit of the refiner. This profit, given by the refining of ordinary sugars, can sometimes attain 8 or 10 fr. per 100 kilos. of the refined sugars, whilst the profit upon the white grained sugar is often negative.

The following is the result of the examination of the Fryer concreted sugar:—

Crystallisable sugar as ascertained by the saccharometer....................................	78·00
Uncrystallisable sugar (Barreswill process)	9·10
Ash	2·80
Water	7·70
Other organic matters........................	2·40
	100·00

The returns of the sugar in Parisian refinery, calculated on the same basis as the good 4th above mentioned, give the following results:—

2·80 ash × 3·73	10·4 sugar
9·10 glucose × 1	9·1
Total.....	19·5

sugar contained in the molasses.

Thus we have, as a maximum, in molasses........	42
Extractible sugar..........................	58
	100

This return may be valued in Paris (not including the profit of the refiner) as follows:—

	fr.	c.
58 kilos. of loaf sugar, at 1·25 fr..........	72	50
42 kilos. of molasses..........................	10	92
Total........	83	42
To be deducted, we have		
Small duty, less the drawback, 37fr. Expense at 20f. the 100 kilos. refined, 11·60 f.	47	60
Remaining fcr the value of the sugar.......	35	82

As a like produce would give relatively four times more molasses in refining than the average actual process, it would soon encumber the material with low produce, and would thus hamper the normal production. Several Paris refiners refuse systematically the purchase of every sugar producing more than three per cent. of dross, that is to say, more than twenty per cent. of molasses. What is to be done with those which give more than 40 per cent.? It is probable that, in such a case, they would not consent to pay for this produce more than twenty to twenty-five francs the 100 kilos. in depôt, although this sugar can render by refining the 35·82 francs above calculated.

If we wish to know, after the composition of concrete sugar and good 4th, what would be the return in concrete sugar No. 4 and molasses, we arrive at the following result:—

Extractible sugar calculated............	58 per cent.
Molasses	16
Or in good sugar No. 4................	74 per cent.
In Colonial Molasses..................	26
	100

The above 74 kilos. are worth, in Paris, 46·9 francs, to which must be added the value of 26 kilos. of colonial molasses; considering that the 74 kilos. of good 4th have paid less freight than 100 kilos. of Fryer concrete sugar, it is plain that there will be more profit in making good ordinary or white sugar at the Antilles, than in making the Fryer concreted sugar.

In fact, in a fiscal point of view, 100 kilos. of Fryer concrete sugar having paid a duty of 37 francs, and only producing in reality 58 per cent. of refined sugar, the 100 kilos. of refined sugar thus produced are burdened with a tax of 63·80 francs, while the loaf sugar is only valued at 47 francs.

The same calculation applied to the good 4th does not burden the 100 kilos. with a tax exceeding 43·13 francs, that is to say, 16·67 francs less than the sugar of the concretor. M. Dubrunfaut does not recommend the adoption of the Fryer system into the French colonies, where the sugars are poor in refined sugar and rich in molasses, but he thinks it well adapted for the Antigua sugars. The sugars imported into England have given the following analysis in 100 parts: crystallisable sugar, 87·79; fruit sugar, 6·00; refuse, 0·81; water, 4·40; organic matter, 1·00. A similar sugar assayed in Paris by the refiners gave on the same bases as those we have given above furnished: extractible sugar 78·7, molasses 21·3 in 100 parts.

M. Robert Houdin has just published a very interesting pamphlet on new instruments suitable for the observation of the different organs of the eye, also the manifestation of entoptic images. These are the names given by him to the shadows thrown on the retina by intra-ocular bodies.

Seven instruments of this class have been invented by M. Robert Houdin; these are: 1. The *Iridoscope*, for the manifestation of entoptic images; 2. The *Diapscope*, by the aid of which the inversion of the images on the retina are determined; 3. The *Pupilloscope*, demonstrating in a magnified form the dilations and contractions of the pupil; 4. The *Pupillometer*, which gives the diameter of the pupil to within a quarter of a millimètre; 5. The *Diopsimeter* for measuring the extent of the field of vision; 6. An *Optometer* for the use of any persons who wish to determine the distance of distinct vision; 7. The *Retinoscope*, an instrument with which one can see the vascular group, in his own eye.

F. Moigno.

REPORTS OF SOCIETIES.

ROYAL INSTITUTION OF GREAT BRITAIN.

On some Experiments of Faraday, Biot, and Savart, by John Tyndall, Esq., LL.D., F.R.S., Professor of Natural Philosophy, R.I.

The discourse was delivered at the request of the excellent President of the Royal Institution. The speaker had no new discovery to make known, and the utmost he could hope to achieve was to give a few old discoveries in such a form as would interest an intellectual audience.

A few of the more striking phenomena of electro-magnetism were first exhibited by means of a helix and a core of soft iron. The question arose, "suppose that core to be transparent, what would be the effect of its magnetisation upon a beam of light passing through it?" Probably such a question presented itself to the mind of Faraday. But iron was not transparent, and our great experimentalist had to seek long before he found a transparent substance, which enabled him to demonstrate the action of magnetism upon light.

Light in its natural condition was not sensibly affected by magnetism. The speaker then defined and illustrated, by means of a Foucalt's prism and a plate of tourmaline, the action of "plane polarized light." He then showed the chromatic phenomena produced when a plate of rock-crystal, cut at right angles to the axis, was placed between the polarizer and analyzer of a polariscope. He also defined and illustrated, by means of the tourmaline, what was meant by the plane of vibration and the plane of polarisation.

A plate of quartz, composed of two semicircles, the one belonging to a right-handed and the other to a left-handed crystal, was placed in front of the electric lamp, and through the plate was sent a beam of plane polarized light. The beam then passed through two perforated masses of iron, which rested on the two ends of a powerful electro-magnet—these pieces of iron were in fact the movable poles of the magnet. Beyond the furthest pole was placed a Foucault's prism, through which the beam also passed, being finally received upon a white screen. A lens was introduced between the circular plate of quartz and the magnet; and by this lens a magnificent image of the plate of quartz was thrown upon the screen.

The speaker showed the changes of colour produced when the plane of polarization was caused to rotate. Bringing, for example, the entire image of the quartz plate to a delicate puce, the slightest rotation of the Foucault's prism coloured one of the semicircles a vivid red and the other a vivid green. Restoring the puce colour, and placing a bar of the heavy glass with which Faraday first demonstrated the action of magnetism upon light from pole to pole of the magnet, the beam was transmitted by the glass, and the image upon the screen was unchanged.

On now exciting the magnet, the uniformity of the colour disappeared; one semicircle ran rapidly into a vivid red, the other into a vivid green. The relative position of the colours changed when the direction of the current was changed: when the current was interrupted, the puce colour was restored. Thus it was proved that the act of magnetization produced the same effect as the mechanical rotation of the plane of polarization; and this is the celebrated experiment which Faraday described as the magnetization of a ray of light.

The beautiful experiment of Biot on the influence of sonorous vibrations on plane polarized light was next thrown into a form which allowed the whole audience to see the effect. A rectangle of glass, 6 feet long, 2 inches wide, and about ¼ of an inch thick, was clasped by a clamp at its centre, being so placed between the polarizer and analyzer that the beam crossed the glass rectangle near its centre. The polarizing prisms were placed so as to darken the field of view. A sweep of a wet cloth over the distant half of the glass rectangle brought out its tone, and immediately a luminous disc a yard in diameter flashed out upon the screen. Every sweep of the cloth threw the glass into sonorous vibration and illuminated the screen.

A plate of selenite was so placed between the polarizer and analyzer as to show a system of vividly coloured rings. By a suitable arrangement of the experiment, the colours were wholly obliterated when the glass rectangle was thrown into longitudinal vibration.

None of these effects could be produced when the polarized beam passed through the rectangle near one of its ends; for here, as is well known, the necessary strains and pressures were absent.

The remaining experiments had reference to the action of sonorous vibrations upon jets of water. A vein was discharged obliquely from the nipple of an ordinary gas burner. The vein broke into scattered drops. By the light of the electric lamp, a dense shadow of the vein was thrown upon a white screen; on sounding an organ pipe, or a tuning-fork of the proper pitch, the drops suddenly gathered themselves together, forming an apparently continuous band several feet in length. On the suspension of the sound the drops broke asunder as before. The minuteness of the vibration, which is competent to produce this effect upon the vein, is extraordinary. After a tuning-fork had ceased to be heard, when placed against the support of the nipple from which the vein issued, the drops gathered themselves together, and remained in coalescence long subsequent to the apparent subsidence of the motion.

A jet of water was permitted to descend vertically. Its two portions, the continuous and the discontinuous, were described. An arrangement was devised by which the vein was vividly illuminated from above. The continuous portion was of dazzling brilliancy; the point of rupture being thus rendered strikingly manifest. On sounding the proper note, the continuous vein shrunk almost up to its aperture. The effect of beats was very fine; as they addressed the ear, the lengthening and shortening of the luminous cylinder, in perfect synchronism with the beats, went on. Here also the amount of motion, if only of the proper quality, which influences the vein, may be infinitesimal; the vein, in fact, declares the existence of the beats long after the ear has ceased to hear them.

MANCHESTER LITERARY AND PHILOSOPHICAL SOCIETY.

Ordinary Meeting, October 15th, 1867.

J. P. Joule, LL.D., F.R.S., etc., *Vice President, in the Chair.*

"*Note on the Occurrence of Sulphocyanide of Ammonium in Gas Mains*," by Peter Hart, Esq.

A few months ago while some gas mains in the street were being replaced, I was induced to examine the scale which forms in the interior, being under the impression that probably I should find sulphide of iron the result of long-continued action of sulphide of hydrogen on the iron. On placing a portion of this scale in pure hydrochloric acid I perceived an intense reddening, much more than would be accounted for by the simple solution of peroxide of iron, and being aware of the fact of sulphocyanogen being one of the products of the distillation of coal, I at once suspected its presence. A portion of these scales was boiled in water. The clear filtrate from this gave off much ammonia on the addition of alkali, and on the addition of a dilute solution of perchloride of iron it gave at once the intense colouration so characteristic of the sulphocyanides. The insoluble portion remaining on the filter was then boiled to dilute caustic soda; the filtrate from this made acid, and a solution of ferric oxide again added, this time with the production of a blue precipitate indicative of a ferrocyanide. This must have existed as ferrocyanide of iron, which on boiling with the alkali became oxide of iron and ferrocyanide of sodium. There appears to me something curious in the fact of these bodies being carried such a distance (in this case fully a mile from the gas works) by the gaseous current. I should think the ferrocyanogen is the result of a reaction between the sulphocyanogen and the metallic or oxide of iron. The amount of these bodies must, I think, be far too small to have any bad effect on the health of gas consumers.

"*Jupiter as observed at Ardwick on the night of August 21st, 1867*," by J. B. Dancer, F.R.A.S.

The somewhat rare phenomenon of Jupiter without a visible satellite extraneous to his disc (as the Rev. W. R. Dawes has correctly designated it) has been described in the *Astronomical Register.* I am not aware, however, that any notice of it has appeared before this Society, and as few observers appear to have enjoyed such favourable atmospherical conditions as we did in this locality, I am induced to offer a few remarks on the appearance which Jupiter presented on the night of August the 21st.

During the early part of the evening the S. and S.E. portions of the heavens were covered with thick haze; between 8 and 9 o'clock this gradually disappeared, and the atmosphere became clear and unusually favourable for astronomical observations. Preparations had been made for noting the times of the phenomena, but from various circumstances this was abandoned. The observations were made with an achromatic telescope of 7¼ feet focus and 6¼ inches clear aperture. Powers employed, 90, 175, and 300. At 9 o'clock Jupiter was clearly defined and presented a remarkable appearance. On the broad south belt three spots were distinctly visible, and at the same time three satellites appeared off the disc. The first spot in the order of transit was very black—this was the shadow of the third satellite. The next spot was brown in colour when contrasted with the belt—this was the third satellite; and the third spot, which was the shadow of the fourth satellite was very dark and well defined. At this time the fourth satellite appeared very faint in comparison with the first satellite, which was just below it. The second satellite, which was on the west edge of the planet, was soon eclipsed, and attention was entirely directed to the fourth satellite, which was the next to transit. As this satellite entered on the disc it was visibly brighter than the planet; after a short time it disappeared, and then, to my surprise, it became visible again as a dark spot, and at intervals it was as black as its own shadow. The alteration in the colour of this satellite during its transit has been noticed by several observers, but I was not prepared for such a complete change from a bright object to one as black almost at times as an ink spot. During these observations the first satellite had entered on the disc of the planet, and when it was about half its diameter on the planet it appeared to me to be in contact with its own shadow. After a short time I could only distinguish the first satellite at intervals, closely following its own shadow. Before the third satellite passed off the disc it became nearly invisible, and when close to the edge of the disc it appeared brighter than the surface of the planet. The fourth satellite was not observed at the end of its transit.

Mr. Robert Worthington, F.R.A.S., observed some of the phenomena with me, and remarked at the time that he did not recollect having seen Jupiter so sharply defined as on that evening. The bays and various markings on the belts were most beautifully distinct.

"*Notes on some Superficial Deposits at Great Orme's Head, and the Period of its Elevation,*" by R. D. DARBISHIRE, B.A., F.G.S.

MICROSCOPICAL AND NATURAL HISTORY SECTION.

October 7th, 1867.

J. B. DANCER, F.R.A.S., *President of the Section, in the Chair.*

The President, in his address to the members of the Section, described the various additions and improvements which had taken place in microscopes and apparatus, and gave a summary of the microscopical researches which had been communicated to various societies, both English and Foreign, during the past year.

The following extract of a letter dated 27th of August, 1867, from Captain Mitchell of Madras, was read:—

One or two things have come under my notice, which a friend who arrived in Madras from London a short time since, said he believed were unknown to microscopists in England; I therefore send you a brief notice of them, in the hope they may interest some of the members of the Society.

The first is the presence of ciliated infusoria in dewdrops on leaves. I have to confess I took the subject up in jest, in consequence of a remark in an article published in one of our local papers; about sunrise I placed an animalcule cage under the point of a leaf and transferred the drop of dew gathered there to the glass plate, and then examined it with the microscope. I repeated the experiment on two other occasions, and the result was, that in about one drop out of every two I found one, and sometimes two infusoria.

As the dew did not begin to fall until midnight, they must have been produced from the germ between that time and 6 a.m.

On the first occasion I saw also what I took to be one or two spores of fungi. I supplied the cage with distilled water and put it by until the next morning, when I found a perfect forest, which continued to multiply so long as I supplied it with distilled water, which I did for several days. It will be interesting to ascertain if infusoria are found under similar conditions in England.

Some time ago Mr. Ross sent me a binocular body for my stand (one of his father's make). I have in my cabinet the tongues (so called) of two house flies, which I had mounted some years back. I was always under the impression that the divided absorbent tubes were enclosed between the two membranes that form the upper and lower surface of the two lobes, and I believe that is the general opinion of their structure; now on placing one of these specimens under a half-inch glass with the binocular body, I was not a little astonished to see the tubes standing out above the surface of the membrane on the lower or under part of the tongue.

I was always at a loss to understand the use of these very curious vessels, but it now seems evident that fluids may be taken up by them and conveyed to the larger central tube into which they all run. It is I presume known that these tubes all open on the upper surface of the lobes by the other and narrower extremity.

Mr. LATHAM read the following communication on Silk-producing worms from Natal:—

In the *Natal Herald* of the 8th August last, there are copies of a correspondence between the Chamber of Commerce of this place and some gentlemen at Natal regarding certain silk-producing worms, as they are termed, found near Graham's Town by Mr. Hillier, feeding on the leaves of the mimosa thorn or acacia.

The result of the correspondence was, that some of the cocoons were presented to the Chamber here to have them reported on and their true value ascertained.

They came into my hands, and the following remarks on them may be interesting to the Society:—

From one of the cocoons the moth now exhibited emerged shortly after its arrival in England, and though much crippled, I have, through the kindness of Mr. Jansen, had it clearly identified with the insect "Pachypasa effusa," of which there are several specimens in the British Museum collection from Natal.

The moth laid about 50 eggs, of which I have mounted two or three, and they are here for examination.

The eggs under the microscope exactly resemble in texture those of the ostrich, but each has a small black point, probably of a softer substance than the rest of the egg, and through which the caterpillar may emerge.

From one of the cocoons I extracted the chrysalis also exhibited, and further the cast skin of the caterpillar rolled into a small ball as usual; by boiling this for some time in caustic potash, it became so softened that it was possible to get it to its original size, and to show its original form. You have it before you dried, and the series is therefore complete, egg, caterpillar, chrysalis, moth, and cocoon.

The original cocoon is, you will notice, a hard woodylike substance, but by certain processes, Mr. Hillier states soaking in a solution of soda, the cement agglutinating the silk is dissolved and a soft silky-looking bag remains.

This consists of a thick outer covering, a loose middle lining, and a thinner internal lining, all of silk, which I hardly think could be wound, but might possibly be carded.

As regards the commercial value of the article, an eminent silk broker writes as follows:—

"It is a carded cocoon; the waste silk is the outer covering of the cocoon and appears of tolerable fine fibre, but is bad in colour and not of a good merchantable appearance, if in quantity worth perhaps 1s. 6d. per lb. all round.

"Enquiries of a similar nature have been made from time to time by customers in the East, where silk does not form one of the staples of the country, and among other places, from Natal, but we do not find that any good can arise from efforts made to produce silk in any quantity; it is probably the most difficult of all known staples to establish in a new land."

Ordinary Meeting, November 12th, 1867.

EDWARD SCHUNCK, Ph.D., F.R.S., etc., *President, in the Chair.*

Mr. John Barrow was elected an Ordinary Member of the Society.

[English Edition, Vol. XVI., No. 413, pages, 229, 230; No. 417, page 277.]

Mr. Binney, F.R.S., F.G.S., said that in the Rev. J. G. Cumming's excellent "History of the Isle of Man" that author, at page 132, says, "the different layers of the Posidonian schist vary both in their lithological texture and organic contents. The finest and most compact layer which is worked for ornamental purposes, is characterized by an abundance of Posidonia and the relicts of tree ferns, which we must necessarily regard with interest as indicating an approach, though still at a considerable distance, from the coal formation of Great Britain." As the discovery of fossil tree ferns in the mountain limestone would be of great interest, he had lately been over to Poolvash Bay, the locality named by Mr. Cumming, and spent a considerable time in searching the black limestones there for those fossils; but although he met with plants, the remains of vegetables common to the carboniferous formation, such as *Stigmaria ficoides*, *Calamites*, and other coal plants, he found nothing resembling tree ferns. The state in which the roots (*Stigmariæ*) were found in this limestone led him to believe that Mr. Cumming had mistaken them for tree ferns. The depressed areo'æ in this fossil have the lower portion of the radicle attached to them so as to give the appearance of a scar not much unlike that of a tree fern, but he convinced himself that they were unquestionably *Stigmaria*. This portion of *Sigillaria* is often met with in the beds of limestone in the Yordale series of Professor Phillips, as well as much lower down in the carboniferous series in the limestone of West Calder, near Bathgate, Scotland, where it occurs in great abundance. He also stated that the lower coal seams of Scotland containing workable beds were of considerably earlier date than the Posidonian schists of the Isle of Man.

Dr. R. Angus Smith, F.R.S., said that he had frequently visited some parts of the Continent, and had been a student of exhibitions such as included the useful arts. He was quite prepared to agree with those who saw a very great improvement in the touch of the workman, in the countries nearest to us, in the north of Europe especially. In France the advance made was very great, and so also in Germany; but in neither of these cases did he consider that the exhibition showed the true state of the arts among the people. A true exhibition would give the proportion of bad and of good manufactures. There was no attempt to do this except in cases where the objects were viewed rather as curiosities, as for example, when costumes and architecture were introduced. When a number of knife-makers showed their manufactures, it was clear that they all showed their best, but they did not show how many made knives that would scarcely cut, and even if they had done so, it would not have been sufficient to exhibit the state of the arts among the people. It would not inform us how many of the population had no knives set down to them at dinner, and how many houses had not a fork of any kind. In one of the countries which had made greatest progress and showed beautiful work of all kinds at the Exhibition, he had been in a hotel where no fork whatever existed, and when he asked for a knife, the landlady handed him one from her pocket. Yet she and the landlord were very respectable-looking people, and the house was clean. True, it was only in a village, but it was only a few miles from a considerable town. The people were not the poorest, but probably the richest in the village. He had not found any similar case in Great Britain. These were decent people of the working classes in want of what we may consider the ordinary tools requisite in modern civilisation only three years ago. He did not wish to speak of any manufacture for the study of which be may have had special opportunity, but, speaking generally, he did not think that the modern changes had penetrated all classes of the community so deeply in France and Germany as the Exhibition represented, whereas England had done more than it showed at the Paris Exhibition. This is quite independent of the question which is best, and relates only to the useful arts.

It is clear, however, that wonderful advances have been made, and who makes them? He considered that they were made by the upper classes of the manufacturers, and used by the upper and middle classes, but had not descended universally even to the middle classes. When the general descent takes place, the manufacturers will have a much larger home market than they have at present. It is for the interest of England that this advance among the people should take place, as in some instances it will prevent competition in foreign markets; but whatever the commercial result may be, there is one lesson which we may all learn. Within the last thirty or forty years, the violent attempts to teach the people here by schools, mechanics' institutes, and lectures given or promoted by benevolent persons attaching themselves to various societies, have wearied the souls of all who have co-operated or even looked on with interest. In Germany, without any commotion, calmly and pleasantly, the youths have been trained in schools and colleges without number, and so thoroughly that they are able to supply foremen and managers to their own manufacturing establishments, and to send a supply also to foreign countries, without diminishing the supply of that higher order of men of learning that have so long made Germany famous. In other words, whilst we have failed after the most violent efforts and much noise to teach our own, they have succeeded not only to teach their own citizens, but to assist in educating the rest of the world. In this matter of education, the governments have been able to mould the nation's mind, and to alter the habits of the leading portion of it in a very few years. A careful education would probably show its influence in less than ten years. True, many questions arise; and we may ask if education would not smooth down the peculiarities of the national character, and prevent it trusting to individual will and genius. Pedantic education might do this, but training is certainly as requisite in civil as in military affairs, and no one says that discipline diminishes the power of an army. It may be a question whether the uniform organisation of education in France is not calculated to produce too much equality in the minds of the nation, but it will certainly produce immense power in the aggregate. The numerous small states of Germany, each fostering its own schools and universities, seem best fitted to produce an intellectual activity. We see there many universities, each developing its own peculiarities. They must make the nation more many-sided, and they have done so. At any rate, the present lesson seemed to be that the more intelligent part of a nation may be fashioned anew in a few years by its instructors as easily as a boy may be taught to be either a shoemaker or a tailor; cases of original bent excepted. We desire schools in this country, but cannot find them; there is no organisation for making them to the extent they are required. The genius of this nation, great as it really is, impatient of details, and looking vigorously to results, will never compete in a wild state with the disciplined army of thinkers and of manufacturers abroad. Those who do succeed here must train themselves, and to that training they owe their success, but it is hard work, and not to be expected of many. However, it is clear that an entirely new spirit may be made to animate society in the course of a single generation, and a nation may be born in our own day; also, as the result has been, so far as the useful arts are concerned, exactly that which the rulers desired, we must believe that governments or large combinations of men have the change in their power.

It was said that this progress in the arts did not necessarily go down to the whole of society. There may be several reasons for this: the upper classes may be far advanced and the lower very poor, even when there is much good feeling. In some of the mining districts of Germany, sending out the most intelligent miners to distant parts for centuries, and teaching their own with great care in schools which have been imitated, but not surpassed, we find a population extremely poor. Nature has presented little to them. We cannot expect all farmers to be equally rich, when soils differ so much. In such cases, however, we can expect a careful superintendence and a thoughtful mode of husbanding resources, mitigating the evils of poverty, and producing content where otherwise abject misery would exist, and that we find.

The Exhibition shows how much may be done for the active minds of nations by a government fostering education, and the state of the same countries shows that intelligence, comfort, and wealth have been promoted also. Whilst the poorer parts alluded to, in Saxony for example, show that when from natural causes wealth has not been accumulated, education has produced intelligence to mitigate those evils which would otherwise have crushed the people. This education is owing to the activity of the governments. We learnt the lesson in this island once, and forgot it. We must be humble enough to learn it again from the world, instead of teaching it to them as we ought to have done.

The unassisted working classes in this country have raised themselves to an enjoyment of the products of civilisation not equalled probably anywhere in Europe, and their progress has been of longer duration; as we love our country we are still disposed to believe in its power of keeping in advance. But how could a regiment, however brave, advance with longbows against modern artillery?

We require education in the fundamental principles of physical science; the moral principles and teachings found in literature are not, when alone, sufficient either for the higher cultivation of every mind or the pursuit of the useful arts. This applies to the rich and not merely to the poor.

ACADEMY OF SCIENCES.

OCTOBER 21, 1867.

(FROM OUR OWN CORRESPONDENT.)

The Newton-Pascal Forgeries—Volcanic Phenomena—Discovery of the Law of Gravitation—Theory of Solar Spots—Determining the Longitude—Formation of Crystals of Gypsum.

SIR DAVID BREWSTER addressed to M. Chevreul, in terms of great friendship, a new letter against the authenticity of the autographs of M. Chasles, which he calls the most audacious imposition of modern times. This attack of our illustrious and venerable friend gives us great pain, and the more so as it is reduced to a series of assertions immediately confuted by incontestable facts.

1. "King James II. could not have written to Newton from Saint Germain on the 16th January, 1688, as he only arrived at the end of 1688." In the *Comptes Rendus* of the Academy, p. 551, the date of 1685 is evidently an error, as the second letter, posterior to that of 12 January, 1689, could have been no other than that of the 16 January, 1689.

2. "How could James II. have written to Newton, whom he knew to be an anti-Jacobite, and one of the conspirers to upset the throne?" In the letters of M. Chasles we read, in fact, that Newton was very embarrassed to have to answer King James; that the King reassured him, saying that in repelling him from the throne he only did his duty.

3. "Newton could not have accused Flamsteed of the disagreements which called forth the injurious words against Descartes and Pascal inserted in the letter to Huyghens, since at that period Newton and Flamsteed were friends." All the correspondence known of this period, manuscript or printed, shows that Flamsteed always complained of Newton's treatment towards him.

4. "Who can believe that Louis XIV. would have occupied himself with an incident so insignificant as two or three disdainful words uttered against Descartes and Pascal?" Authentic autographs of Louis XIV. prove, as Sir David could have seen by the note of General Morin, that the great king occupied himself with the change of garrison of a detachment of 25 to 30 men. Louis XIV. was also much occupied with the injury done, by Newton, to our two illustrious countrymen. There are mixed up in this affair not only James II and the Abbé Bignon, but Huyghens and the celebrated astronomer Bouillaud, whom he made his confidential adviser in matters relating to science and savants, by whose medium he negotiated the recall into France of Huyghens and Cassini. M. Chasles has in his hands the letters by which Louis XIV. asks Bouillaud to make a report on Newton's affair, invites Huyghens to return to Paris to give explanations on this subject, and excuses himself towards James II. for the susceptibility he had manifested on this occasion in indicating the duties of a sovereign towards his most illustrious subjects. Thus, we see, the assertions of Sir David Brewster are promptly refuted; the timid and embarrassed style of Newton's letter to the King of France is revolting to him. M. Chasles, besides the rough copy of the letter of Newton, possesses the answer to Newton on which we see the words "*Vu bon*" in the well known handwriting of Louis XIV. Evidently the ground is no longer tenable by the adversaries of the authenticity of the autographs of M. Chasles. Every objection raised against it becomes an argument in its favour. M. Chasles has indicated, without been obliged to do so, the source of these documents,—the cabinet of Desmaizeaux; it is now known that they were purchased by the Chevalier Blondeau de Charnage; that two English gentlemen, Messrs. T. Winthrop and W. Robertson, wished to purchase them for 40,000 francs. It is even impossible that there does not remain among the papers of Newton's family, in the possession of the Earl of Portsmouth and the Earl of Macclesford, traces of this negotiation and of the relations between Pascal and Newton. We know also the greatest number of the portfolios from the cabinet of Desmaizeaux are in London, probably at the British Museum, since Desmaizeaux died in London; it is necessary to find them, and thus furnish evident proofs of the absolute authenticity of the treasure of M. Chasles.

M. Fouqué, charged with studying in a chemical point of view the phenomena of the volcanic irruption of the Azores, which took place at the beginning of last June, writes that he has succeeded, not without great trouble, in collecting enough gas, rising from the bottom of the sea, to make the analysis, and to ascertain that it is entirely free from carbonic acid, and rich in oxygen.

M. Babinet fixes the year 1670 as the epoch of the definitive discovery, by Newton, of the laws of universal gravitation. It was only then that he finished the calculation of the fall of the moon upon the earth. At the moment when he foresaw the result of his calculations conformable to his previsions, he was obliged through illness to get them finished by a friend.

M. Kirchhoff and M. Faye agree upon the theory of solar spots, which the latter attributed to an opening in the solar photosphere, allowing the kernel to be seen, the emitting power of which is inferior. M. Faye continues to deny the possibility of the transmission of light through the nucleus.

M. Faye announced that the new method of determining the longitude by M. Littrow, has given the best results.

M. Drouke, professor of Chemistry at Coblentz, read a note on the gradual formation in a mass of clay of crystals of gypsum, some of which were 14 centimetres long.

F. MOIGNO.

OCTOBER 28, 1867.

Osmogenic Extraction of Sugar.—The Pascal-Newton Forgeries.—Electro-Capillary Researches—The Nerves of Nerves.

M. PAYEN read a note on the employment in sugar refining of the osmogene apparatus completed by M. Dubrunfaut, in order to separate from the sugars the crystallisable salts.

The molasses, which refuse to crystallise, contain in general fifty per cent. of crystallisable sugar, half of which is recovered by the osmotic process. Osmosis is more profitably applied for eliminating the salts of the syrups obtained by the forced straining of the first and third portions of the crystallisation, which yield more syrup, and thus give results calculated to economise the apparatus crowded together in the works.

"M. Dubrunfaut," adds M. Payen, "has also found a method for the assay of raw sugars, which indicates not only the total quantity of sugar, but that of the mineral salts. One part of the saline residue of incineration corresponds, in the mean, to the formation of 7·45 of molasses containing 3·73 of sugar uncrystallisable as long as salts are present. This method is generally adopted at the present time.

A member of the Zollverein, relying upon the authority of

Dr. Shreiber, recently declared that he had found by experiment that the salts in molasses, especially the nitrates and chlorides, did not hinder the crystallisation of the sugar. But in studying directly and separately the influences of the alkaline nitrates and chlorides, M. Payen has arrived at the following conclusions: nitrate of potash in various proportions does not hinder the crystallisation of sugar, chloride of potassium exercises an influence in making the crystallisation slower, and chloride of sodium acts in the same way, but more powerfully.

November 4, 1867.

M. Arthur Chevalier deposited a sealed packet containing the description of a new apparatus.

Sir David Brewster, responding to the appeal of M. Chasles, has asked Lord Portsmouth, Lord Macclesfield, and the Director of the British Museum, if in the collection of autographs in their possession, there were not some traces of relations between Pascal and Newton, and the efforts made by Messrs. Winthrop and Robertson to purchase from the Chevalier Blondeau de Charnage the compromising documents which M. Chasles now possesses. The answer of Lady Macclesfield is negative; that of Lord Portsmouth has not yet come to hand, but Sir David, who has had for a long time in his hands the autographs which he possesses, does not fear to affirm also to the contrary. The Director of the British Museum recognized that his collections possess in fact a great number of documents from the cabinet of M. Desmaizeaux; that they form four great volumes, but that no correspondence is found between Newton and Pascal. Sir David, as we have foreseen, rushes into the conclusion that Desmaizeaux is the forger himself—Desmaizeaux, the intimate friend of Newton, who would have done him an injury, while he refused to Fontenelle, for his eulogium of Newton, and to many others, the documents relative to the relations between Newton and Pascal!

M. Chasles answered that he is glad of the declaration of the Director of the British Museum, for the comparison between the two series of manuscripts will certainly prove the authenticity of many of his documents, as, for example, those of Liebnitz, and he mentioned that all these papers are not from the Desmaizeaux collection. Many are from Msme. de Perier, Dreux du Radier, Madame de Pompadour, etc.

M. Chasles presented also four series of photographs taken by reflected or transmitted light. Four of these autograph letters demonstrate invincibly, according to the artist (M. Murcit), an ancient pupil of the Saint Barbe College, that the ink on the paper has really as ancient a date as the water mark, and corresponds to that on the letters.

M. Becquerel, sen., read an additional note to his electro-capillary researches. He shows definitely that—1. The alteration exerted on the sides of the capillary spaces between two liquids. 2. The electricity disengaged at the contact of these liquids in the capillary spaces. 3. The electric conductibility of the sides covered with liquid. He has happily modified his method of experimenting. Instead of forming the fissures in the tubes, he fastens at their extremity a strong stopper very tightly fixed, made with filtering paper soaked in water; a platinum wire traverses the stopper and connects the two liquids together.

M. Trecul answered at great length, and we think victoriously, the objections made by the celebrated botanist, Shultze, to his observations on the laticeferous organs of plants.

M. Peligot read a *résumé* of his very long researches on the part played by soda in plants.

M. Charles Robin announced that M. Sapey had discovered the nerves of nerves, *nervi nervorum*, the existence of which was well known, but not well observed. He examined by a microscope the mucous membrane, and found that they formed round each nerve so many fibrous nerves that enclosed a canal in which the nervous pulp was lodged.

M. Robin presented also, in the name of M. Blondeau, Professor of the Laval Lyceum, the result of the experiments made relatively to the action of induced electricity on the seeds of plants.

M. Edmond Becquerel communicated some curious experiments of M. Bouchotte, of the electrolytic power of the currents of the magneto-electric machine of the Alliance Company. When the current sent by the commutator is always in the same direction, the electrolytic power is that of 144 Daniell elements with sulphate of copper; but when the current is alternate, as in the production of the electric light, the electro-motive power is *nil*.

M. Duchartre communicated with great praise the curious and interesting experiments of M. Joseph Balsamo, Professor of Physics at the Royal Lyceum, Lecce (Provincia de Otranto) Italy, on the production by hybridation of new sorts of cotton. He has succeeded in the fecundation, one after the other, of long-staple, short-staple, etc. He has obtained interesting varieties, which, if they multiplied, would render great service to the industrial world. One of the principal aims of M. Balsamo was to create a species of cotton the maturity of which would be more advanced, and which would be proof against the autumn rains, which in the south of Italy form one of the greatest obstacles to the indigenous culture of cotton.

F. Moigno.

November 11, 1867.

Vacant Academic Chair.—The Imperial Observatory.—Functions of the Roots of Vegetables.—The November Meteors.—Parallax of the Sun.

In a letter to the President M. Dubrunfaut requested the Academy to insert his name in the list of candidates for the vacant chair in the section of rural economy, and promised to submit to it the numerous titles which he possesses for the honour which he solicits. He will be received, we are sure, with open arms as one of our most eminent practical chemists. M Dubrunfaut at an early age imbibed a taste for agriculture, and worked at its elementary practice. Taking into consideration that the great source of the prosperity of France is the industrial development of the soil, M. Dubrunfaut made himself a manufacturing chemist. Every one knows the immense progress he has made in the production of beet-root sugar and alcohol; the benefits to be reaped by the application of osmose and the purification of beet-root juice and molasses will be counted by millions of francs.

M. Elie de Beaumont commenced the reading of three long letters relative to the autographs of M. Chasles, two from Sir David Brewster, and one from M. Grant. The Academy decided that they should be inserted in the *Comptes Rendus*. It is high time, however, to finish this discussion, and M. Balard begged of M. Chasles to cease answering these assertions, and M. Chasles promised to publish all the original documents.

M. Leverrier called attention in a long note to the inconveniences experienced by the observatory, owing to the new streets and constructions in the neighbourhood of the Saint Jacques quarter.

M. Corenwinder read a memoir on the functions of the roots of vegetables. It has been long known that the roots possessed the property of absorbing carbonic acid. M. Corenwinder proves on the contrary by his experiences that if these organs are put in communication with a certain proportion of this acid, either in a gaseous state or in solution in water, it is found that the quantity present in the roots is greater than that which had been supplied to the plant.

November 18, 1867.

MM. Coulvier-Gravier and Chapelas communicated the following note on the falling stars of November. The first great appearance of this phenomenon noted dates from 1766; the second, of 1799, was observed by MM. Humboldt and Bonpland. Consequently, if these apparitions are truly periodical, these two observations furnish a period of 33 years. But, since 1799, we must arrive at 1833 for the observation of a similar phenomenon which has served as basis for the calculation of Olbers, by which he thought he could prove that the period of the phenomena of November was definitively 34 years, and that the first return would take place in 1867. Now, we are forced to note that the illustrious astronomer had not made a correct statement, for this year, though the moon was bright and the atmosphere foggy, we were able to ascertain the presence of only a veritable minimum. Last year the apparition was very beautiful, though inferior to 1833, and many observers watched impatiently the return of the phenomenon, relying on the theory of Olbers. Now the period has arrived and all observers have been able to state that the expected phenomenon of November was not produced. The solution of this problem must be postponed for some years.

This year M. Le Verrier had organised the observation of the falling stars of November. He had them observed at several places, but the height of the moon above the horizon had rendered observation almost impossible. At Limoges the sky was overcast, and in Paris too brightly lit up by the moon. They only observed before two o'clock a few meteors; from 2h. 48m. to 5h. 42m. the number of meteors seen in the moonlight was constantly on the increase. In the last hour before daylight the number was 25. This gives reason to believe that the maximum occurred last year. M. Wolff observing that a great many did not come from the constellation Leo, concludes that the periodical meteors are unconnected with the sporadical ones.

The Academy proceeded to the nomination of the commission for drawing up the list of candidates for the chair rendered vacant by the death of M. Civiale; the Section is composed of two members of the Section of Mathematics, MM. Mathieu and Becquerel; two members of the Physical Science Section, MM. Lecaisne and Longet; two free Academicians, MM. Leguin and de Verneuil; under the presidency of M. Chevreul. The chances are in favour of Dr. Larrey.

M. Chasles made a further communication on the subject of the Pascal-Newton forgeries.

M. Chasles also communicated the translation of a memoir of an American astronomer, M. Simon Newton, with respect to the parallax of the sun. He gives it at 8·56; M. Leverrier adheres to 8·95, a value which agrees with that determined by M. Foucault.

F. Moigno.

November 25, 1867.

Paralysis caused by Santonine.—Safety in performing large Amputations.—The "Just Susceptibility" of Marshal Vaillant.

M. Eugene Pelikan, director of the civil medical department of Russia, etc., presented a note on the local paralysis produced by saponine and analogous substances. He summed up the results of his experiments as follows: 1. Saponine and similar substances produce a local paralysis, followed by a rigidity of the muscles, and also paralysis of the nerves of sensation; 2. With regard to this local paralysing action, there exists an analogy between saponine and substances acting upon the pupil, such as atropine, physostigmine, etc.; 3. Saponine, now employed in medicine, is probably destined to perform another part than that at present attributed to it, and for this reason it should be submitted to new clinical experiments; 4. That saponine does not cause either contractions of the muscles or of other parts to which it is applied, and that it annuls completely the irritability of the muscles (even rendering them rigid) submitted to its action, provided that the animal is in its normal state of health and is in possession of all its functions.

Dr. Maisonneuve, surgeon of the Hôtel Dieu, read a paper on the continuous method of aspiration and on the advantages for the healing of great amputations. In a recent work presented to the Academy he explained that:—The numerous and febrile accidents which render complex the greater number of wounds, and which constitute the principal danger of surgical operations, are always the result of poisoning. The liquids exuding from the surface of the wound become morbid in contact with the external air, and poisonous putrefaction at once ensued, and the author came to the conclusion that the liquid at the surface of the wound could be hindered from putrefying and that great surgical operations, such as amputations, etc., could thus always be performed with safety to the life of the patient. The process recommended by Dr. Maisonneuve consists in submitting the stump of the limb amputated to a continuous aspiration, which draws off the liquids secreted by the wound according as they are formed, and to transport them to a recipient before they have time to putrefy. The following is the method employed: After having as usual stopped the flow of blood, by means of the ligature of the vessels, the wound is cleaned most carefully; it is washed with alcohol and dried with clean dry linen; the edges are gently united by means of bands of diachylon plaster, but without hindering the flow of the liquids; a layer of lint is then laid on, saturated with antiseptic liquids such as tincture of arnica, aromatic wine, or any other analogous substance, and the extremity of the limb is bound round with cloth soaked with the same liquid. After this preliminary dressing the process of the aspiratory apparatus is brought to play. The apparatus is composed of: 1. A sort of burette of caoutchouc furnished with a tube of the same substance. 2. A flask of three or four litres capacity; 3. An air-pump which exhausts by means of a flexible tube.

Dr. Guerin read a memoir on the same subject, viz., Pneumatic Occlusion of Wounds, and he claims priority for his process. His apparatus consists of a very stout glass receiver with three tubular openings, one at the top and two lateral. That at the top leads to a dynamometer of very simple construction, a graduated glass tube terminated by an india-rubber ball filled with mercury. If the pressure diminishes, or a vacuum takes place in the balloon, the india-rubber bag dilates, and the level of the mercury lowers in the tube, thus giving the variations of the pressure. The inventor is convinced that by his method the expense of hospital dressings and the dangers of operations will be much diminished. He has shown us his apparatus, which we have much admired, and he informs us that, at the Hôtel Dieu, Dr. Maisonneuve had obtained wonderful results with his apparatus, which, after all, is only a modification of that of Dr. Guerin, to whom belongs exclusively the idea of the application of pneumatic occlusion to wounds, amputations, etc. We were eye-witnesses to the efficacy of Dr. Guerin's valuable instrument.

The Academy of Sciences has lost, for the time being, one of its most learned and honourable members, Marshal Vaillant, whose bland and noble countenance was one of the principal ornaments of the meetings. A just susceptibility has kept him at a distance, for more than six months, from his companions, for whom he was always ready to render a service, and to whom he was much attached. A favourable occasion has presented itself of bringing him again to the vacant chair which he had deserted. He has been named member of the commission charged with presenting a list of candidates to fill the place of free academician, vacant by the death of M. Civiale.

F. Moigno.

QUEKETT MICROSCOPICAL CLUB.

The monthly meeting of this Club was held at University College on Friday evening last, Oct. 25 (Mr. Arthur E. Durham, President, in the chair).

Mr. S. J. McIntire read a paper on "*Chelifers*," in which he gave some interesting facts with regard to the haunts, habits, and mode of capture of these curious animals, resembling minute scorpions and having the backward and sideway motions of crabs. Of the 54 known species 8 are British, and are chiefly found under the bark of trees, and in houses amongst old papers, etc., often rendering good service by feeding on the insects which are usually so destructive in old libraries. Several living specimens were exhibited under the microscopes, where their activity in the pursuit of their prey was conspicuous.

A paper by Mr. C. Nicolson, M. A., "*On Object Glasses for the Microscope*," was read.

Nine members were elected.

CHEMICAL SOCIETY.

Thursday, November 7.

Warren de la Rue, Ph.D., F.R.S., *President, in the Chair.*

At this, the first meeting after the summer recess, there was an unusually large attendance of Fellows, and a full programme of interesting matter was provided. The proceedings were opened as usual by reading the minutes of the last meeting, and announcing the contributions to the Society's library. Mr. Henry Dircks, C.E., was formally admitted a Fellow of the Society, and the name of Charles Meymott Tidy, M.B., of the Hollies, Cambridge Heath, Hackney, was read for the second time. The names of the following candidates were proposed for election:—Thomas Hall, B.A., Lond., Lecturer on Chemistry and Natural Philosophy at the City of London School; Charles Walter Maybury, Teacher of Chemistry, 90, King Street, Manchester; George Lunge, Ph.D. (Breslau), 10, Albert Terrace, South Shields; Facundo J. R. Carulla, Chemist to the Atlas Steel and Iron Works, 59, Gell Street, Sheffield; and Alexander Crum Brown, M.D., Lecturer on Chemistry, 4, Rillbank Terrace, Edinburgh.

The **President** stated that the Council had appointed a sub-committee to consider and report upon the mode of election of Fellows into the Society, and to canvass the opinion of the members as to the qualifications necessary for the attainment of the honorary distinction which the Society has it in its power to confer. A great number of replies had been received in answer to the circular which the Secretary issued in June last, and the committee would at an early date be prepared to advise the general body of members of the results of their enquiry.

The **President** further stated that a melancholy event had occurred since the last meeting in the death of Professor Faraday. Several of the Fellows shared his own feelings in this matter, and considered that the sad occasion demanded a special notice on the part of the Society, of which the deceased was one of the earliest and most distinguished members. It was now proposed to present an address of condolence to the widow, framed in the following terms:—

"Resolution—That the Fellows of the Chemical Society request their President to convey to Mrs. Faraday their deep sense of the loss which science has sustained in the death of her highly distinguished and much esteemed husband, and that they beg respectfully to express their heartfelt sympathy with her in this great loss."

Mr. W. H. **Perkin** was then invited to read a paper "*On the Action of Acetic Anhydride upon the Hydrides of Salicyl, Ethyl Salicyl, etc.*" The author endeavoured to obtain the hydride of aceto-salicyl by acting upon the hydride of salicyl with acetic anhydride, but the expected reaction did not take place. There resulted from the direct union of these bodies a white crystalline substance, fusible at 103°—104° C., and insoluble in water. An analysis of this compound led to the formula—

$$C_{11}H_{12}O_5 = C_7H_6O_2,\ C_4H_6O_3.$$

By a similar action between the same anhydride and the hydride of ethyl salicyl a body crystallising in small, brilliant, transparant prisms was formed, which fused at about 89° C. Its composition was—

$$C_{13}H_{16}O_5 = C_9H_{10}O_2,\ C_4H_6O_3.$$

The methyl salicyl compound was easily formed. Its fusion point was 75° C.

The author has likewise investigated the corresponding compounds in the benzoyl series, and arrived at results in general accordance with the previous statements of MM. Geuther, Hübner, and others; but the author does not accede to the view expressed by the latter, which asserts that the body produced by the action of acetic anhydride upon the hydride of benzoyl is identical with the diacetate of benzoyl of M. Neubauer. On the other hand, this class of compounds, like those of the preceding series, are but further examples of the same kind as the compound of acetic anhydride and ordinary aldehyde discovered by Geuther; and not identical, but only isomeric with the acetate of ethylene of Wurtz.

After a few words of explanation had been given in reply to Dr. Odling's inquiry as to what was intended by an expression made use of in the concluding paragraph of the author's paper, to the effect that "the phenolic properties disappear," the President invited Mr Chapman to give an account of the "*Nitrous and Nitric Ethers,*" samples of which were upon the table.

The authors, Messrs. Chapman and Smith, commence with a detailed account of the method adopted by them in the preparation of the nitrites and nitrates of amyl ethyl, and methyl, and in the second place describe a considerable number of reactions and decompositions which the ethers in question undergo on treatment with metals, acids, and sundry chemical reagents in a digestion apparatus. The most remarkable feature of the author's communication appeared to consist in an easy mode of preparing the nitrate of amyl in large quantities. This was shortly as follows:—Nitric acid of specific gravity 1·36 was mixed with twice its bulk of oil of vitriol, and allowed to cool; of this mixture 150 c.c. was placed in a beaker surrounded by iced water, to which is added a little salt to reduce the temperature one or two degrees below zero. 50 c.c. of amylic alcohol were now gradually added from a small dropping funnel the limb of which reached nearly to the bottom of the mixed acids, and kept constantly stirred by the motion of the funnel itself, six or eight minutes being occupied in this process of admixture. No apparent action takes place beyond the production of an oily layer upon the surface of the mixed acids, which is removed by means of a separating funnel and washed in three or four changes of water. 100 parts of amylic alcohol yield by this process within five per cent. of the theoretical quantity, or about 144 parts of the nitrate of amyle instead of 151. After rectification over chloride of calcium a colourless liquid is procured, which boils at 147°—148° C., and at the temperature of 7° or 8° C. has the same gravity as water. Hence there is an advantage in using warm water for the purification of the crude acid product. The inhalation of the vapour of this substance is to be guarded against, for it invariably produces headache and other distressing symptoms. The general result of the author's investigation is to show the great stability of the nitric ethers and the extraordinary chemical mobility of the nitrous ethers; bodies of the latter class are not, however, to be considered as unstable, since they exhibit no tendency to spontaneous decomposition.

The **President**, in moving a vote of thanks to the authors, referred to the interest attaching to the ready means of preparation which had been described. There was another light in which these investigations would prove valuable; namely, by pointing out facilities for obtaining some of the secondary products by simple reactions.

Mr. **Robert Warington**, jun., then gave an abstract of an exhaustive agricultural research undertaken for the purpose of determining "*the Part taken by Oxide of Iron and Alumina in the Absorptive Action of Soils.*" Experimenting with the artificially prepared hydrates of alumina and ferric oxide, as well as with two samples of native soil containing widely different amounts of the same ingredient (or rather, "oxide of iron and alumina," 6·82 and 19·31 per cent. respectively), the author tried the effects of passing solutions of tricalcic phosphate, alkaline carbonates and sulphates, ammonium salts, etc., through them for the purpose of ascertaining the rate and extent of absorption. Inasmuch as the calcareous constituents in the natural soils would have interfered with the actions which it was now intended to observe, these matters were first removed by digesting in weak acetic acid and thoroughly washing with water. The soil thus purified was left for several days in contact with a carbonic aqueous solution of the tricalcic phosphate, a current of carbonic acid gas being occasionally passed, and after the lapse of a week the ferruginous soil was found to have withdrawn 93.8 per cent. of the phosphoric acid originally present in the solution and only 49 per cent. of the lime, hence the author believes that the ferric oxide and alumina may be considered to possess a special affinity for this mineral acid, and that all the phosphoric acid applied to land in the shape of manure must ultimately become converted into the phosphates of these bases. If the amount of iron be sufficiently large, all the phosphoric acid will be retained by preference in the form of ferric phosphate. The absorption power of soils for potassium salts was found to be much greater in the instances of the phosphate, sulphate, and carbonate, than with either the chloride or nitrate. The corresponding ammonium salts behaved in a similar manner. The author deduces from his experiments a general conclusion to the effect that the absorptive action of soils, for the constituents named, is dependent upon true chemical affinities, in contradistinction to the view which asserts it to be a consequence of the exercise of merely physical attractions.

In passing a vote of thanks to Mr. Warington, the **President** invited the opinions of the eminent agricultural authorities whom he saw in the room; for his own part he should incline to the belief that caustic lime, so largely applied by the farmer, would have a great influence in absorbing the phosphoric acid.

Professor **Way** conceived there was more difficulty in accurately ascertaining the degree of absorption of the carbonates of ammonia and potash than in the cases of the other alkaline salts named. Mr. Warington concluded that the oxide of iron and alumina absorbed more of these ingredients than did any other constituent naturally occurring in soils, but he would remark that the very existence of hydrate of alumina was at the outset a matter open to question. This earth was generally supposed to occur in the form of a double silicate, and many kinds of clay contained lime locked up in such a manner that it could not be extracted by acids; bodies of this class had the power of absorbing ammonia without any apparent change of a chemical character.

Dr. **Völcker** said his experimental results were mainly in accordance with the conclusions stated by Mr. Warington. He likewise had noticed the powerful absorption of phosphoric acid, and, in a less degree, potash, and ammonia by ferruginous soils. There was a kind of ferric oxide precipitated by lime, which behaved in an extraordinary manner as to the amount of phosphoric acid it could take up, and lime in a soil has great influence in absorbing ammonia. So also has hydrated silica, although the artificial preparation is in this respect much inferior to the silicates naturally occurring in soils; and, again, there were other additional constituents of which no account had been taken. There was a remarkable tendency in nature for "the soil to take care of itself," and if there should happen to be a deficiency of any one ingredient this was quickly remedied by prior selection from out of a mixture of materials presented in the form of manure; thus the affinities were regulated by bulk, and the land seemed to avail itself of those constituents of which it stood most in need, and the double silicates were in this respect pre-eminently fitted to absorb ammonia, etc., as was first pointed out by Professor Way.

Dr. **Gilbert** said that many years ago Mr. Ronalds and himself pursued a somewhat similar line of research, and although he was not prepared to endorse all that Mr. Warington had advanced, his paper possessed merit inasmuch as it established certain points; but there was a question as to the applicability of these results to soils as they really exist. The speaker agreed with Dr. Völcker in believing that soils have almost an instinct to guide them as to what they should or ought to do.

Mr. **Warington** briefly replied by asserting that when the soil contains lime in addition to the ferric oxide these bases act more freely but in the same direction. With regard to the absorption of phosphoric acid by a ferruginous soil it was only a matter of time as to how large a proportion was combined; humus appeared to be capable of absorbing free ammonia, and so it must be admitted that each constituent will require to be studied singly.

"*Analysis of the Water of the Holy Well, a Medicinal Spring at Humphrey Head, North Lancashire,*" by Thomas E. Thorpe, Dalton Scholar in the Laboratory of Owen's College, Manchester. This paper was read by the Secretary, and described the composition of a brackish water issuing from the rocks on the northern shore of Morecombe Bay. Its specific gravity is 1005·8, and constant temperature 11·5° C. The results are stated both in grammes per litre and grains per gallon; the latter column only is here quoted.

	Grs. per Gallon.
Barium sulphate	0·0329
Strontium sulphate	0·2912
Calcium sulphate	88·4898
Potassium sulphate	9·1749
Sodium sulphate	24·3971
Magnesium bromide	0·0294
Magnesium iodide	traces
Lithium chloride	0·1414
Sodium chloride	331·7524
Ammonium chloride	0·0231
Magnesium chloride	43·4882
Calcium phosphate	0·0266
Calcium fluoride	traces
Calcium carbonate	9·2029
Ferrous carbonate	·2191
Manganous carbonate	0·0168
Silicic acid	1·2341
Organic matter	traces
	508·5199

A paper entitled "*On the Action of Permanganate of Potash on Urea, Ammonia, and Acetamide in strongly Alkaline Solutions,*" by Messrs. J. A. Wanklyn and Arthur Gamgee, was read in abstract by the first-named author. Referring to the anomalous reaction observed on boiling various organic substances with permanganate of potash and excess of alkali by Chapman and Smith, the authors treated the bodies named in the heading with the same reagents under a variety of circumstances. Amongst others, the following experiments were made—

	I.	II.
Urea artificial	·1 grm.	·1 grm.
Permanganate of potash	1·0 "	2·0 "
Caustic potash	10·0 "	10·0 "
Water	10·0 "	12·0 "

I. Heated in sealed tube for 12 hours at 130° C. II. Heated for one hour at 160° C.

Results.—In the first experiment rather less than half the nitrogen of the urea appeared as nitrogen gas; about half was oxidised to nitrites or nitrates, and a small portion was found as ammonia. By increasing the permanganate, as in II., there was little or no oxidation; nearly all the nitrogen being liberated in the form of gas. When much weaker solutions were employed, and the materials introduced into a common retort and distilled, there was a slow evolution of ammonia, but the total quantity of nitrogen eliminated in this form, and estimated by Nessler's test, did not amount to one-fourth of that existing in the urea.

Ammonia, similarly treated with liberal amounts of potash and permanganate in pressure tubes, was completely changed into nitrate. The same result, or a nitrate, was observed in operating upon acetamide.

From a consideration of the differences in the behaviour of urea and ammonia, etc., under the action of the permanganate, the authors refuse to accept the common view that urea is carbamide; they prefer to write its formula on the marsh-gas type, thus—

$$C\left\{\begin{matrix}NH\\NH_2\\OH\end{matrix}\right.$$

Professor **Wanklyn** also read a paper entitled "*Verification of Wanklyn, Chapman, and Smith's Water Analysis on a Series of Artificial Waters.*" The author deemed it desirable to place on record the results of a series of synthetical experiments undertaken for the purpose of testing the accuracy of the method of analysis lately proposed. Fresh albumen (white of egg) was dissolved in water with the aid of a little carbonate of soda, and diluted to make a solution containing one per cent. Various measures of this solution were diluted to half-a-litre with pure water, and submitted to distillation, first with the simple addition of ·5 grm. of carbonate of soda, when it was found that mere traces only of ammonia passed over in the first 100 c. c. of the distillate. At this stage, caustic potash (about 10 grms.) and permanganate of potash (·5 grm.) were added, and the distillation proceeded with. The total amounts of ammonia thus procured, and estimated by Nessler's test, accorded with the quantities of albumen taken, or at least within the limits of 5 per cent. error. The proportion of ammonia obtained is never the total quantity, but always that corresponding to two-thirds of the nitrogen contained in the albumen; and the authors base their calculations on the fact that 1 part of moist white of egg yields ·0121 of ammonia. Seven experiments of this kind were quoted by the author, performed upon quantities of albumen varying between 7 and 41 milligrammes. The estimation of urea by the same method was not nearly so accurate, and when pure materials were employed, no ammonia, or a mere trace only, was evolved; but when the urea occurs along with "albuminoid matter" in a natural water, the surrounding impurities start the action, and enable the operator to obtain much of the ammonia (·37 out of ·46) by long boiling with carbonate of soda. The addition of alkaline permanganate to a known quantity of urea did not induce the evolution of the whole of the nitrogen in the form of ammonia. (See also experiment recorded in the previous communication.)

At a late hour a vote of thanks was passed to the authors of the several communications, and the titles of papers in hand were announced. "*On the Pyro-phosphoric Amides,*" by Dr. J. H. Gladstone; and "*The Relation between the Results of Water Analysis and the Sanatory Value of the Water,*" by Mr. E. T. Chapman. The meeting was then adjourned until Thursday, 21st instant.

Thursday, November 21.

WARREN DE LA RUE, Ph.D., F.R.S., *President, in the Chair.*

THE minutes of the previous ordinary meeting were read and confirmed, and the donations to the library announced.

The PRESIDENT then read a letter of acknowledgment written by Mrs. Faraday in answer to the address of condolence which was forwarded to her agreeably to the resolution passed at the last meeting. The names of candidates proposed were—Mr. Alfred E. Fletcher, Inspector of Alkali Works, Johnston, near Prescot; and William Frank Smith, M.D., Lond., Lecturer at the Sheffield School of Medicine, Glossop Road, Sheffield. For the second time were read the names of Thomas Hall, B.A. Lond., Lecturer on Chemistry and Natural Philosophy at the City of London School; Charles Walter Maybury, Teacher of Chemistry, 90, King Street, Manchester; George Lunge, Ph.D. (Breslau), 10, Albert Terrace, South Shields; Facundo J. R. Carulla, Chemist to the Atlas Steel and Iron Works, 59, Gell Street, Sheffield; and Alexander Crum Brown, M.D., Lecturer on Chemistry, 4, Rillbank Terrace, Edinburgh. The name of Charles Meymott Tidy, M.B., of the Hollies, Cambridge Heath, Hackney, was read for the third time.

The PRESIDENT gave notice that the next meeting of the Society, December 5, would be made general for the purpose of considering and taking action upon the proposal to alter the first by-law relating to the election of Fellows. The law at present stands thus:—

"Every candidate for admission into the Society shall be proposed according to a form of recommendation (No. 1, appendix) subscribed by three Fellows of the Society, to one, at least, of whom he should be personally known, and such certificate shall be read and suspended in the Society's rooms, or place of meeting, for three ordinary meetings."

It was now proposed to require the names of five members (instead of three) subscribed on the form of recommendation, to three of whom (instead of one) the candidate should be personally known. With respect also to the printed form of recommendation, it was suggested to modify the phraseology to "name;" and (in second line) "qualification or occupation, if any."

Mr. E. T. CHAPMAN made a statement on "*The Relation between the Results of Water Analysis and the Sanatory Value of the Water.*" Much has been said and written upon the proper method of performing water analysis, but comparatively little attention has hitherto been given to the interpretation of the results, and thus it happens after determining the ammonia, nitrates, phosphates, etc., the reporter usually appends to his analysis an opinion stated somewhat as follows:—"This water is perfectly harmless;" or, on the other hand, "exceedingly deleterious." The speaker's object was to fix a standard according to which the chemical quality of any given sample of water was to be judged. A good drinking water should not contain any appreciable amount of ammonia; lime salts communicate "hardness," but are not directly injurious to health; water containing nitrates in solution, but otherwise pure, was also harmless; if, however, these several ingredients occurred together in a water, the conditions were favourable to the development and growth of the lower forms of vegetable life, and such water kept in a cistern quickly assumed purgative properties. Mr. Chapman has made experiments upon pigeons, and found that when the birds were supplied with water of the last named quality, they were purged almost to death, and when the water was changed to a pure sample, or such as contained any one of the above ingredients alone, they soon rallied. These results were confirmed by experiments on the human system, and the author argued the necessity of observing the relations between the several ingredients in a water before pronouncing upon its sanitary value. The extended use of Clark's process was recommended as a means of removing the most objectionable forms of organic matter contained in waters holding in solution the carbonate of lime. A small proportion of impure water, such as that drawn from certain pumps in the City of London, was sufficient to start the decomposition and render unwholesome large bulks of Artesian water with which it was mixed. The quality of the water raised from the well in the Bank of England was such that, after storage in tanks, the colour became quite green, and the vegetable confervæ accumulated to such an extent as to choke the delivery pipes. Considerable amounts, both of ammonia and nitrate, existed in this water.

The PRESIDENT referred to the well water in Golden Square, which bore so bad a character in times of cholera visitation, as an example of a perfectly clear water having a pleasant flavour without any sign of desmidieæ or other species of vegetable organisms. This water contained nitrates and carbonate of lime, but its deleterious character was supposed to be due to decomposable organic matters existing in solution.

Mr. DUGALD CAMPBELL said there were a great many wells about London supplying water pleasant to the taste, and containing 30 or 40 grains to the gallon of nitrates and nitrites. When an outbreak of cholera occurred the wells were closed. In Lincoln's Inn Fields were two water mains, marked S and H (*i.e*, *soft* and *hard*); the first was New River Water, and the other possessed a degree of hardness amounting to 50 of Clark's scale, and contained a large quantity of nitrate. Instead, however, of its being purgative the use of the water had the opposite effect.

Dr. STEVENSON had occasion to examine a supply of water of which cholera patients had partaken. He could not find in it any infusoria or other kind of pseudo-vegetable matter, nor products of their decomposition. The general opinion of physicians was adverse to the introduction and use of a water containing but a very small proportion of carbonate of lime.

Mr. SPILLER referred to the treatment of Kent water by Dr. Clark's process as now conducted at the Herbert Hospital, Woolwich. The softened water was found to differ in a remarkable manner from the original supply by its not permitting the growth of vegetable organisms.

Mr. CAMPBELL observed similar results many years ago with the softened water formerly supplied by the Plumstead Company.

Mr. **Chapman** said that Dr. Clark's process was still being carried out at Caterham, and that by its use fully six-sevenths of the nitrogenous organic matter present in a water might be removed by the precipitated carbonate of lime.

Professor **Abel** considered that the germinal inactivity and flat taste of the lime-softened waters were simply due to the removal of free carbonic acid. Organic matter was no doubt partially precipitated together with the carbonate of lime as a kind of lake, but that portion which remained in solution would eventually make its existence manifest when a re-absorption of carbonic acid from the air should have occurred.

The **President**, in moving a vote of thanks, took occasion to refer to a successful instance of well-sinking by the American system; whereby he obtained a supply of good water at the rate of fifteen gallons per minute. This was accomplished by driving into the earth 1½ inch pipes shod with steel, which could be lowered twenty feet into hard gravel in the short space of three hours, a weight, or "monkey," of 70 lbs. being let fall upon the upper extremity of the tube.

Dr. J. H. **Gladstone** then read a paper "*On the Pyrophosphoric Amides.*"

The author stated that in former communications he had described three acid bodies that may be viewed as pyrophosphoric acid, in which one, two, or three molecules of amidogen have displaced an equal number of molecules of hydroxyl. Their composition and their relation to the original acid may be thus exhibited:

Pyrophosphoric acid..............	$P_2H_4O_7$
Pyrophosphamic acid.............	$P_2(NH_2)H_3O_6$
Pyrophosphodiamic acid	$P_2(NH_2)_2H_2O_5$
Pyrophosphotriamic acid	$P_2(NH_2)_3HO_4$

Since these papers were written he has come across some additional facts, and has formed a more precise conception of the rational formulæ of these bodies.

Pyrophosphamic Acid.—This acid had hitherto been formed only by the breaking down of the higher amide, but the author gave reasons for believing that it might be prepared synthetically. When the pyrophosphate of an earth or metal is prepared in the presence of ammoniacal salts the precipitate when heated *per se* gives off ammonia and a peculiar sublimate, which is supposed to be characteristic of a pyrophosphamate. The ferric salt, however, differs from the ordinary ferric amate in being more soluble, and in being easily broken up by acids, and it was never obtained pure for analysis.

Pyrophosphodiamic Acid.—The author had previously published a characteristic test for this acid, founded on the fact that when a solution containing it is rendered strongly acid, and is heated with a few drops of a ferric salt, the flocculent white pyrophosphamate makes its appearance. But a chemist inexperienced in these compounds might easily be misled by the formation of the insoluble ferric pyrophosphate, especially if the solution is not very acid. Hence it will be generally desirable, if not necessary, to dry a portion of the precipitate, and examine which compound it is by heating it *per se* in a test-tube, when the pyrophosphate simply fuses, and the pyrophosphamate does not fuse, but turns black at first, and gives off ammonia and a little white volatile salt. Still, as the proof that this acid may be prepared by the eleven methods noted in his paper, rests mainly on the evidence of this test, the author thought it well to repeat the principal experiments, examining whether it was the amate that was really produced. He has found it to be so in all cases; and has no reason to doubt that in each instance it had been formed by the decomposition of pyrophosphodiamic acid.

Pyrophosphotriamic Acid.—The method formerly given for preparing this body was not a productive one, and the acid was apt to be contaminated with another compound, unless great attention was paid to the temperature. The following is a far more productive and a better process:—Saturate oxychloride of phosphorus with dry ammonia gas, without regard to the rise of temperature, heat the resulting mass at about 200° C., add water to it, and boil for about a minute. This will convert the whole of the insoluble portion into triamic acid, with very little loss from the production of other phosphoric compounds. This acid has also been met with among the products of decomposition of one of the tetraphosphoric amides that remain to be described at some future time.

Theoretical Constitution.—In his last communication the author suggested as the rational formula of pyrophosphoric acid, $P_2(HO)_4O_3$, or at greater length,

$$\left.\begin{array}{l}P(HO)_2O\\P(HO)_2O\end{array}\right\}O;$$

and he expressed his conviction that when this acid is produced by the mutual action of water and oxychloride of phosphorus, the two atoms of hydrogen in the molecule of water are attached simultaneously by two molecules of the chloride, and the water type is preserved in the new phosphorus compound.

The same principle was applied to explain the reactions by which these pyrophosphoric amides are formed, especially the symmetrical pyro-diamic acid: its rational formula will be

$$\left.\begin{array}{l}P(NH_2)(HO)O\\P(NH_2)(HO)O\end{array}\right\}O$$

The unsymmetrical pyro-amic and pyro-triamic acids will be respectively,

$$\left.\begin{array}{ll}P(NH_2)(HO)O\\P(HO)_2 & O\end{array}\right\}O, \text{ and}$$

$$\left.\begin{array}{ll}P(NH_2)_2 & O\\P(NH_2)(HO)O\end{array}\right\}O.$$

A vote of thanks having been passed to Dr. Gladstone, the President adjourned the meeting until Thursday, December 5th, when Mr. W. H. Perkin would read a paper "*On the Artificial Production of Coumarine and its Homologues.*"

The following report has been forwarded to every member of the Chemical Society:—

Sir,—At a meeting of the Council, held on May 16th, 1867, it was resolved, "That a Committee of five be appointed to consider the by-laws relating to the elections of Fellows, Honorary Members, and Associates, and to report to the Council." It was further resolved, "That the Committee consist of Mr. Crookes, Dr. Miller, Dr. Odling, Mr. Wanklyn, and Dr. Williamson."

Upon the presentation of the Committee's report, at a meeting of the Council, held on November 7th, it was resolved, "That this report be approved, and that a copy of it be sent to each Fellow of the Society."

We beg to append the report in question, and have the honor to remain,

Your obedient servants,
W. **Odling**,
A. Vernon **Harcourt**,
Hon. Secretaries.

"Your Committee were appointed by a resolution, passed at a Meeting of Council, held on May 16th, 1867, in fulfilment of the intention which the Council announced to the Society in its anniversary report.

"As bearing upon the standard of qualification for admission to the Fellowship of the Chemical Society, your Committee, from replies they have received to a circular which they addressed to all the Fellows, and from conversations they have held with different Fellows whom they chanced to encounter, have ascertained the existence among the Fellows of the Society of two very distinct views as to its nature and purposes.

"Many Fellows appear to regard the Society as being by rights an association of eminent scientific men; and they accordingly look upon the Fellowship of the Society as a distinction which should be conferred only upon those who have given evidence of marked chemical proficiency, as for example, by the production of some original memoir; so that the election of any one as a Fellow of the Society should stamp him at once as being a well-trained chemist and competent investigator.

"In favour of this view, it is urged that the initials F.C.S., appended to the name of any gentleman, seem to imply, that his attainments have won for him a public recognition somewhat in the character of a degree; and that these initials ought to signify, in reality, that which they seem to imply, and which is indeed their proper signification.

"It is further urged that the Fellowship of the Chemical Society is essentially an honorary distinction, although from the ease with which it can be obtained, practically by any who choose, it is a distinction but little valued by the better sort. It is, however, eagerly sought after and obtained by men who are not perhaps altogether desirable—who certainly have no claim to the title of scientific chemists—and who, in some cases, do not even join the Society from any interest they take in chemical science, but solely with the view of parading a distinction to which their merits do not really entitle them.

Moreover, from the circumstance that chemistry is pursued not only as a science but also as a profession and trade, the right to append the initials F. C. S. possesses a sort of trade value, exceeding its cost, to mere trading or professional chemists; as suggesting that those who have the privilege of using these initials are better qualified men than their brethren who are not thus distinguished.

"From these causes, it is said, the Fellowship of the Chemical Society has gradually sunk in public estimation; and accordingly it is very desirable that something should now be done to restore, if possible, its original prestige.

"On the other hand, many Fellows are of opinion that the Society is merely an association of individuals, having joint but various interests in the progress of both pure and applied chemistry; that the object for which the Society exists is not to confer honour upon any individual whatever, but to promote the general advancement, distribution, and application of chemical knowledge; and that, as a general rule, men engaged in pursuits more or less dependent on or connected with chemistry, and taking a sufficient interest in chemistry to wish to join the Society, should, unless personally objectionable, have every facility afforded them for joining it.

"In favour of this view, the preamble to the charter is adduced, and especially the following paragraph: whereas certain of our subjects 'did establish and are now members of a society known by the name of the Chemical Society, for the general advancement of chemical science, as intimately connected with the prosperity of the manufactures of the United Kingdom . . . and for a more extended and economical application of the industrial resources and sanitary condition of the community,' etc.

"It is further maintained that the Society, from its origin until the present time, has always been of a mixed rather than of an exclusively scientific character—that the present Fellows form quite as distinguished a body as have ever constituted the Society—and that many, at any rate, of the most distinguished individual Fellows do not feel themselves at all discredited by being associated as joint Fellows of the Society with men who are engaged or interested in chemical pursuits, but whose scientific or social position is inferior to their own.

"Moreover, of scientific as distinguished from purely professional societies, the Royal Society, it is urged, is the only one of which the Fellowship is conferred in recognition of eminent scientific merit—the special science societies being practically open to all students of and workers at their respective subjects, who may wish to be elected to their respective Fellowships. To limit the Chemical Society then to eminent scientific chemists would be tantamount to making it the chemical section of the Royal Society, instead of allowing it to have a distinct function and character of its own.

"It is further urged that the circumstance of chemistry being to some extent a profession, so far from indicating the propriety of making the Fellowship of the Chemical Society an honorary distinction, rather contra-indicates it. For, independently of the difficulty, or rather impossibility, of withholding or conferring the honour without doing much injustice to individuals, the Society, by professing to choose out the most worthy, would naturally be held responsible for its choice, and identified more or less with the acts of each and all of its Fellows.

"Your Committee having given these different views their best consideration, are not prepared to recommend any alteration in the by law relating to the election of Fellows, which would have the effect of confining the Fellowship of the Society to strictly scientific men.

"But they think it may be advisable, although they have failed to elicit evidence of the admission of any significant proportion of unsuitable persons into the Society, to make some modification in the present by-law, with a view to increase the security against the accidental election of undesirable candidates.

"They accordingly suggest that in future, or after a certain interval of time, the form of recommendation of a candidate, referred to in the first paragraph of the by-law in question, shall be required to be signed by five instead of by only three Fellows of the Society, of whom three at least instead of only one shall be required to sign from personal knowledge; and further, that in the second line of the printed form of recommendation, the words 'Qualification or Occupation' shall be substituted for the words 'Position, Profession, or Occupation.'

"At present your Committee are not disposed to advise any alteration in the second paragraph of the by-law, which requires three-fourths of the votes given to be in favour of the candidate, in order to effect his election. If, however, contrary to the anticipations of the Committee, any section of the Fellows should be found to make an improper use of this requirement, your committee would then recommend that one or other of two courses should be proposed by the Council and adopted by the Society; that is to say, that the by-law should be so altered as to render valid the election by a mere majority, or else that the by-law should be temporarily abrogated, and during its abrogation the election of Fellows be delegated by the Society at large to a Committee appointed for the purpose."

PHARMACEUTICAL SOCIETY OF GREAT BRITAIN.

Wednesday Evening, November 6, 1867.

G. W. Sandford, Esq., *President, in the Chair.*

The minutes of the preceding meeting were read and confirmed.

Several donations to the library and museum were announced, and the thanks of the meeting given to the respective donors.

Dr. **Attfield** made some remarks on a specimen of farina which had been forwarded by Mr. Palmer.

Mr. **Tilden** then proceeded to describe a new way of preserving the syrup of iodide of iron. He began by referring to the mode of preparing it, and also to the addition of iron wire, which had not been found to prevent the change it undergoes, after the syrup has been prepared. Diffused light accelerated the action of atmospheric oxygen; but if exposed to direct sunlight it became bleached again. Mr. Tilden had found that it could be preserved from contact with the air by a stratum of oil floating on the surface; but it was necessary to put the oil into the bottle before the syrup, and it was better if kept in a dark place. The syrup might be removed from the bottle by means of a tap.

Dr. **Redwood** said he believed the introduction of iron wire was originated by Mr. Squire, but it had not been found a satisfactory mode for the preservation of the syrup, for part of the iodine was taken out of the solution through the action the iron exerted. He thought Mr. Tilden's process an ingenious one. When it was first described to him there seemed to be some practical difficulties which Mr. Tilden had taken cognizance of, and had remedied to a great extent.

Dr. **Attfield** asked Mr. Tilden if the specimens of the syrup had exhibited any acidity. Some years ago Mr. Phillips considered that hydriodic acid was formed through decomposition of water.

Mr. Tilden had not noticed such to be the case.

Dr. Redwood said if it was kept for some time the sugar would undergo a change and become solidified, being converted from cane into grape sugar, and it was quite possible that a little hydriodic acid might occur.

Mr. Hills and Mr. Umney said that by bottling it all off while hot they had found it keep well for six months.

A Member had noticed that by simply immersing the bottle in a water-bath for five minutes the syrup returned to its original colour.

Mr. Barnes had noticed a change in the syrup of phosphate of iron, and enquired if it was necessary to keep it in a dark place.

Dr. Attfield said it was owing to oxidation, and recommended the bottle to be kept well stopped.

Dr. Redwood then read a paper on "*The Adulteration of White Precipitate,*" which had been sent by Mr. Borland of Kilmarnock.

The author commenced by alluding to the excellent paper read at the British Pharmaceutical Conference by Mr. Barnes, F.C.S., regretting that he had not stated whether the samples he had examined were fusible or infusible. He then referred to the forms for preparing it in the Pharmacopœias. The infusible would volatilise without fusing at a heat below redness. The form for the fusible was NH_2HgCl, and the infusible NH_2Hg_2Cl. He had examined 24 samples, and only five were made according to the authorised form. The fusible would not make so white an ointment as the infusible.

Mr. Barnes said that three or four of the samples he examined were fusible and the rest were infusible.

The President was very glad to find that it was not adulterated to such an extent as it used to be.

A Member asked Professor Redwood if there was any difference in the value of the two preparations.

Dr. Redwood was not aware that any experiments had been made.

Dr. Attfield did not imagine there would be any difference in their value, the mercury being in the same condition in both preparations.

Dr. Redwood exhibited some moulds which had been sent by Mr. Procter to illustrate his paper "*On Suppositories and Medicated Pessaries,*" in the present number of the *Pharmaceutical Journal*, in which he recommended the use of cones made of tinfoil, the usual conical shape being obtained by softening the end of a rod of gutta-percha, or of a stick of sealing-wax, and pressing it into a conical minim measure.

Several members and associates gave the results of their experience in the preparation of suppositories, etc., and Dr. Attfield referred to the great merits and the great educational value of Mr. Procter's paper, showing as it did what a large amount of work could be done with simple materials, but he did not think the pessaries made in the way Mr. Procter had described were so neat in appearance as those made in the gun-metal mould.

Mr. H. S. Waddington read an highly interesting paper, "*On the Preparation of Microscopic Crystals,*" in which he strongly recommended the process of rapid crystallisation. Mr. Waddington has evidently devoted a great deal of time and trouble in preparing processes for the preparation of microscopic crystals, and we regret that he read his paper so quickly, for it contained much that was most interesting and instructive to the microscopist. After the reading of the paper, Dr. Attfield, Professor Redwood, Professor Bentley, and the President spoke in very high terms of Mr. Waddington's paper, and hoped that he would pursue the subject still further.

Mr. Umney postponed the reading of a paper "*On a New kind of Kamala*" till the next meeting, which was announced to take place on the 4th of December.

ROYAL DUBLIN SOCIETY.

At the last evening scientific meeting of this Society, Dr. De Ricci read a paper "*On the Japanese Oak-feeding Silk Worm*" (*Bombay Japonica*). The speaker's attempts to rear this worm in Ireland had been comparatively successful, and from the results obtained he was inclined to believe that this species of silkworm could be easily acclimatised. The oak was the indigenous tree of this island, and he firmly believed that with care it would be feasible to establish in this country the cultivation of this important silkworm, and thus create a new branch of industry and a new source of wealth. The great disadvantage that this worm laboured under in this climate was that the worms were hatched before the oak leaved. Dr. Wallace, of Colchester, had previously failed in hatching this worm.

Amongst an interesting collection of minerals which had been brought for the musuem of the Royal Dublin Society, was a remarkable specimen of flexible sandstone from Delhi. Although ¼ of an inch thick, this sandstone could be moved about in the air like a piece of ribband, and exhibited either way a curvature of at least 5 or 6 inches from the original line occupied by the stone.

THE INSTITUTION OF CIVIL ENGINEERS.

The first meeting of the Session 1867–68 was occupied by the reading of a supplement to and the discussion upon the Paper "*Experiments on the Removal of Organic and Inorganic Substances in Water,*" by Mr. Edward Byrne, M. Inst. C.E., which was read at the close of last Session. The author now gave an account of experiments he had since made on the well-known filtering materials, magnetic carbide, and silicated carbon; and, after recording the results in a tabular form, he proceeded to make a comparison between those substances and animal charcoal.

His experiments were to the effect, that the action of the magnetic carbide was exceedingly feeble as regarded the removal of organic and inorganic impurities, and that it did not possess the property of softening the water except to a very small extent; whereas this property was possessed in a high degree by the two other filtering materials. Silicated carbon, however, quickly lost this power, and, after a short time, it rendered the water positively harder than it was before filtration. Animal charcoal, in its softening property, was not only more powerful than the silicated carbon, but more permanent in its action; and so far as the experiments went, it continued to remove inorganic matter. After a short time, however, it commenced to give back a portion of the organic impurity which it had previously removed. The silicated carbon, too, was found, in an equally short time, to give back not only the organic, but also the inorganic, matter which it had previously taken up.

To decide whether the organic matter contained in the water, so far as the nitrogen was concerned, had undergone any oxidation by its passage through these substances, the amount of nitrogen in the original water, and in that passed through each filter, was determined by the process which Professor Wanklyn had recently made known. By this extremely delicate test it was found that, for equal quantities of organic impurity, the amount of albuminous matter in the original and in the filtered waters was precisely the same; which fact was considered a sufficiently clear proof that the organic matter contained in the water had undergone no change by its percolation through these filtering materials.

The author then expressed the opinion, that while filtration must ever be considered most valuable for the removal of matter in mechanical suspension, it was practically useless as a means of removing substances in solution. He argued that the deductions to be drawn from these experiments, though made on a small scale, would, by reason of the systematic manner in which they were conducted, be safely applicable to cases of far greater magnitude. He concluded by expressing a hope, that the result of these investigations would serve the purpose of pointing out the danger of depending too much on the system generally of filtration, as well as of exposing the inconsistency of bringing home foul water, to undergo a delusive method of purification, instead of adopting the proper and only satisfactory plan of procuring water which was itself naturally pure.

CHEMICAL NOTICES FROM FOREIGN SOURCES.

Cobaltic Sulphide.—Th. Hiortdahl. Black cobaltic oxide heated to redness in a current of sulphuretted hydrogen fuses, and is converted into a yellow sulphide of a strong metallic lustre, of the composition Co_4S_3. Dehdyrated cobaltic sulphate fused together with baric sulphide and an excess of sodic chloride, yields a mass in which after cooling prismatic crystals are observed. Sometimes the baric oxide, formed during the reaction, separates in large leafy crystals which are intersected with prisms of the sulphide. This cobaltic sulphide is of a grey colour, and shows metallic lustre; it is soluble in acids, even acetic acid, and its composition is CoS.—(*Comptes R.* lxv. 75.)

Volcanic Gases.—Janssen has investigated the flames of the volcano of Santorin by means of the spectroscope, and found in it sodium, hydrogen, copper, chlorine, and carbon. —(*Comptes R.* lxiv. 1303.)

Nitriles, Action of Bromhydric Acid on.—C. Engler. If a current of dry bromhydric acid is passed through propionitrile (from potassic cyanide and sulphovinate) a crystalline mass is obtained which is propio-nitrilic dibromhydrate $N\Theta_3H_5 2HBr$. It fuses between 50° and 55°C., and sublimes when heated a little above that temperature. It is pretty stable in dry air, but decomposes readily in moist air with formation of propionic acid. The following equation shows the decomposition caused by water:

$$N\Theta_3H_5,2HBr+2H_2\Theta=NH_4Br+\left.\begin{matrix}\Theta_3H_5\Theta\\H\end{matrix}\right\}\Theta+HBr$$

The reaction between benzonitrile and bromhydric acid takes place in an analogous manner, benzonitrilic dibromhydrate, $N\Theta_7H_5,2HBr$, being formed. This compound differs little in its properties from the one just described; it fuses at 70°. Its decomposition with water gives rise to the formation of benzamid according to the equation:

$$N\Theta_7H_5,2HBr+H_2\Theta=N\left\{\begin{matrix}\Theta_7H_5\Theta\\H_2\end{matrix}\right.+2HBr$$

(*Zeitschr. Chem.* N.F. iii. 506.)

Carbohydrates, Action of Water at high Temperatures on.—O. Loew. Cane sugar is decomposed when heated with water in sealed tubes to 160° C., carbonic anhydride is formed and carbon separates, the latter amounting to nearly half the quantity of sugar taken. The contents of the tube show strong acid reaction, due to formic acid; a small quantity of ulmic acid is also formed. No decomposition takes place if sugar is heated with alcohol to the same temperature, or with a solution of baric hydrate. Other members of the sugar group treated in this manner show a similar behaviour. The action of water on gum gives rise to the formation of a new acid which is insoluble in water but soluble in alcohol and ether.—(*Sill. Am. J.* [2] 43, 371, and *Zeitschr. ch.* iii. 510.)

Ferrocyanides, Volumetric Determination of.—Gintl. The solution containing a ferrocyanide-compound is acidulated with sulphuric acid (in preference to chlorhydric acid, which causes turbidity), a trace of a soluble ferric salt is added, and the quantity of ferrocyanide measured with a standard solution of potassic permanganate. The end of the operation is marked by the sudden change of the originally blueish-green colour of the solution into a yellow and finally red tint.

Ferricyanides are previously converted into ferrocyanides by reducing them with sodium-amalgam.—(*Akad. Z. Wien.* lv. 1867.)

Melilotic Acid.—C. Zwenger. Melilotic acid, which is found in *melilotus officinalis* partly in the free state, partly combined with cumarin, belongs to the salicylic acid series. When fused with potassic hydrate hydrogen is evolved and salicylic and acetic acid formed. Heated in a retort, two equivalents of water separate and melilotic anhydride, $C_{18}H_8O_4$, distils over. The acid is monobasic but diatomic; its salts mostly crystallise well. Dibrom-melilotic acid, $C_{18}H_8Br_2O_6$, is obtained by adding bromine slightly in excess to melilotic acid at ordinary temperature. This acid is crystalline, sparingly soluble in water, readily in alcohol or ether. The action of strong nitric acid gives rise to the formation of dinitromelilotic acid, $C_{18}H_8(NO_4)_2O_6$. Melilotamide,

$$\left.\begin{matrix}C_{18}H_8O_2\\H_2\end{matrix}\right\}\begin{matrix}O_2\\N\end{matrix}$$

is formed by treating melilotic acid with ammonic hydrate. Melilotic acid may be prepared artificially from cumarin by joining to the elements of the latter first two equivalents of water (converting it into cumaric acid), and then two of hydrogen:

$$C_{18}H_6O_4+2HO+2H=C_{18}H_{10}O_6$$

This reaction is accomplished by treating cumarin in aqueous solution with sodium-amalgam.—(*Ann. Chem. Pharm. Suppl.*, v. 100.)

Erucic Acid, Derivatives of.—O. Kausknecht. Erucic acid was prepared by treating beet-root oil (Rüböl) with plumbic oxide, extracting the lead soap with ether, and decomposing the remaining plumbic erucate with chlorhydric acid. The action of alcoholic potassic hydrate upon erucic dibromide under pressure and 140°—150°C., gives rise to the formation of behenolic acid $\Theta_{22}H_{40}\Theta_2$:

$$\Theta_{22}H_{42}Br_2\Theta_2=2HBr+\Theta_{22}H_{40}\Theta_2;$$

at ordinary temperature monobromerucic acid, $\Theta_{22}H_{41}Br\Theta_2$, is formed:

$$\Theta_{22}H_{42}Br_2\Theta_2=HBr+\Theta_{22}H_{41}Br\Theta_2.$$

The dibromide of the latter, treated by this reagent loses two atoms of bromine, but whether monobromerucic acid is regenerated, or monobrombehenolic acid formed, has not been decided. Behenolic acid is soluble in water and in alcohol, and fuses at 57·5°; it unites with two or four atoms of bromine, forming the bromides $\Theta_{22}H_{40}Br_2\Theta_2$ and $\Theta_{22}H_{40}Br_4\Theta_2$ (perhaps $\Theta_{22}H_{38}Br_4\Theta_2$). When heated with fuming nitric acid three bodies are obtained: 1. Dioxybehenolic acid $\Theta_{22}H_{40}\Theta_4$; monobasic, fusing at 90°—91°, insoluble in water and less soluble in alcohol than behenolic acid. 2. Brassylic acid, $\Theta_{11}H_{20}\Theta_4$; dibasic, scarcely soluble in water, soluble in alcohol or ether, fusing at 108·5°. 3. An oil, $\Theta_{11}H_{20}\Theta_3$, probably the aldehyde of brassylic acid, which may be converted into the latter by oxidation with bromine:

$$\Theta_{11}H_{20}\Theta_3+H_2\Theta+Br_2=\Theta_{11}H_{20}\Theta_4+2HBr$$

Erucic dibromide treated with freshly precipitated argentic oxide is converted into an oily mass which is a mixture of liquid oxyerucic acid, $\Theta_{22}H_{42}\Theta_3$, and solid dioxybehenic acid, $\Theta_{22}H_{44}\Theta_4$, the latter being obtained as a bye-product. The reaction proceeds in the following manner:

$$1.\ \Theta_{22}H_{41}Br_2Ag\Theta_2=AgBr+\Theta_{22}H_{41}Br\Theta_2$$
$$2.\ \Theta_{22}H_{41}Br\Theta_2+AgH\Theta=AgBr+\Theta_{22}H_{42}\Theta_3$$

Oxyerucic acid is a heavy oil, insoluble in water, soluble in alcohol and ether. Dioxybehenic acid may be more readily prepared by boiling oxyerucic acid with an aqueous solution of potassic hydrate,

$$\Theta_{22}H_{42}\Theta_3+KH\Theta=\Theta_{22}H_{43}K\Theta_4$$

and decomposing the potassic dioxybehenate with chlorhydric acid. The acid is soluble in alcohol, and fuses at 127°. When erucic acid is boiled with diluted nitric acid it is converted into an isomeric compound, brassidinic acid, $\Theta_{22}H_{42}\Theta_2$, which is monobasic, fuses at 60°, and is less soluble in alcohol or ether than erucic acid. The dibromide of brassidinic acid is a crystalline compound, thus differing characteristically from the dibromide of the isomer, which shows no trace of crystallisation.—(*Ann. Chem. Pharm.* cxliii. 40.)

Copper, Determination of.—Lecoq de Boisbaudran. Instead of precipitating copper from solution, in presence of other metals, with zinc, the author proposes to effect its separation by means of an electric current. The slightly acid solution of the sulphates of copper and other metals is contained in a platinum crucible which forms the negative pole of an electric current from two of Bunsen's cells; the positive pole terminates in platinum foil, and passes through a perforation in a watch-glass which covers the crucible, dipping into the liquid. The copper separates in a perfectly pure state, and the reduction is accomplished in three or four hours. Intensity of current, concentration of solution, temperature, and amount of free acid may vary within wide limits without impairing the accuracy of the result.—(*Bull. Soc. Chim.* vii., 468.)

NOTICES OF BOOKS.

Geschichte der Chemie. Bearbeitet von Dr. TH. GERDING, *Leipzig*, 1867. (*History of Chemistry, by* Dr. T. GERDING).

THIS is a highly interesting and valuable contribution to the literature of chemistry. It is avowedly founded on the classical work of H. Kopp, published in four volumes in 1843–47; but it differs from it, not only in its smaller size, but also to some extent in its arrangement. Part I., which occupies nearly half the book, is entitled "General History of Chemistry; or Historical Development of Chemical Science with respect to the most important Chemists and their labours." This, the true history, is divided into four periods. The first, which may be distinguished as the ancient period, brings us down to A.D. 400. The second, the mediæval, extends to the 17th century, and is subdivided into separate histories of alchemy and of medical chemistry, which last may be said to have commenced with Paracelsus in the commencement of the 16th century. The third and fourth periods comprise modern chemistry. The former extends from the middle of the 17th to the end of the 18th century, and contains the reign of the celebrated phlogistic theory; while the latter, which occupies nearly one-quarter of the entire book, is the "quantitative age," the short but brilliant career of true chemistry. This last section is particularly valuable to the modern chemist, for not only does it present us with succinct sketches of the lives and labours of the earlier chemists of the century, Lavoisier, Dalton, Gay Lussac, Davy, Berzelius, etc.; but it reviews with great impartiality, and, on the whole, with great exactness, the researches of many of the chief living chemists, and the English reader will notice with pleasure the names of Graham, Stenhouse, Anderson, Gladstone, and many other distinguished fellow countrymen. We are indeed compelled to remark some glaring omissions, such as the absence of all notice of Andrews, Baeyer, Cannizzaro, Frankland, Odling, Zinin. and a few others of equal celebrity; but when we consider the extent of the plan and the smallness of the book, we cannot wonder at finding some blemishes.

The second part of the work is of even more practical importance. It is entitled "Special History of Chemistry; or, History of the most important Doctrines, Theories, and Single Substances." Under each head we find a clear though brief summary of the chief discoveries which have been made upon it. Thus under nitrogen (page 365) a very interesting account is given of the views which have at various times been taken, not only of the nature of nitrogen itself, but also of some of its chief compounds. We might give many similar instances, and should be glad, if space permitted, to give some extracts from the book. As it is, however, we can only commend it to the notice of our readers, and express our opinion that an English translation would meet with a favourable reception.

Pharmaceutical Chemistry. By JOHN ATTFIELD, PH.D., F.C.S. London: John Van Voorst, Paternoster Row. 1867.

DR. ATTFIELD'S Chemistry has for its object the thorough teaching of Pharmacy and Chemistry as applied practically to Pharmacy; professedly and indeed truly it is a guide-book. The information conveyed in the text is mainly that which cannot be learnt by experiment, such as the derivations of words and terms, the places whence different materials, pharmacal or chemical, are derived, with some of the various theories lately advanced upon the composition of somewhat complex drugs, *e. g.*, bismuthic carbonate, goulard water, and tartar emetic. The work being of the size of the British Pharmacopœia of 1867, and mainly founded upon it, forms, in many instances, a really invaluable commentary to it. Thus the symbolic expressions in the Pharmacopœia, called by courtesy the formulæ of compounds according to the new system, with their old names (truly a most characteristic example of a compromise)—formulæ which are neither rational or empirical—with Dr. Attfield's aid are presented to the student in a more intelligible form. The expressions of the Pharmacopœia, such as $2(Bi_2CO_5)H_2O$, are here translated into a form reconcilable with the trivalent character of bismuth. The most modern expressions of chemists are used throughout the text, thus we meet with trivalent, etc., and equivalent is very well explained to the student as opposed to atoms and molecules.

We cannot, however, say that Dr. Attfield has had the same success in the consistency of his nomenclature. When new words like mercurous and mercuric salts are explained to the pharmaceutical chemist, who has to connect the ideas with compounds like calomel and corrosive sublimate, they should be strictly limited to metals like mercury, that have two classes of salts; other salts should have names consistent with their formulæ, or already familiar to the pharmacist or dispenser. Thus when the student reads of calcic carbonate and calcic oxalate (p. 417) he compares them in his mind to mercuric carbonate, and mercuric oxalate, concluding justly enough that there is a calcous carbonate and calcous oxalate. This is more particularly the case, from the expressions, sulphate of calcium, carbonate of calcium, and nitrate of potassium being almost invariably used in the text. Again, under lead, there is no plumbous oxide, it being called oxide of lead, although plumbic peroxide is shortly after mentioned.

In our opinion the most valuable part of the work is that devoted to the preparations of the alkaloids and active principles, the most difficult thing that the student of *materia medica* has to learn. The *reason* for every successive process is given fully, and, what is more, accurately and clearly. The many excellent features of Dr. Attfield's book would have been increased had more attention been devoted to the spectroscope and saccharometer. To this might be added in a future edition a discussion of chemical organic compounds not actually mentioned in the British Pharmacopœia, but of the greatest value in full detail.

As it is, Dr. Attfield's book is written in a clear and able manner; it is a work *sui generis* and without a rival; it will be welcomed, we think, by every reader of the Pharmacopœia, and is quite as well suited for the medical student as for the pharmacist.

Companion to the New Edition of the British Pharmacopœia. 1867. By PETER SQUIRE, F.L.S. London: John Churchill and Sons, New Burlington Street. 1867.

WE are glad to find that Mr. Squire's Companion has reached a fifth edition. The reward is well deserved, and is accorded to few works of the kind; and this fact alone would suggest the void there would be in the literature of *Materia Medica* without it.

But a still more unprecedented fact is that this current edition has been reprinted within the short space of a fortnight, owing to the great demand. It was wise therefore, we think, to make no additions to the reprint, so as to avoid the formation of a new edition. However, if the sale of the work is sufficiently great, a new edition will, we hope, be prepared shortly, which will incorporate in it many new researches in Pharmacy, such as the use of carbolic acid externally in various solutions, the extended applications of the new remedies of the Pharmacopœia, and the detection of impurities in various commercial specimens of drugs, for so many of which researches we are indebted to Mr. Daniel Hanbury, *e. g.*, Burgundy pitch and Storax. Mr. Squire will find his materials now increasing in a very rapid progression, and will yearly find the duty that he has undertaken a more difficult one. This perhaps may be sufficient to atone for some trifling matters which under other circumstances might serve for complaint.

Thus, when a new and strikingly useful remedy is proposed, and the formula for the preparation of such is given, it is only fair to mention the authority introducing this, as a sort of guarantee.

A remedy proposed by Mr. A. may have its own value, but one proposed by Dr. B., whose name is a household word in the profession, may probably be thought more worthy of a trial.

In two succeeding paragraphs Mr. Squire gives a notice of urethral suppositories, or medicated bougies, followed by that of the medicated pledgets of cotton. The latter we are told were introduced by Dr. Greenhalgh, but who introduced the former? We should have thought that the name of Sir Henry Thompson, a court surgeon, would have been not only gracefully, but also authoritatively coupled with the mention of medicated bougies.

Again, under the head of Staphisagria we find only this: "The oil of the seeds, 1 to 7 of lard, has been successfully used by Mr. Balmanno Squire in *Prurigo senilis.*" By the omission of the fact that an Unguentum Staphisagriæ (made from the seeds) has been prepared in most of the hospitals of London for many years, it is implied that Mr. Balmanno Squire was the first to make known the vermicide properties of Staphisagria.

When, in a work of authority, names of originators are given, it is impossible to exercise too much delicacy and tact.

All the chemical formulæ are given as in the Pharmacopœia of 1867, with the addition of the equivalents; we get as a result the following information: "Diluted phosphoric acid, $3HO,PO_5$, or H_3PO_4, eqv. 98, dissolved in water . . . 6 fluid drachms contain half an equivalent PO_5, or a quarter of an equivalent P_2O_5." The confusion here results from following too closely "official" authority.

The chief value of the work, viewed as a whole, lies in the amount of practical observation shown by its author, and the list of antidotes and incompatibles, always a very strong feature, has been in this edition strengthened still further. This information, so often neglected in text-books of *Materia Medica*, is really indispensable, and Mr. Squire's book is the authority in this respect. The "Companion" may be fairly called the parent of the Pharmacopœia for the United Kingdom, but the honour of having for a younger offspring a universal Pharmacopœia, or Codex, is we believe quite unattainable. The present edition, or perhaps the next, with a chemistry adapted to Continental views, we think deserves that honour as much as any work does. If any of our readers are about to purchase the British Pharmacopœia, we offer them the advice of *Punch* to those intending to marry,—"don't," buy "Squire's Companion" instead.

A Programme of Atomechanics; or, Chemistry as a Mechanics of the Panatoms. By Gustavus Hinrichs, Professor of Physics, Chemistry, and Mineralogy at the Iowa State University; Chemist of the Geological Survey of Iowa, &c.

It must be confessed that scientific men in Europe are not, as a rule, accustomed to look to America, or rather to Americans, for new lights in science, and when such lights appear on the American horizon, it is generally found that the philosopher is a foreigner. Americans appear to be too eager for an immediate and material return for their intellectual capital to devote themselves to the more recondite forms of scientific speculation. That Americans are *capable* of any amount of work of mind or body, that they are eminently clever and ingenious, we freely admit, but few will even attempt to deny that American scientific literature does not, as yet, hold so high a position as might be desired. It is therefore with great interest that we receive any contributions to science emanating from that great country. That the public feeling in America is against us we are unfortunately aware; and this dislike to England takes, among many others, the form of underrating English science, and, whenever possible, French or German models are used by professors to hold up to the study or emulation of their pupils. Being aware of this, and knowing also that in England we not only bear no animosity to America, but are anxious to cultivate her friendship, we are very unwilling to criticise too harshly any scientific treatise emanating from our brethren on the other side of the Atlantic. But the name of the author of the work, the title of which we have quoted above, is so obviously German that we trust we may indulge in a few remarks upon his treatise (so long as we keep within the limits of fair criticism) without hurting the susceptibilities of any of our American brethren.

The author of this work has sent us a printed sketch of his production, but we may at once confess that the materials before us are insufficient to induce us to follow his argument in detail, and we can therefore only lay them before our readers accompanied by such comments as naturally present themselves.

The author includes in his "Programme" some historical remarks, and he especially states that he made the discovery of "Pantogen" in 1855, when studying at the Polytechnic School of Copenhagen. This pantogen, or "Urstoff," as M. Hinrichs writes it in German, is, in fact, the one primary or truly elemental matter of which all the so-called elements of the chemist of the present day are made up. It is almost impossible within the limits of an article like this to state the reasons which the author gives for assuming the existence of pantogen; and then for concluding that, by assuming the existence of one primary elemental matter, and calling it by a specific name, he has discovered this only true element.

It is needless, perhaps, to say that, like all discoverers of this class, he falls back continually on "analogy." This too free use of analogy has been the bane of science from the time of Plato, and it would appear that the race of speculators who mistake fanciful analogies for fundamental scientific laws is by no means yet extinct.

M. Hinrichs finds that "the history of science is one and the same," and that "we may therefore learn the history of chemistry by studying that of her elder sister, astronomy." He also finds that:—"*Lavoisier* is the *Copernicus* among chemists; both pierced the veil of appearances, and discovered the true order of things. Copernicus found the earth too insignificant to move the heavens. Lavoisier found the metal lighter than its ash (oxide). *Modern Chemistry*—not the chemistry of most of our text-books—is truly *Keplerian*. The beautiful laws of *Dulong* and *Petit* (specific heat of the atoms), of Gay Lussac (volume of atoms), of Mitscherlich (on isomorphism), &c., were grasped by *Gerhardt* (chemical types) and now make modern chemistry an exact science. The great discoveries in organic chemistry, from *Liebig* to *Berthelot*, and the spectral analysis of Bunsen and Kirchhoff, have made the domain of chemistry as universal as that of astronomy.

"We may venture to conclude that this parallelism in the history of astronomy and chemistry does not end here. The history of astronomy since 1619, when Kepler's third law was discovered, may teach us what changes await modern chemistry.

"We must conclude from this analogy that there exists some general principle which will transform modern chemistry into a *mechanics of the atoms*; for about fifty years after *Kepler* astronomy had become a mechanics of the heavenly bodies, *i.e.*, of *cosmical atoms!*

"The basis of this celestial mechanics (as Laplace so fitly termed astronomy) is but a *hypothesis*,—that of universal gravitation,—which essentially consists in the affirmation that the heavenly bodies only differ in regard to the amount or quantity of matter.

"Let us have the boldness to pronounce a similar hypothesis in regard to the chemical atoms. Let us suppose that the atoms of the different elements only differ in regard to *quantity*—that is, in regard to the *number* and relative *position* of the atoms of some *one* primary matter, just as the planets only differ according to the *number* of kilogrammes of ponderable matter they contain, and its distribution around their axes. Since everything thus would be composed of this *one* primary matter we call it *pantogen*, and its atoms *panatoms*."

We have felt bound to give this long quotation in order to show the method adopted by our author, and the way in which "pantogen" was "discovered."

It must not be supposed for an instant that what we have said gives more than an exceedingly minute idea of our author's "Programme." By assuming the hypothesis of pantogen he is enabled to explain and calculate all the physical, chemical, and morphological properties of the elements and their combinations, which *may be calculated* just as the orbit of a planet is calculated. He is also enabled to ascertain the atomic constitution, and from them to calculate the *atostere* (specific volume), fusing and boiling point, refracting power, and, to some extent, spectral lines. From all this it is obvious, we think, that M. Hinrichs' work is by far the most wonderful production of modern times, and its author certainly the most remarkable man in his adopted country. It is then with feelings of profound astonishment that we learn that the author's paper containing the first instalment of his researches promised to the world on page 4 of his "Atomechanik," was refused admission into *Silliman's Journal*, although the MS. only contained 31 pages large quarto! His second paper, "On the Trimorphism of Titanic Acid," met the same fate, "only more so."

How M. Hinrichs arrives at his results we do not know until we see his work. We must confess that from the slight glimpse we obtain of his mental process, as seen in his "Programme," we fancy that the results are obtained by arguing backward and writing forward. He confesses to using principally "the method of successive approximations." To deduce all the physical characters of a substance with the aid of the hypothesis of pantogen must tax the resources of this method somewhat severely. The mere fact that the addition of C_2H_4 to a group in one case *raises* its fusing point 2°, and that the addition of C_3H_6 in another case *reduces* it 4°, must, we think, be somewhat difficult to explain, even by one who has the "Atomechanik" at his fingers' ends; but there is one feat which cannot, we feel sure, be beyond the resources of the author of the work alluded to, and if he only accomplish it we will "pronounce" at once in his favour, and instantly send in our adhesion to the new theory; the feat we allude to is simply to isolate pantogen.

CORRESPONDENCE.

The Camphor Storm Glass.

To the Editor of the Chemical News.

Sir,—I see by your Notices to Correspondents that you sometimes have to answer inquiries respecting the storm glass and its value as a meteorological instrument. The frequent reference made to it by the late Admiral Fitz Roy gave an almost official sanction to its use, and induced some instrument makers to manufacture it largely, and even to attach it to the ordinary barometer and thermometer. This led me to examine the storm glass with some care. I made one on a large scale, in a quart bottle, placed it on the window ledge, and kept a journal of its behaviour during some months. The conclusion I arrived at was that the storm glass is not acted on by light, or atmospheric electricity, or wind or rain, etc., but solely by variations in temperature; that it is, in fact, a rude kind of thermoscope, vastly inferior to an ordinary thermometer, and has no meteorological value whatever.

My paper on the subject is printed in the *Philosophical Magazine* for August, 1863. It produced a few remonstrances to the effect that I had degraded a pleasing instrument to the level of a toy. I believe it to be, as you replied to your correspondent, only a toy, but it is a very pretty one, and exhibits effects of crystallisation of great beauty and variety. I generally have one hanging up in a back window, and it affords me pleasure to look at it and to show it to my friends.

One of my conclusions, viz., that light has no action on the storm glass, was questioned by a gentleman with apparently so much reason and good sense that I gladly examined the case as put by him.

This gentleman had a storm glass hanging up in a west window of a house in a street in London. He was in the habit of observing it every day during a few years. He went to reside in the country, and hung up his favourite storm glass again in a west window. He soon noticed that the crystals were larger and finer than any he had ever seen in the tube when it was in town. He naturally referred the change to the increased light and brilliancy of the sky, as compared with the dingy atmosphere of London. On inquiry I found the change to be really due to heat and not to light. In the town house the sun disappeared some time before sunset behind the opposite houses. In the country house, the west window was in full view of sunset. Now, as soon as the sun came round to the window in the afternoon of every warm, cloudless day, it melted the solid contents of the storm glass, and allowed impurities to subside; but after sunset crystallisation again took place, excluding other impurities, so that these repeated fusions and recrystallisations produced the fine crystals, and not the improved light. Such at least is my explanation of this fact as stated.

I also proved some years ago that the motion of camphor and other volatile bodies towards the light, is really towards the coldest part of the bottle. The coldest part is generally towards the light, because on that side of the bottle radiation is most free. A gentleman wrote to me that my theory could not be true, because in a number of his bottles the deposits were on the sides farthest from the light. On inquiry it turned out that his shelves were erected against an outer wall, facing the north, so that the sides of the bottles nearest the wall were constantly the coldest.

By dipping a piece of filtering paper into ether, and placing it on a bottle containing a little camphor, etc., a deposit may be determined in a few seconds to any part of the bottle at pleasure, and of any pattern or device we may choose to give to the filtering paper.—I am, etc. C. Tomlinson.

King's College, W.C., Oct. 26, 1867.

Volatility of Sesquichloride of Iron.

To the Editor of the Chemical News.

Sir,—Since writing the letter which you kindly inserted in your last issue (*Amer. Reprint, Dec.*, 1867, page 319), the subject of it has received fuller investigation at my hands, and the views therein contained are strengthened by the following experiments I have made:—

(1). Another sample of the best sulphocyanide of potassium, obtained from large operative chemists, tested with pure hydrochloric acid, gave a distinct pink tint. This sulphocyanide of potassium was dissolved in hot absolute alcohol, the solution filtered, cooled, and the crystals separated; they were then freed from alcohol by heat. An aqueous solution was made from the salt thus obtained and filtered.

(2). The aqueous solution tested with the pure hydrochloric acid used above, gave no tint.

(3). Mr. Skey's experiment was tried with this pure reagent solution. The sesquichloride of iron and hydrochloric acid were placed in a large watch-glass having ground edges, covered with a glass plate wetted on the under side with the sulphocyanide solution. After the lapse of several minutes, the cover was removed, placed upon white enamelled glass, and more of the test solution added. The experiment was repeated several times under varying conditions; with regard to the proportions of sesquichloride of iron and hydrochloric acid, in no case was any colouration produced in the sulphocyanide of potassium. Mr. Skey, who is evidently a diligent worker in science, has apparently misinterpreted the effect produced in a colourless solution of ordinary sulphocyanide of potassium by the vapour of hydrochloric acid.

In conclusion, Sir, I deny that at present we have any proof of the volatility of sesquichloride of iron at common temperatures.—I am, etc., Henry Seward.

Science Teachers.

To the Editor of the Chemical News.

Sir,—Seeing that the subject of science teachers is under discussion in your columns, I venture to offer a few remarks, in the hope that something will be done to remedy the existing anomalous state of things with regard to science examinations; feeling, doubtless with many others, that a measure which bids fair to benefit the country, by enlarging the area of our educational basis, is falling from lack of well-directed energy to accomplish its desirable purposes.

It is admitted on all sides that the days when Latin and Greek should monopolise our school hours are at an end, and that it is necessary, in order to effect material progress, that our scholastic system should be modernised by bringing within its range those departments of intellectual culture in which, more especially, such rapid advances have been made in latter years. But I cannot think that the latest step on the part of the authorities at South Kensington is calulated to further the propagation of scientific knowledge, or to facilitate the introduction of chemistry and its concomitant sciences as recognised and essential branches of a middle-class education. I refer to the abolition of the November examinations for teachers and the amalgamation of pupils and teachers at the May examinations. It is now only necessary for a candidate to obtain a first or second class certificate in order to style himself and act as a "science teacher." Such a sudden transformation of pupils into teachers cannot, I think, be productive of beneficial results; for it is certainly not a matter of difficulty for a pupil of 16 or 17 years of age, who has applied himself to his subject, to pass in the second, or even in the first class; but it is very rarely that such a pupil becomes by this circumstance qualified to hold the responsible position of a teacher. It may be argued that there was so little difference between a third-rate teacher and a first-rate student that it was absurd to have separate examinations for them; but was not this fact sufficient in itself to prove the necessity of raising the standard of examination for teachers, instead of lowering it to that of the pupils?

This, then, appears to be the root of the evil. The examinations for teachers were never sufficiently stringent, or rather they did not sufficiently touch upon the real work of the teacher, which, in experimental science, is to a great extent of a practical nature. If a separate examination for teachers be again instituted, I would suggest, in order to render it more efficient, that something like the following scheme should be adopted:—

That no candidate should be allowed to style himself a "science certificated teacher" until he has passed in at least four subjects.

That there should be no second or third class; a certain standard to be fixed which all candidates must reach.

That there shall be a theoretical and practical examination in those subjects which admit of such treatment.

That each candidate shall be obliged to deliver a lecture of not less than half an hour's duration before a committee or board of examiners, on some portion of the subject in which he is a candidate, and that a few simple pieces of apparatus should be provided for the illustration of the lecture.

This last requisite I consider to be of the utmost importance, as lecturing forms the chief portion of a teacher's work, and his facility of expression, as well as his skill in manipulation, might thus be thoroughly tested.

I am of opinion that such a scheme as the above would meet with the approval of the majority of those concerned, and none more than science teachers themselves would appreciate the consequent elevation of their profession in the eyes of the public.

Doubtless objections will be raised on account of the extra time and expense involved; but has not experience shown that neither time nor money can be more wisely expended than upon education, and never with such certainty of ultimately yielding great and profitable results?—I am, etc.,

F. N. J.

October 29.

On the Occurrence of Sulphocyanide of Ammonium in Gas Mains.

To the Editor of the Chemical News.

Sir,—The existence of sulphocyanide of ammonium in gas mains, even at considerable distances from the gas works, is of very constant occurrence; and it is not caused by the gas, as Mr. Hart suggests in his paper before the Manchester Philosophical Society (*American Reprint* Chemical News, January, 1868, page 35), but is produced in the mains and service apparatus by the action of the ammonia in the bisulphide of carbon contained in coal gas, thus:—

$$2NH_3+CS_2=NH_4CNS+H_2S$$

This may at any time be proved by passing the purified gas supplied to the public through hydrate of lime, and soon, if ammonia be present, the lime will be found to be charged with more or less of sulphocyanide of calcium.

This reaction will account not only for the presence of sulphocyanide of ammonium in the water of all the hydrants and water meters of a district, but also for the ferrocyanide and sulphide of iron which are so commonly found in the iron mains.—I am, &c. H. Letheby.

College Laboratory, London Hospital, November 5, 1867.

Siliceous Stalactites.

To the Editor of the Chemical News.

Sir,—As a matter of curious interest I send you specimens of siliceous stalactites of peculiar origin. In the manufacture of "superphosphate" from phosphates of fossil or mineral character, a snow-like substance is abundantly deposited in the flues, &c. This is silica, and has its origin in the action of the sulphuric acid upon the fluoride of calcium present in the phosphates. Fluoride of silicon is given off as a gas, which is carried on with the dry steam, but when the latter becomes moist, is decomposed, giving rise to hydrofluosilicic acid and silica. For the first time in my experience this deposit of silica has taken the form now sent. Probably some of your readers may have met with better examples.—I am, &c. Charles F. Burnard, F.C.S.

Nov. 2, 1867.

The Camphor Storm Glass.

To the Editor of the Chemical News.

Sir,—The question put by your correspondent (*American Reprint* Chemical News, Jan., '68, page 59) as to the origin of the oily looking layer sometimes seen at the top of the liquid column of the storm glass, admits of a ready answer. I have often noticed this oily looking layer. It is never formed, I believe, except when the glass has been exposed to the heat of the sun, so as to liquefy a considerable portion of the solid contents. These consist of nitre, sal-ammoniac, and camphor. The first two are taken up by the water of the composition, and the last by the spirits of wine, in quantities varying with the temperature. Now, while the tube is being warmed by the sun, a portion of the alcohol distils to the upper or air-filled part of the tube, and when the sun goes off the window and the tube cools, the vapour condenses into strong alcohol, the solvent powers of which for camphor are much greater than the spirits of wine originally employed. This alcohol, then, in settling down becomes saturated with camphor, and forms a well-marked layer at the top of the liquid column.

This effect may be very well shown by the following contrivance: — Put into a long test tube a quantity of nitre or sal-ammoniac; fill the tube one-half with water. There should be more salt than the water can dissolve. Next put the tube into a gallipot containing warm water and standing on the hob. When the tube is quite warm, fill it up with strong camphorated spirit, leaving room for a well fitting cork and a little air. Shake up the contents and return the tube to its warm water bath. After a short time a well-defined oily looking layer, from two-tenths to five-tenths of an inch in thickness, will be seen at the top of the liquid column. If crude sal-ammoniac be the salt used, the colour of this layer will be yellow passing into brown, due probably to a trace of iron in the salt. If pure nitre be used, the layer will be colourless. That this layer really consists of a very strong alcoholic solution of camphor may be shown by dipping into it a cold glass rod, which will be quickly incrusted with solid camphor. If the tube be left to cool undisturbed small camphor stars will be formed, and circulate in regular order within the oily looking layer. The effects vary according as nitre or sal-ammoniac be used, but they are very pretty in either case.

This I believe to be the best explanation of the phenomenon in question. There is, however, a strong tendency to stratification in materials of such different densities, when in consequence of a gentle heat, such as that of the sun and of slow cooling, they are allowed to settle down. I have noticed three and even four distinct layers in a tube containing only nitre and camphorated spirit. If the storm glass be exposed to a fierce sun, its contents will in cooling be so far separated that before the glass will act properly it must be inverted and shaken. It will then require a day or two of repose before its usual phenomena are exhibited.—I am, etc.,

C. Tomlinson.

King's College, London, November 15, 1867.

Organic Matter in Water.

To the Editor of the Chemical News.

Sir,—Since the publication of Messrs. Wanklyn, Chapman, & Smith's paper, which has furnished a process, at the same time both qualitative and quantitative, for the estimation of organic matter existent in potable and other waters, the idea has suggested itself to me that the results obtained representing the amount of ammonia in millegrammes per litre, present as such, or in the form of albumenoïd nitrogen, may perhaps be regarded degrees, or parts of a degree, referable to some arbitrary standard.

For such a standard, we may take for instance ·1 mlgrm. of ammonia per litre, and call that quantity 1° of nitrogenous matter. The amount I have chosen, viz. ·1 mlgrm. is perhaps not the one best adapted to the purpose; it is a quantity, however, often found in the analysis of waters, and will serve for explanation.

I will give the results of three analyses lately made, and then apply the system I propose.

The specimens examined I have numbered 1, 2, and 3. 1000 c.c. was taken for No. 1, and half that quantity for Nos. 2 and 3. The examination was conducted in the manner described by the authors of the paper above referred to.

Water No. 1 is a specimen of that supplied by the Liverpool Corporation to the town in which I am staying. No. 2 was taken from a brook contaminated to some extent with sewage; No. 3 was from a stream receiving much sewage and refuse from bleach and other works.

I have, for the sake of comparison, examined waters from three totally different sources, waters in which the ratio of organic impurity was presumed to be progressive.

No. I. 1,000 c.c.		No. II. 500 c.c.		No. III. 500 c.c.	
By distillation with Na_2CO_3	Mlgrm.	By distillation with Na_2CO_3	Mlgrm.	By distillation with Na_2CO_3	Mlgrm.
1st 100 c.c. } 2nd ...... }	·0		·07		·09
By alkaline permanganate	·15	By alkaline permanganate	·47	By alkaline permanganate	·75
	·15		·54		·84

In order to compare the above results it is necessary to double the amounts of ammonia found in Nos. 2 and 3, and we have—

No. 1	·15
No. 2	1·08
No. 3	1·68

Translating these figures into degrees, and taking ·1 mlgrm. of NH_3 per litre as the representative of a water of 1° of "organic nitrogen," we have for—

No. 1 a water of	1°·5
No. 2 "	10·8
No. 3 "	16·8

It will be seen that I have not made any distinction between the degrees corresponding to the ammonia originally present as such, or due to the decomposition of urea, and those which represent the amount afterwards evolved on the addition of alkaline permanganate, but some distinction will probably be necessary.

I may remark, in conclusion, that the result of the analysis of water No. 1 confirms Professor Wanklyn's statement in the *Laboratory* (No. 26, page 442), with reference to the stability of albuminoïd matters in the presence of a boiling solution of sodic carbonate of the strength used by himself and colleagues in their process.—I am, etc.,

Philip Holland.

Chorley, Lancashire.

Salt as an Adulterant in the Dyeing Trade.

To the Editor of the Chemical News.

Sir,—Amongst the substances fraudulently added to chemicals and drysalteries, both organic and inorganic, common salt holds a prominent place. When thus employed it not merely dilutes or lets down the article to which it is added, but often exerts a most injurious positive action. Thus, when used to "spring," as it is called, extracts of dyewoods, it very much injures their quality. It is well known that to dye an even, fast, and brilliant shade, the colouring matter must be held in a state of perfect solution; or, if insoluble, in the finest possible suspension, so that it may be slowly and gradually delivered to the fibre. Now the presence of salt impairs the balance between the solvent and the colour, and causes the latter to be rapidly and irregularly deposited on the surface of the goods in a dull state, capable of easy removal. In some cases the affinity of the fibre for the colour seems masked, so that the latter, instead of "taking on," falls as a sediment to the bottom of the dye-pan. At other times a colour or mordant previously put on is impoverished by the salt thus present. Thus, to take a very common case—suppose a coburg or union damask has to be dyed green. The worsted having been dyed as usual, and the warp done a Prussian blue, the piece is passed through extract quercitron. If this has been adulterated with salt, the piece when apparently dyed will be found, after having been rinsed and dried, to have a green weft and a pale blue warp, the Prussian blue deposited on the cotton having been impoverished by the salt.

Ground turmeric is another article often mixed with salt, which imparts to it a brighter appearance in the state o powder. The action of the salt here is very similar to what has been above described. The mordants are, as the dyers term it, "naiged away" from the warps of the salt. Drysalters, when the salt in their turmerics is detected, have been known to state in extenuation that the article had been damaged by seawater, although men in their employment own that they are told to mix a certain proportion of salt with the root during grinding.—I am, etc., W.

MISCELLANEOUS.

The Atmosphere of the Underground Railway.—On Wednesday Dr. Lankester held the adjourned inquest relative to the death of a young woman who died at King's Cross Station. Scientific evidence was given as to the quality of the atmosphere in the tunnels by Professor J. E. D. Rodgers and Dr. Letheby. A special report has been prepared for this journal and will appear in our next.

Royal Institution of Great Britain. — The following lectures have already been decided upon:—The Christmas lectures, adapted to a juvenile auditory, will be delivered by Professor Tyndall, LL.D., F.R.S. Subject—Heat and Cold. Professor Tyndall will also deliver ten lectures on "the Discoveries of Faraday." Professor Roscoe, F.R.S., will deliver eleven lectures on "the Chemistry of the non-Metallic Elements." George Scharf, Esq., F S.A., will deliver six lectures on "Historical Portraiture of various Times and Countries;" and Professor Foster will deliver four lectures on "the Development of the Chick in the Egg." The Friday evening meetings will commence on January 17, when a discourse will be delivered by Professor Tyndall. After Easter, eight lectures in continuation will be delivered by Professor Foster on "the Development of the Chick in the Egg;" four lectures by Professor Odling, F.R S., on "Chemical Combination;" and four lectures by Professor Bain, on "Popular Errors."

Death of the Earl of Rosse.—With regret we announce the death of the Earl of Rosse, who expired, after a lingering illness, at Birr Castle, King's County, on the 31st October, in the sixty-seventh year of his age. His name will always be associated with astronomical research and discovery, and with the gigantic reflector known by his name. He was President of the Royal Society in 1849, and he held the Chancellorship of the University of Dublin two years before his decease.

Sulphite of Uranid, Double-salts of.—L. Scheller. Pure oxide of uranid (uranid=U''''=240) prepared according to Malaguti's method by heating an alcoholic solution of uranic nitrate and washing the residue, was suspended in water, and sulphurous acid gas passed through until all was dissolved. On adding to this solution potassic, sodic, or ammonic disulphite, crystalline precipitates are obtained of the following compositions:—UK. NHSΘ_3, UNa,HSΘ_3, and UNH_4.HSΘ_3. They are difficultly soluble in water, but dissolve readily in sulphuric acid.—(*Zeitschr. Chem.* N.F. iii. 522.)

Experiments in Electrolysis.—It has generally been inferred that the power of nitro-hydrochloric acid as a solvent for gold and platinum is owing to the evolution of free chlorine. The proof of the inference has been this:—When aqua-regia is heated until no more chlorine is evolved, the residual liquid is "found to be a solution of hydrochloric and nitrous acids that is incapable of dissolving gold."—*Turner.* In experimenting on the electrolisation of compounds the other day, it occurred to me that this hypothesis is capable of decided proof; and this was the series of experiments. (1). Into an ordinary apparatus for the electro-chemical decomposition of water, having platinum electrodes, a weak solution of hydrochloric acid was poured. Over the anelectrode a glass tube was placed, and in this tube some gold leaf. Twelve pairs of Wollaston's double coppers were employed excited by dilute sulphuric acid only. On completing the circuit, the penetrating odour of chlorine was very perceptible, and in a few seconds the gold in the tube over the anelectrode was completely dissolved; as also were some fragments that had been put into the solution outside the tube. (2). If chlorine has this power over gold, it may be supposed that the chloride of either a metal or an alkali, providing that the compound is an electrolyte, will exhibit, on electrolysation, the same result. Chloride of sodium was the substance first experimented with. A saturated solution of the salt was made, and with precisely the same arrangement as before, the gold in the tube over the anelectrode was speedily dissolved. (3). The same result was obtained on electrolysing a solution of chloride of ammonium and chloride of barium. By a power of 20 pairs of Wollaston's double coppers the gold was dissolved with a rapidity equal to that when a solution of chloride of sodium was the liquid electrolysed. Both times the blue colour of litmus was quickly discharged, but there was no previous reddening of the colouring matter to indicate the generation of hydrochloric acid. (4). A solution of chlorate of potassa was the liquid next electrolysed. With the same power of 20 plates the gold was very gradually dissolved, though the battery was in good action. The odour of chlorine was perceptible, though fainter than in the former experiments. A solution of litmus was poured into the vessel, and a tinge of red was then perceived at the anode, owing to the action of the evolved chloric acid upon the colouring matter. The blue colour of the solution became fainter by degrees, evidently proving that since chloric acid does not possess bleaching properties, free chlorine was evolved. Possibly this formation of chlorine from chloric acid is a secondary result of the current; but it is quite as probable, and more so, that the chlorate of potassa and the chloric acid were successively decomposed by the current of electricity. I am not aware that the dissolution of gold, and the influence of chlorine over the metal, has been shown in this way before. True, Davy has proved that nitro-hydrochloric acid does not dissolve gold unless free chlorine is developed. Mr. Grove, also, has shown the action of chlorine liberated by the voltaic current; but in a different way. Two strips of gold leaf, one in nitric, the other in hydrochloric acid, in contact through a porous division, were connected by a gold wire: the hydrochloric acid was decomposed, and the gold in it immediately dissolved. The experiments now made may not possess the less interest because they refer to a foregone conclusion, and show that by the decomposition of other compounds of chlorine besides hydrochloric acid the precious metals may be dissolved.—*Ernest W. Bartlett.*

The Pascal-Newton Forgeries.—Sir David Brewster has forwarded the following letter to the *Times:*—"As the French Academy of Sciences is now convinced that the Pascal and Newton Letters are forgeries, it has become an object of interest to discover the name of the forger, the time when he executed his work, and the motives by which he was influenced. That M. Pierre Desmaizeaux, a Frenchman resident in London, was the author of these forgeries, will appear from the following considerations:—1. Desmaizeaux resided in England between the years 1692 and 1745, the year of his death. He was a Fellow of the Royal Society, and was intimately acquainted with Newton and with the leading scientific men of the day. He was a contributor to the *General Dictionary*, as is stated in the preface to that work, and he possessed that knowledge of physical science which appears in the correspondence between Pascal and Newton. 2. Desmaizeaux's work entitled *Recueil de Diverses Pieces, etc., par Leibnitz, Clark, et Newton*, several portions of which appear in the forged letters of Newton, connect him in a peculiar manner with the forgery. 3. Desmaizeaux is the most important personage in the fabricated documents—the hero in the romance so ingeniously composed to transfer the discoveries of Newton to his countryman. He is himself the author of six of the letters published by M. Chasles, and no fewer than nine are addressed to himself by some of the most distinguished writers of the day. 4. Desmaizeaux's poverty adds to the evidence of his being the forger. He lived chiefly by his writings. He was employed by Dutch booksellers to send them literary news from England. In a letter to a nobleman, in 1732, he states 'that he was reduced to a pension on the Irish Establishment, which brought him £40 a year.' . . . 'After 40 years stay in England, and in an advanced age, I find myself and family destitute of a sufficient livelihood, and suffering from complaints in the head and impaired sight by constant application to my studies.' 5. Desmaizeaux's character, both in its religious and moral aspect, was quite consistent with his criminality as a forger and a systematic slanderer of Newton. 'He was a great man,' says Mr. Disraeli, 'with those who are pleased to be called Free-thinkers, particularly with Mr. Anthony Collins, and collects passages out of books for their writings.' Anthony Collins, who was a great friend of Locke, placed such confidence in Desmaizeaux that he bequeathed to him eight octavo volumes of his manuscripts, 'in order,' Disraeli says, 'that they might be secured from the common fate of manuscripts.' In an unguarded moment, however, he relinquished this precious legacy of the manuscripts, and accepted 50 guineas as a 'present' from Mrs. Collins, who, it is supposed, threw them into the fire. 6. A large portion of the forged correspondence, embracing 120 letters from Newton, and 88 letters and notes of Leibnitz, was in Desmaizeaux's house at the time of his death in 1745, and either he himself or his family sold it for £800 to a celebrated collector of manuscripts. During the interval between 1734 and 1740 he had no doubt good employment as a contributor to the *General Dictionary*, and it is therefore probable that he spent the last five years of his life in the difficult work of composing the Pascal and Newton Correspondence. That his motive was to calumniate Newton, who was his friend, and exalt Pascal, who was his countryman, is by no means probable. In 1743, two years before his death, he had, as Disraeli tells us, 'procured his pension to be placed on his wife,' and there can be little doubt that his crime against Newton, like his crime against Collins, had no other object than to make a provision for his family."

Transparency of Molten Metals.—The assertion of Secchi, a few months ago, regarding the transparency of heated iron, has given rise to much talk; but we have not yet seen it confirmed by the statement of any other competent eye-witness. Meanwhile, however, many assertions have been made as to the transparency of metals when melted; and the evidence on this point begins to stagger the *savans*. It so happens, that, so far as we are aware, no professional chemist or educated physicist has yet testified to the phenomenon as actually observed by him. It is merely said, in various quarters, to be a fact, well known to the workmen employed in melting and moulding certain metals. We deem it, therefore, important to mention the first authentic endorsement which has come to our notice. M. Paul Morin, the accomplished chemist in charge of the Aluminium Bronze Works near Paris, asserts that the melted alloy, when poured into the mould, is transparent; and Mr. T. Sterry Hunt, to whom the assertion was made, and who saw the operation performed, assures us that the appearance of the molten stream seemed to corroborate the statement. There is a possibility of optical illusion in the inspection of a body which is itself intensely luminous, to discover whether it is transparent. We suggest that the question may be easily settled by the means employed to show the transparency of ordinary flame, namely, by burning magnesium, or in some other way producing a more brilliant light, behind it. The aluminium bronze is remarkable for two things, among other qualities, which distinguish it from ordinary alloys. One is the intense temperature developed by the union of the two metals, and the other is the extreme fluidity of the molten compound. Perhaps these qualities may be connected with the alleged phenomenon of transparency. Copper may also be transparent in the liquid state; but, in pouring it into moulds, it often oxidizes very rapidly; and the whole liquid mass is believed to be filled with disseminated particles of the red oxide of copper, which is opaque. Whether from this cause or not, we cannot say, but the evidence as to the transparency of molten copper, as likewise in the case of other metals, is still conflicting and inconclusive.—*American Journal of Mining.*

Composition and Quality of the Metropolitan Waters in October, 1867.—The following are the Returns of the Metropolitan Association of Medical Officers of Health:—

Water Companies.	Total solid matter per gallon.	Loss by Ignition.*	Oxidisable organic matter.†	Hardness.		Organic and other ammonia.
				Before boiling	After boiling	
Thames Water Companies.	Grains.	Grains.	Grains.	Degs.	Degs.	Grains.
Grand Junction...	19·67	1·00	0·56	13·5	4·5	0·004
West Middlesex...	16·67	0·85	0·52	12·5	4·0	0·004
Southwark and Vauxhall........	18·50	1·00	0·59	13·0	4·5	0·004
Lambeth..........	19·00	1·25	0·60	13·5	4·5	
Other Companies.						
Kent..............	27·50	0·75	0·26	18·0	7·5	0·000
New River.......	17·00	0·25	0·31	12·0	3·0	0·001
East London......	19·20	0·50	0·49	13·0	4·5	0·001
Surface Wells in City.						
Idol Lane (Church)	81·33	2·00	0·06	32·0	16·0	0·004
Leadenhall Street..	120·00	11·51	1·51	50·0	—	1·281
Dunning's Alley..	94·67	8·00	0·96	36·0	—	0·008

* The loss by ignition represents a variety of volatile matters, as well as organic matter, as ammoniacal salts, moisture, and the volatile constituents of nitrates and nitrites.

† The oxidisable organic matter is determined by a standard solution of permanganate of potash—the available oxygen of which is to the organic matter as 1 is to 8; and the results are controlled by the examination of the colour of the water when seen through a glass tube two feet in length and two inches in diameter.

Encouragement of Chemistry in France.—Dr. Quesneville writes in *Le Moniteur Scientifique*: "We have just heard two pieces of bad news. The first, which is irremedial, is the death of Millon, that distinguished chemist, whose career was cut short and health impaired by being sent to pass the best years of his life in Algeria, because his liberal opinions were too advanced to permit his remaining in Paris. The second is the arrest of our distinguished colleague M. A. Naquet, *Proféssеur Agrégé à la Faculté de Médecine*, which was doubtless owing to the same cause."

The Frescoes in Westminster Palace.—Mr. Ward has been recently engaged in the reparation of his pictures in fresco and water-glass, which fill panels in the Commons Corridor (these had, in more than one case, been affected by unknown and variously effective causes), and has succeeded to his satisfaction, so far as the experiment permitted. After cleaning the works with bread, they were coated with gelatine size, and the artist repaired the affected part with pure water-colour, which embodied with the size and formed distemper. The general appearance of the picture being thus restored, a coating of a new composition, consisting of benzole and paraffin, was applied to some of the parts which required additional fixing, and had the effect of deepening the colours and enriching the tones, as varnish upon oil-painting, without the shining surface. Except in one or two cases, this latter application was not made to the heads. This new mixture has been extensively employed on the pictures by Dyce in the Queen's Robing Room, which had suffered from the scaling off of portions of their surfaces, and, as we have witnessed, with remarkably good effect, that will be, we trust, permanent. The composition by Dyce, called "Courtesy," has been entirely covered thus. The large picture called "Hospitality" has not yet been so treated, and truly looks so brilliant and well in tone, that it will be superfluous to touch it. These pictures are pure frescoes; but the application may be made to distemper or size-painted works, as with those of Mr. Ward. The fluid is warmed, and used at a temperature of about 70°, the surrounding atmosphere being heated to that degree. Mr. Wright, chemist of Kensington, devised, and, in conjunction with Mr. Cope, R.A., perfected the composition in question, which has been employed, with the sanction of Dr. Percy on the part of the Government, upon his own pictures at Westminster whenever they required it. —*Athenæum.*

Magnetic Carbide Filters.—In the *British Medical Journal* of last week the following paragraph appeared: "Our excellent contemporary, the Chemical News, seems to doubt, in their remarks upon this Report on the Purification of the Hooghly Water for the Supply of Calcutta, the value of the method of filtration by magnetic carbide. We can assure the Chemical News that experience and experimental investigation fully bear out the statements of Mr. Spencer. That 'so little has been heard of this process during the seven years in which the author says he has had practical experience with it,' perhaps Mr. Spencer or his friends can best explain. And it is, indeed, true that, considering its importance, it is little known; for 'if,' as our contemporary remarks, 'the purifying material possess the virtues accorded to it by the discoverer, it is especially our duty to protest against anything less than the universal application of the process in this country.'" We regret that the purport of our remarks has been misunderstood, but at the same time we find no reason to qualify the opinions we expressed. Our objections took ground less against Mr. Spencer's *process*, than his *theories* explaining the action of magnetic carbide. Our talented contemporary is silent upon these, and would possibly hesitate, for instance, before allowing, with Mr. Spencer, that magnetic oxide of iron impregnated with carbon, in virtue of its magnetic nature, attracts into its pores the oxygen from atmospheric air, leaving the nitrogen behind. If Mr. Spencer can prove this it would be highly interesting, but at present evidence tends to an opposite conclusion, in fact the experiments of Faraday prove the contrary. Granting, for the sake of argument, that oxygen is attracted into the pores of magnetic carbide, what proof, or even probability is there of it being thereby converted into ozone? The extracts inserted in the article upon the water supply of Calcutta (Chemical News, *American Reprint*, *January*, 1868, *page* 4) must, we think, have shown our readers how much rested upon Mr. Spencer's *ipse dixit*, and how little upon experiment. It is not too much to say that if experiment bore out these statements they would possess extraordinary interest for the scientific world. With regard to the process itself we do not deny that magnetic carbide exerts a purifying action upon water, possibly greater than carbon. But as to its differing from carbon in the action not gradually declining after immersion in water, we can only say that experiments such as carry conviction to the minds of scientists are wanting, whilst the assertion is opposed to the existing opinions upon the subject.

The Star-Shower of November 14th next.—The geographical limits of visibility of the star-shower of 1866, coincide with the area over which the November meteors appeared in 1832. The latter shower was seen as far south as the Mauritius, as far east as Arabia and the Persian Gulf, and over the whole continent of Europe, with the British Isles, but it was not visible in America. It was, moreover, a moderate display, but it was followed, twelve months later in America, by the great storm of meteors which suddenly appeared on the morning of the 13th of November, 1833. The recent exhibition may therefore be regarded as the prelude of a similar meteor-rain in America, perhaps partially visible in Europe, as great and bright as the two star-storms seen in America, and partially visible in Europe, in the years 1799 and 1833. Unless unforeseen curvatures of the meteoric current disturb the geographical boundaries of the display, the first symptoms of the approaching star-shower will be perceived at day-break in England, on the morning of the 14th of November, 1867, when the light of the moon, then three days past the full, and of dawn appearing, will detract something from the numbers and brightness of the meteors. But the same oscillation of the curves in an opposite direction, it should be borne in mind, will bring Great Britain into full view of the centre of the shower, and make the principal spectacle of the meteors visible in Europe before day-break, as well as in America.—*A. S. Herschel.*

New Reflecting Telescope to be used at Melbourne, Australia.—The Rev. Dr. Robinson, F.R.S., gives the following description of the new Reflecting Telescope recently made for the Melbourne Observatory, in a letter to the President of the Royal Society. "As you express a wish to know my recent impressions respecting the great telescope, I can say that they are very satisfactory. When I saw it six weeks ago the first of the two great specula was just polished; and though the essential parts of the equatorial were in position, and one could estimate the facility with which it could be managed, the optical part of the telescope remained incomplete. Now, I found the great and small specula in their places, a finder of four inches aperture attached, the circles divided, and the clock for driving the telescope enshrined in the pier. One thing was wanting, weather fit for trying its power; and during eighteen nights there was only one of even middling goodness. That, however, was sufficient to prove that the instrument was thoroughly up to its intended work. I examined several nebula and clusters, with whose appearance in Lord Rosse's six-feet reflector I am familiar, and the difference was far less than I expected. I may specify among them 51 Messier, whose spirals were seen on strong aurora and the nebula in Aquarius, with its appendages like the ring of Saturn. Its definition of stars is very good; *a* Lyræ had as small and sharp an image as I ever saw on such a night; and a few pretty close double stars were well and clearly separated. Part of this is probably due to the lattice-tube, which permits the escape of heated air, but more to the figure of the speculum, which is truly parabolic. The peculiar nature of the mounting brings the circles completely within reach of the observer's assistant; and the mechanical appliances for the motions in right ascension and polar distance are so perfect, that we set the instrument on the faint objects which we were examining with great facility and rapidity. One man can reverse the telescope in a minute and a quarter; the quick motion in polar distance is of course far easier, and the slow one acts more like the tangent screw of a circle than the mover of such a huge mass. The clock is rather gigantic, but does its work with great precision, the objects which I examined remaining steady on the wire as long as I watched them; and there is an ingenious and new contrivance for suiting its speed to planets or the moon. There remain but a few matters to be completed; the second great speculum is nearly polished, the glass small one is ready; the micrometer and observing-chair are not commenced, nor the photographic apparatus and spectroscope. These two last are no part of Mr. Grubb's contract; but the committee thought themselves justified by the correspondence in ordering them, as their cost is small, and they will add greatly to the utility of the telescope. In the fine sky of Melbourne it will, I trust, yield spectroscopic results surpassing any that have as yet been obtained. That it will realize fully the expectations of the people whose enlightened liberality has ordered its construction I am quite certain; but I am not so certain that it will retain its present perfection very long if exposed without some shelter. It is true that Mr. Cooper's great achromatic has stood exposed to the rain and wind of Connaught for more than thirty years, and is still serviceable; but besides its inferior size it is of coarser workmanship, and is provided with fewer of those beautiful contrivances which in this instrument make its movements so easy. At Melbourne the rain of Markree is not to be feared; but if one may judge from its position on the verge of a great continent, and from the analogy of India and the Cape, another enemy is to be dreaded, the fine dust which winds from the interior will probably bring. This would find its way into all the bearings, and besides clogging their action would grind them out of truth. The danger of this induces me, after careful discussion with Messrs. Le Sueur and the two

Grubbs, to lay before you my views, which (if you think them sound) you may hold it advisable to mention to the authorities of Victoria. Three modes occur to me of covering the telescope. In any case it must be surrounded by a wall, for the comfort of the observer and to prevent intrusion. This wall may support a movable covering of such a kind as to let the instrument be pointed to every part of the sky. The most usual form of this covering is a dome running on a circular railway, and with an opening or chase on one side reaching from its base to its summit, and closed by a sliding shutter. The disadvantages of this plan are, that the performance of the telescope is somewhat injured by currents of warm air rising through the chase, and that it is much heavier and more costly than either of the others. In this instance its diameter could not be less than 56 feet; and though that magnitude is not beyond the resources of an accomplished engineer, yet it is not one to be encountered without the prospect of some adequate advantage. The largest dome which I know (Sir James South's, of 36 feet diameter) is a total failure; but this does not weigh much with me; for, though planned by the celebrated Brunell, it transgresses against the elements of mechanical science. A much simpler plan is the sliding roof. In this case the walls are rectangular, enclosing a space rather broader than the instrument, and about three times as long. The longer sides carry two rails, on which runs a kind of house long enough to cover the instrument and pier, and high enough to clear the latter. That end which at Melbourne will be its north is closed by doors, which are opened at the time of observation, and the roof is wheeled away, leaving all in the open air. It will be the cheapest and least bulky of the three. Its defects are, that the open end presents some engineering difficulty, that the roof will hide about 12° under the pole, and that the whole machinery is exposed to any dust that may be stirring during the hours of observing. That which appears the best is the revolving roof. Its vertical part is a prism of sixteen sides, six feet high, springing from a ring of cast iron which revolves by rollers on a circular rail borne by the wall. The top is nearly flat, with a chase large enough to let the telescope work freely, which can be covered by sliding shutters. The tube, when in use, would project through the chase, and be essentially in free air, at other times could be lowered and completely sheltered; while the other parts would be as well protected as under a dome. In this case the internal diameter should be about 46 feet, with a chase 16 feet wide. These dimensions would give complete command of the heavens, and such a roof would give less hold to a high wind than either of the others. I enclose a rough sketch of its framing. The panels and the three girders at the top to be of angle-iron, light but strong, and these covered with tin plate. If it were adopted, I suppose the frame would be made here, sent out in pieces, and put together and covered on its arrival. The weight would be about 5 tons. As to its cost, no estimate can be given, as labour costs more at Melbourne than with us; but in Ireland it would be about £1,200. I will conclude this long letter by telling you how much I am satisfied with our selection of the astronomer who is to work this glorious instrument. He is not a mere mathematician; such a one might be very helpless when he came to the practical details of observing, but he is thoroughly versed in its optical and mechanical requirements, and in the daily work of an observatory. For this last he has been trained by Professor Adams, during the past year; one of the Committee, Mr. Warren De la Rue, the first of celestial photographers, has instructed him in the mysteries of that surprising art; and for the last three months he has been constantly in Mr. Grubb's works, studying all the mechanism of the telescope (of which I see he has acquired full command), and taking an active part in the polishing of the great specula. He seems fully to understand this most delicate process; and it is my opinion that, if repolishing becomes necessary, he is fully competent to do it successfully. I may therefore congratulate you in full hope on the inestimable harvest of discovery and triumph which will soon crown this magnificent enterprise."

Professor Naquet.—M. Naquet, the distinguished Professor of Chemistry of the Paris University, who was lately arrested for political reasons, has been transferred from Mazas to a *maison de santé*, on the intervention of Dr. Wurtz, the Dean of the Faculty of Medicine. The very valuable work of M. Naquet has lately been translated into English and published by Mr. Cortis, of Guy's Hospital. M. Naquet is a great favourite with the students, and a very accomplished man of science. His arrest for political reasons has caused a sensation of deep pain in the faculty and schools of Paris.—*British Medical Journal.*

The Royal Society.—At the anniversary meeting on November 30, 1867, the following elections took place: *President.*—Lieut.-General Edward Sabine, R.A., D.C.L., LL.D. *Treasurer.*—William Allen Miller, M.D., LL.D. *Secretaries.*—William Sharpey, M.D., LL.D.; George Gabriel Stokes, Esq., M.A., D.C.L., LL.D. *Foreign Secretary.*—Prof. William Hallows Miller, M.A., LL.D. *Other Members of the Council.*—Frederick Augustus Abel, Esq.; William Benjamin Carpenter, M.D.; Prof. A. Cayley, LL.D.; J. Lockhart Clarke, Esq.; John Evans, Esq.; Capt. Douglas Galton, C.B.; John Peter Gassiot, Esq.; John Hall Gladstone, Esq., Ph. D.; Sir Rowland Hill, K.C.B., D.C.L.; William Huggins, Esq.; Thomas Henry Huxley, Esq., Ph. D.; Prof. John Phillips, M.A., LL.D.; Prof. Andrew Crombie Ramsay, LL.D.; Colonel William James Smythe, R.A; Lieut.-Col. Alexander Strange; Thomas Thomson, M.D.

CONTEMPORARY SCIENTIFIC PRESS.

[Under this heading it is intended to give the titles of all the chemical papers which are published in the principal scientific periodicals of the Continent. Articles which are merely reprints or abstracts of papers already noticed will be omitted. Abstracts of the more important papers here announced will appear in future numbers of the Chemical News.]

Journal für Praktische Chemie. No. 12. 1867.

A. Muller: "*Colorimetrical Studies on Sulphate of Iron.*"—A. Muller: "*On some Chromatic Relations between Solutions of Annatto, Acetate of Iron, and Bichromate of Potash.*"

Bulletin de la Société d'Encouragement. May, 1867.

Caultier de Claubry: "*Report on Guenier-Lauriac's Method of Casting Rolls and other objects with a Skin of Hard Iron.*"

Mittheilungen des Gewerbe-Vereins für Hanover. Nos. 1 and 2. 1867.

F. Kern: "*On a new Vegetable Fibrous Material known as* Kellenfilster, *for stuffing Furniture.*"—W. Liecke: "*Some Methods of Preserving Cast Iron.*"—W. Liecke: "*On Coating Cast Iron with other Metals in the Wet Way.*"—H. Grothe: "*On the Amount of Heat radiated from Steam Pipes, and on the Use of a Jacket for preventing such Radiation.*"—Bergmann: "*On the Pollution of Wells by Cesspools and Privies.*"—W. Liecke: "*Notes on Glycerine.*"

Le Technologiste. July, 1867.

Oudemans: "*On the Specific Gravity of Acetic Acid and of its Aqueous Solutions.*"—F. Junkmann: "*Improvements in the Manufacture of Beetroot Sugar.*"—H. Robertson: "*On a new Distilling Apparatus for the Extraction of Mineral Oils.*"

Dingler's Polytechnisches Journal. July, 1867. No. 1.

Blazek: "*On a Means of Establishing Telegraphic Communication between Two Places without the use of a connecting Wire.*"—J. L. Fromont: "*On Ammonia Vapour Pumps.*"—C. Bischof: "*Analysis of and Pyrometric Experiments on Grünstadt Fire-clay.*"—G. Schnitzer: "*On Citric Acid.*"—M. Rosler: "*On the Manufacture of Indigo Carmine.*"—H. Deville: "*On the Use of Glycerine for preventing the Adhesion of Mercury to the Glass Tubes of Steam Gauges.*"

July. No. 2.

H. Scheffler: "*On the Use of the Thermometer for preventing Boiler Explosions.*"—L. Rinman: "*On the Presence of Nitrogen in Steel and Pig Iron, and on the Condition of Carbon in Hard and Soft Steel.*"—H. Wagner: "*On the Manufacture of Baryta and its Salts.*"—M. Rosler: "*On the Manufacture of Nitrate of Iron for Use as a Mordant.*"—Chevallier: "*A Method of rendering Mortar* *capable of resisting the Action of Rain by the Addition of Coal Dust thereto.*"—Chevallier: "*On the Use of a Solution of Amber in Bisulphide of Carbon as a Cement.*"—E. Sostmann: "*On the Solution of Gypsum by Saccharine Solutions.*"—H. Rust: "*On the Discrimination of true Creosote and the so-called Coal Tar Creosote (Carbolic Acid).*"

Comptes Rendus. August 19, 1867.

M. Charles: "*On the Pascal Correspondence.*"—Chevreul: "*On the Pascal Correspondence.*"—Schultz-Schultzenstein: "*Researches on Animal Electricity.*"—C. Ozanam: "*On the Representation of the Beats of the Heart and the Pulse by means of Photography.*"—Conte: "*Supplement to the Author's Memoir on the Diseases of the Vine, published in the 'Comptes Rendus' of August 12.*" C. Grad: "*On the Temperature of the Rivers Rhine, Ill, and Fecht.*"—H. Schiff: "*On the Monamines derived from the Aldehydes.*"—L. Cailletet: "*On the Influence of Coloured Light on the Decomposition of Carbonic Acid by Plants.*"—Coulvier-Gravier and Chapelas-Coulvier Gravier: "*On the Shooting Stars of the 9th, 10th, and 11th of August, 1867.*"—Jullien: "*Letter to Chevreul on Capillary Affinity.*"—Chevreul: "*Reply to Jullien's Letter on Capillary Affinity.*"

August 26.

E. Blanchard: "*Remarks on the Pascal Correspondence.*"—Charles: "*Remarks on the Pascal Correspondence.*"—Regnault: "*Remarks on the Pascal Correspondence.*"—Balard: "*Remarks on the Pascal Correspondence.*"—Chevreul: "*Remarks on the Pascal Correspondence.*"—A. W. Hofmann: "*On a new Series of Homologues of Hydrocyanic Acid.*"—Faugere: "*On the Authenticity of the Pascal Correspondence.*"—Prat: "*Researches on the Chemical Constitution of Fluorine Compounds, and on the Isolation of Fluorine.*"—Chevreul: "*Remarks on the Foregoing Paper.*"—Pool: "*Second Note on an Explosive Compound obtained by treating Glue with Chlorate and Nitrate of Potash.*"—Leon: "*On the Changes in the French Monetary Standards occasioned by the Introduction of the Decimal System (Second Note).*"—Lippmann and Louguinine: "*On the Synthesis of Di-Ethyl-Toluol.*"—M. Simpson: "*On the Formation of Succinic Acid from Chloride of Ethylidene.*" A. Oppenheim: "*New Researches on the Isomerism of Protochloride of Allyl and Monochlorinated Propylene.*"—J. Chmoulevitch: "*Researches on the Influence of Heat on the Mechanical Work of the Muscles of the Frog.*"—R. Radau: "*On a Self-registering Meteorological Apparatus invented by Magellan in 1782, and on the Theory of the Static or Steel-yard Barometer.*"

Bulletin de l'Académie Royale de Belgique (Classe des Sciences).

July 6, 1867.

Kekule: "*Report on T. Swart's Memoir on the Derivatives by Addition of Itaconic Acid and its Isomers.*"—Kekule: "*Report on C. Glaser's Researches on some Derivatives of Cinnamic Acid (Part 2).*"—Stas: "*Report on the two previous Memoirs.*"—Melsens: "*Researches on Yeast and on the Fermentation of Beer.*"—A. Quetelet: "*On a Meteor observed on the 11th of June, 1867.*"—A. Quetelet: "*On a Remarkable Thunderstorm at Ghent on the 2nd-3rd of June, 1867.*—T. Swartz: "*On the Derivatives by Addition of Itaconic Acid and its Isomers (Part 2).*"—C. Glaser: "*Researches on some Derivatives of Cinnamic Acid (Part 2).*"

Sitzungsberichte der Kaiserlichen Akademie der Wissenschaften zu Wien. (Mathematisch-naturwissenschaftliche Classe.)

March, 1867.

H. Hlasiwetz: "*On Hydrocaffeic and Hydroparacoumaric Acids.*"—A. Rollett: "*On the Effect of Contrast on Colours.*"—J. Seegen: "*On the Separation of Nitrogen from Albuminoid Substances in a Dog.*"—V. von Lang: "*On the Optical Properties of Crystallised Compounds of Ammoniacal Bases, and of Salts of Thallium, Rubidium, and Cæsium, with respect to Homologous and Isomorphous Series.*"—A. Rollett: "*On the Theory of Accidental Colours, and of the Change in the Colour of Accidental Images.*"

April.

M. Erofejeff: "*On the Determination of the Principal Index of Refraction of Sulphate of Ammonia.*"—V. von Lang: "*An Improved Apparatus for Measuring the Optic Axes of Crystals.*"—W. von Haidinger: "*On J. Schmidt's Observations on Meteoric Stones, and on the Ridges of the Surface of the Moon.*"—O. Rembold: "*On Quino-tannic Acid.*"—O. Rembold: "*On Quinova-tannic Acid.*"—A. Grabowski: "*On Rhatania-tannic Acid.*"—G. Malin: "*On Filico-tannic Acid.*"—A. Grabowski: "*On Filicic Acid.*"—O. Rembold: "*On the Tannic Acid of the Root Bark of the Pomegranate Tree.*"—H. Hlasiwetz: "*On the Relations between the Tannic Acids, Glucosides, Phlobaphenes, and Resins.*"—E. Brucke: "*On the Action of Boracic Acid on Living Muscular Fibre.*"—E. Klein and E. Verson: "*On the Importance of Salt as an Article of Human Food.*"

Sitzungsberichte der Königlich-Bayerischen Akademie der Wissenschaften zu München. (Mathematisch-physikalische Classe.)

February 9, 1867.

M. von-Pettenkofer and Voit: "*On the Carbonic Acid exhaled and Oxygen consumed by Man.*"—Von Kobell: "*On the Behaviour of Disthene in the Stauroscope, and on the Permanence of the Cross observed under such Circumstances.*"—A. Vogel: "*On the Estimation of Fat and Albumen by a Method based on the Principle of the Optical Milk Test.*"

Jahrbuch der Kaiserlich-Königlichen Geologischen Reichsanstalt.

January–Febuary, 1867.

V. von Zepharovich: "*On Fluorite from Hieflau, Styria.*"—F. Bauer: "*Note on the Present Condition of the Ore-Washing Establishments of Schemnitz.*"—K. von Hauer: "*Analysis of Brown Coal, Lignite, Copper, Slag, and Iron Ores.*"

Poggendorff's Annalen der Physik. No. 6. 1867.

R. Bunsen: "*On the Temperature of the Flames of Carbonic Oxide and Hydrogen.*"—A. Toppler: "*Optical Studies by the Author's new Method.*"—B. Riemann: "*Contributions to Electrodynamics: On the Connection of the Laws of Electricity and Magnetism with those of Light and Radiant Heat.*"—L. Lorenz: "*On the Identity of Luminous Vibrations and Electric Currents.*"—C. Rammelsberg: "*On the Phosphites.*"—H. W. Schroder van der Kolk: "*On the Mechanical Energy of Chemical Action.*"—F. Zirkel: "*On the Microscopical Constitution of the Phonolites.*"

Annalen der Chemie und Pharmacie. August, 1867.

E. von Gorup Besanez: "*Researches on Rhenish Beechwood Tar Creosote.*"—E. Menzner: "*On the Salts of Sulphophenic Acid.*"—Glutz: "*On some Chlorinated Derivatives of Phenol.*"—Glutz: "*A simple Method of Preparing Chlorosalylic Acid.*"—T. Meves: "*On Oxyethylene-sulphurous Acid, and on a new Method of Forming Isathionic Acid.*"—T. Meves: "*Researches on some Salts of Cyanacetic Acid.*"—R. Otto: "*On the Bye-Products obtained during the Preparation of Benzolsulphurous Acid.*"—R. Otto: "*Note on the Preparation of Cyanurate of Sulphobenzide.*"—H. Hubner, J. Ohly, and O. Philipp: "*On the Isomerism of the Aromatic Acids.*"

Journal für Praktische Chemie. No. 13. 1867.

G. Merz: "*On a Method of Intensifying the Light given out by Sulphur when burnt in Oxygen.*"—"*An Improved Method of Exploding Mixtures of Hydrogen and Oxygen.*"—"*A Method of showing the different Temperatures at which various Gases take Fire.*"—"*An Instructive and Simple Experiment on Diffusion.*"—"*A Method of collecting the Products of Combustion of Gun Cotton, and of demonstrating the Presence therein of Peroxide of Nitrogen and Carbonic Oxide.*"—"*A Method of Demonstrating the Production of Carbonic Oxide during the Decomposition of Carbonic Acid by Incandescent Charcoal.*"—"*On the Behaviour of Carbonic Acid towards Water at High Pressures.*"—"*A Method of showing the Variation in the Illuminating Power of Flames when separate and when combined into One Light.*"—"*On the Combination of Hydrogen and Chlorine under the Influence of the Magnesium Light.*"—"*On the Use of Glycerine for Preserving the Gelatinous Mass obtained by treating Bone with Dilute Acids.*"—"*Apparatus for Showing the Igniting Points of Various Substances.*"—"*A Method of Preparing Starch Papers for the Detection of Iodine.*"—"*A Method of Demonstrating the Formation of Chloride of Sodium by the Action of Sodium on Hydrochloric Acid.*"—"*On the Property possessed by Bichromate of Ammonia of Assuming, when heated, an appearance resembling Tea Leaves.*"—"*On the Chemical Toy known as 'Chinese Grass Paper.'*"—"*On the Increase in the Explosive Power of Gun Cotton and Gun Paper when Treated with Bichromate of Potash.*"—"*On the Fluorescence of Uranium Glass by the Magnesium Light.*"—"*On a simple Method of Preparing Ferrate of Potassium.*"—"*On a rapid Method of Preparing a Solution of a Salt of Manganese.*"—"*A Method of Distinguishing the Yellow Sublimate of Oxide of Lead from that of Oxide of Bismuth obtained on Charcoal before the Blowpipe.*"—"*An Experiment showing the rapid Reduction of Heated Oxide of Copper to Metallic Copper by means of Alcohol.*"—"*On a Method of causing the Ignition of Illuminating Gas by Spongy Platinum.*"—"*On the Fluorescence of certain Coloured Glass when viewed by Reflected Sunlight.*"—"*A Method of Imitating Red Glass.*"—"*On a Method of showing the Different Colours of Solutions containing small quantities of freshly precipitated Gold.*"—Boettger: "*On the Crystallisation of Supersaturated Solutions of Acetate of Soda.*"—"*On a new and delicate Test for Alkalies and Alkaline Earths.*"—"*On some new Voltaic Batteries.*"—"*On the Occurrence of Teroxide of Thallium during the Electrolysis of Thallium Compounds, and on the Possibility of using that Body for the Manufacture of Matches.*"—"*On the Use of Solutions of Silicate of Soda for Producing Arborescent Crystals of Metallic Salts.*"—"*On the Action of Lead on Distilled Water.*"—J. Dogiel: "*On the Presence of Volatile Fatty Acids in the Gall.*"—G. Merz: "*On the Volumetric Estimation of Acetic Acid.*"—F Gauhe: "*Is the Product of the Action of Iodide of Phosphorus on Aqueous Picric Acid, Iodide of Pikrammonium, or Triamidophenol?*"

No. 14.

C. F. Schonbein: "*On the Presence of Ozone in the Atmosphere.*"—Fritzsche: "*On the Solid Hydrocarbons of Coal Tar.*"—F. Beilstein and U. Kreusler: "*On Para-Nitrotoluylic Acid and its Derivatives.*"—C. F. Barford: "*On the Isomerism of the Stannic Acids.*"

No. 15.

E. Mulder: "*On Trisulphocarbonic Acid Acetonium.*"—F. Goppelsroder: "*On a Fluorescent Substance obtained from Cuba Wood.*"—J. C. Igelstrom: "*On the Analysis of some Minerals from Wermland, in Sweden.*"

Bulletin de la Société d'Encouragement. June, 1867.

H. Bouillet: "*On the Production of Busts and Statues by the Electrotype Process.*"—"*On the Electro-deposition of Gold of various Colours.*"—Stoss: "*On the Use of Oxide of Chromium for Polishing Metals.*"—Balard: "*On E. Carré's Improved Machine for the Manufacture of Ice by the Absorption of Aqueous Vapour by Sulphuric Acid in a Vacuum.*"—Dumas: "*On the same Subject.*"—Thenard: "*On the Preservation of Milk by the Application of Cold, à propos of the same Subject.*"—Peligot: "*On the Flexibility of Glass.*"—De Luynes: "*On the Colouring Matters of Orcine, and on a Product derived therefrom analogous to Carthaminic Acid.*"—Isambert: "*On two Improved Magnesium Lamps.*"—Dumas: "*On a Specimen of Anthracite or Black Diamond of remarkable Hardness.*"—Bouis: "*On De Milly's Improvements in the Manufacture of Fatty Acids.*"—H. Monier: "*Improved Crystal Gas Burners.*"—J. Agnellet: "*On the Foundation of a Prize for an Improved Constant Battery.*"—Dumas: "*On the Manufacture of Tartaric Acid.*"

L'Invention. August, 1867.

M. Milan: "*A Process for extracting Silver from Lead.*"—Rave: "*On a new Aniline Black for the Manufacture of Printing Ink.*"—Schlumberger: "*On the Preparation and Use of a New Green Colouring Matter extracted from Toluidine.*"—Piedaul: "*On the Use of a Solution of Fatty Matters in a Hydrocarbon or in Bisulphide of Carbon for preserving and softening Leather.*"—P. Javal: "*On the Preparation and Use of Certain Fatty Bodies.*"—Mallet: "*On the Production of Oxygen and Chlorine Gas, either together or separately, from Sub-chloride of Copper.*"—Schlumberger: "*On the Preparation of some Blue and Violet Colouring Matters from Rosotoluidine.*"

Dingler's Polytechnisches Journal. August, 1867.

A. Ott: "*On Lugo's Apparatus for Distilling Petroleum.*"—H. W. Ilgen: "*On the Use of Refuse Grape Skins for manufacturing Gas and Lamp Black.*"—"*On the Manufacture of Plastic Charcoal for Filters.*"

Kunst und Gewerbeblatt. June, 1867.

R. Wagner: "*Report on the Chemical Products at the Paris Exhibition.*"—E. Dieterich: "*On the Preparation of Indigo Carmine.*"—R. Wagner: "*On the Detection of Paraffin in Samples of Wax by a Comparison of their relative Specific Gravities.*"

Journal des Fabricants de Papier. July 15, 1867.

E. Bourdilliat: "*On Testing the Chemical Products used in Paper-making. Continuation: Sulphate of Iron.*"

Revue Universelle des Mines. March—June, 1867.

L. Perard: "*On the Conservation of Force.*"—P. Kupfferschlaeger: "*On some Modifications of Margueritte's Process for the Volumetric Estimation of Iron.*"—Grundman: "*On the Decomposition of Coals when exposed to the Air.*"

PATENTS.

Communicated by Mr. Vaughan, F.C.S., Patent Agent, 54, Chancery Lane, W. C.

GRANTS OF PROVISIONAL PROTECTION FOR SIX MONTHS.

2828. A. Ticozzi, Bedford Street, Bedford Square, Middlesex, "An improved preparation or lotion for diseases of the eyes."

2832. J. Player, Glaisdale, Yarm, Yorkshire, "Improvements in the manufacture and refining of iron and steel."—Petition recorded, October 8, 1867.

2848. T. Blackhurst, Conway Street, Birkenhead, Cheshire, "A new or improved composition to be used to prevent the oxidation of iron, the fouling of ships' bottoms, and other submerged things, and preserving wood from decay and from worms, and also preserving iron and wood exposed to the action of the atmosphere."—October 10, 1867.

2852. A. M. Clark, Chancery Lane, "Improvements in the preparation or treatment of extracts of madder for dyeing and printing purposes, and in apparatus used in such treatment."—A communication from G. A. Schaaff and G. E. Lauth, Boulevart St. Martin, Paris.—October 11, 1867.

2874. E. Leitenberger, Cosmanos, Austria, "Improvements in treating madder for the purpose of obtaining purpurine or alizarine from the same, and in apparatus to be made use of thereby."—October 12, 1867.

2549. F. Tolhausen, Boulevart Magenta, Paris, "An improved process and apparatus for instantaneously disinfecting fæcal and manuring matters, improving the same, and also rendering them fit for feeding domestic animals."—A communication from L. J. B. A. Lemoine, and A. M. Turrel, Paris.—Petition recorded September 9, 1867.

2893. A. Aitchison, Wilton Terrace, Peckham, Surrey, "Improvements in the treatment of hydrocarbons for the production of gas sufficiently permanent to be stored for lighting and heating purposes, and especially in the treatment of the hydrocarbons and other products obtained by distilling or carbonising coal, wood, and other carbonaceous materials, including those obtained in the manufacture of gas and in the distillation of coal-tar."

2894. T. H. Baker, and T. Woodroffe, Tonbridge, Kent, "Improvements in treating sewage or other liquid matters so as to purify the more fluid portions thereof, and recover some of the contained matters for use in manufacturing processes, and in the preparation of others for use as manures."—October 15, 1867.

2918. J. Bannehr, Exeter, "Improvements in apparatus for supplying deodorising matter to dry or earth closets, and in processes of and apparatus for treating the liquid portion of human or animal excreta after removal from such closets or from other receptacles."—October 17, 1867.

2657. J. Hargreaves, Appleton-within-Widnes, Lancashire, "Improvements in the manufacture of iron."—Petition recorded September 21, 1867.

2952. W. Crossley and T. C. Hutchinson, Middlesborough-on Tees, "Improvements in the manufacture of alumina and salts of alumina from blast furnace slag, or from other silicates of alumina containing an excess of lime or magnesia."—October 21, 1867.

2976. S. Welton, Grafton Street, Fitzroy Square, Middlesex, "Ozonised or oxygenated bread, biscuits, cakes, and other substances."—October 23, 1867.

2984. F. Gerharz, Cologne, Rhenish Prussia, "Improvements in the manufacture of manures and disinfectants."

2998. R. Weare, Compton. Staffordshire, "Improvements in and in apparatus for the treatment and for the reception of urine and fæcal matter."—October 24, 1867.

3006. W. R. Lake, Southampton Buildings, Chancery Lane, "Improved modes of and means for curing, drying, preserving, and packing meat, fruit, vegetables, and other perishable substances."—A communication from D. E. Somes, Washington, U.S.A.—October 25, 1867.

3032. J. Young, Aspull, Lancashire, "Improvements in the application of Cannel coal 'slack' to the manufacture of gas and coke."—October 28, 1867.

2919. J. Cubitt, Claremont Place, Brixton, Surrey, "An improved process and composition for the preservation from decay of stone, brick, plaster, metal, and other substances, and for hardening or glazing their surfaces, so as to prevent smoke or dirt from adhering to them."—Petition recorded October 17, 1867.

2997. C. W. Harrison, Oberstein Road, Clapham Junction, Surrey, "Improved means of preventing incrustation in boilers and other vessels in which water is heated."—October 24, 1867.

3051. G. Davies, Serle Street, Lincoln's Inn, Middlesex, "An improved mode of preventing incrustation in and removing it from steam boilers."—A communication from J. J. Allen, Philadelphia, Penn., U.S.A. October 30, 1867.

3064. W. S. Dixon, Bellisle, Ayrshire, N.B., "A new process of producing a blue dye or colouring matter."—October 31, 1867.

3105. J. Kidd, Paul's Wharf, London, "Improvements in obtaining artificial light, and in the apparatus employed therein."

3108. W. R. Lake, Southampton Buildings, Chancery Lane, "An improved artificial compound chiefly designed for use as a substitute for india-rubber or caoutchouc."—A communication from A. G. Day, Seymour, Conn., U.S A.—November 4, 1867.

3127. E. C. Prentice, Stowmarket, Suffolk, "Improvements in the treatment of gun-cotton and charges or cartridges made therefrom, as also in the processes employed in their manufacture."

3132. I. Baggs, High Holborn, Middlesex, "Improvements in preparing and oxidising certain substances capable of producing chlorine."—Petition recorded November 6, 1867.

3168. E. B. Wilson, Bolton, Lancashire, "Improvements in furnaces."—November 9, 1867.

3194. J. C. Bayley, and D. Campbell, John Street, Adelphi, "Improvements in fire-lighters and fire-revivers."

3198. W. H. Crispin, Marsh Gate Lane, Stratford, Essex, "Improvements in the manufacture of artificial fuel."—November 12, 1867.

INVENTION PROTECTED BY THE DEPOSIT OF COMPLETE SPECIFICATION.

3972. W. Brookes, Chancery Lane, "An improved method of treating hides in the process of tanning, and for apparatus employed therein.—A communication from M. Michaelis, J. Hallenstein, and A. Cleghorn, Footscray,,Victoria."—Petition recorded October 22, 1867.

NOTICES TO PROCEED.

2606. G. Pickin, Birmingham, "Improvements in the treatment and preparation of mineral oils and spirits for illuminating and other purposes."—Partly a communication from H. Chadburn, St. Louis, Missouri, U.S.A.—September 16. 1867.

2745. T. Prideaux, Sheffield, "Improvements in blast furnaces or cupolas."—September 28, 1867.

1713. H. Fletcher, Old Hall Street, Liverpool, "Improvements in the manufacture of artificial fuel."—Petition recorded June 11, 1867.

1748. G. McKenzie, Glasgow, N.B., "Improvements in the manufacture of illuminating gas."

1754. C. Erba, Milan, "Improvements on depilation and leather tanning."—June 15, 1867.

[English Edition, Vol. XVI., No. 417, pages 281, 282; No. 413, page 233; No. 414, page 245; No. 415, page 258; No. 416, page 269; No. 417, page 282; No. 413, pages 233, 234.]

1767. F. B. Miller, Sydney, New South Wales, "An improved method of toughening brittle gold bullion, of refining alloyed gold, and of separating therefrom any silver they may contain."—June 17, 1867.

1803. H. K. York, Cardiff, "Improvements in the manufacture of steel."—June 20, 1867.

1819. G. Dickie, Kilwinning, Ayr, N.B., "Improvements in the manufacture of illuminating gas."—June 21, 1867.

1846. T. Crow, West Ham, Essex, "Improvements in the manufacture of illuminating gas from gas tar, oil, or from gas tar."—June 25, 1867.

1747. J. Onions, Devon Place, Newport, Monmouthshire, "Improvements in the manufacture of iron and steel."—Petition recorded June 15, 1867.

1886. C. O. Heyl, Berlin, "An improved method of and apparatus for making sulphuret of carbon."

1887. C. O. Heyl, Berlin, "An improved method of and apparatus for extracting fatty matters from and for scouring wool and other fibrous substances and textile fabrics, by the use of sulphuret of carbon, and for regaining the sulphuret used in such operation."—June 28, 1867.

1897. C O. Heyl, Berlin, "An improved method of and apparatus for extracting oil or fatty matter from cotton or woollen waste, shoddy, mungo, rags, and analogous substances, by means of sulphuret of carbon."—June 29, 1867.

2011. W. E. Newton, Chancery Lane, "Improvements in the processes of and apparatus for the rectification and purification of alcohol."—A communication from J. G. Bequet, and H. Champonnois, Rue St. Sebastien, Paris.—July 9, 1867.

2801. J. Anderson, Londonderry, "Improvements in obtaining chlorine, sodium, potassium, phosphorus, and their compounds."—October 5, 1867.

1845. J. Webster, Birmingham, "A new metallic zinc paint."

1850. L. Brunetti, Rovigno (Italy), "An improved process of embalming and preserving animal substances from decay for anatomical purposes."—Petitions recorded June 25, 1867.

1900. A. M. Fell, West Calder, Mid Lothian, N.B., "Improvements in purifying or preservative compounds to be applied to the fleeces or skins of sheep and other animals."—June 29, 1867.

2034. J. H. Johnson, Lincoln's Inn Fields, Middlesex, "Improvements in the manufacture of refined sugar."—A communication from E. P. Eastwick, Baltimore, Md., U.S.A.—July 11, 1867.

2484. C. Geistharp, Low Walker, Newcastle-on-Tyne, "Improvements in the manufacture of malleable iron, cast iron, and steel, and in the construction of furnaces to be employed for such purposes."—September 2, 1867.

1924. G. A. F. Fowlke, St. James' Street, Westminster, "Improvements in compositions for preventing the fouling of the bottoms of ships, floating docks, and other similar structures, and in the mode or means of applying the said compositions."—Petition recorded July 2, 1867.

2611. C. Holste, Henrietta Street, Covent Garden, Middlesex, "Improvements in blast furnaces."—A communication from F. Lurmann, Oeside, near Osnabruck, Prussia.—September 17, 1867.

3332. J. Young, Aspull, Lancashire, "Improvements in the application of cannel coal 'slack' to the manufacture of gas and coke."—October 28, 1867.

2006. G. Gabillon, Rue Jaquelet, Paris. "A process to prepare and preserve paper and tissues with a solution of perchloride of iron, intended for stopping the bleeding of wounds."—Petition recorded July 9, 1867.

2046. J. Hargreaves, Appleton-within-Widnes, Lancashire, "Improvements in the manufacture of steel and soft iron from cast iron."—July 12, 1867.

2973. W. Brookes, Chancery Lane, "An improved method of treating hides in the process of tanning, and for apparatus employed therein."—A communication from M. M. J. Hallenstein and A. Cleghorn, Footscray, Victoria.—October 22, 1867.

NOTES AND QUERIES.

It has been represented to us that our column of Notes and Queries has occasionally been made the vehicle for the surreptitious disposal of trade secrets by subordinates in chemical works, unknown to their principals. This column has proved to be sufficiently useful to a large class of our readers for us to be reluctant to discontinue it for the sake of a few who abuse its privileges. Probably a more rigid supervision will enable us to obviate the difficulty. There will be no objection to a correspondent asking for information on trade subjects; but the answer must likewise be made public, and in such cases no name or address can be given, no private communications forwarded through us, and no offer of payment for information can be published.

Perfect Vacuum.—I have a recollection that some chemist described a method of getting a perfect vacuum some years ago. Can any of your readers refer me to the exact description?—Electron.

Granular Effervescing Citrate of Magnesia.—It is stated on good authority that there is no citrate of magnesia whatever in the above popular medicine. Is this the case? If so it ought to be widely known. Can any of your readers give me a simple and trustworthy test for its presence in an effervescing mixture?—A Surgeon.

Manufacture of Epsom Salts.—Some weeks ago I noticed a correspondent asking, through Notes and Queries, at what towns in the north Epsom salt is made on the plan I described in your journal some time ago. I should have answered this before but have been prevented through illness. I now beg to state, if it is not too late, that Manchester, Bolton, Wigan, St. Helens, etc., are some of the places working on the plan described.—J. H. Swindells.

Spontaneous Ignition of Charcoal.—Has this phenomenon ever been observed? A case has just come under my notice in which a considerable quantity of finely powdered wood charcoal has been found in a smouldering state. I have carefully examined into the circumstances, and can assign no reason whatever for the ignition except that it occurred spontaneously.—W. Holmes.

Action of Salts on Glass.—It does not seem to be generally known that several ammoniacal salts act on glass at a high temperature. Thus fusing sulphate of ammonia, and also a mixture of nitrate and chloride of ammonium, in a state of fusion attack glass. An ignorance of these facts has just led me astray in considering fluorine to be present where it was not.—Medicus.

Spontaneous Ignition of Charcoal.—Mr. Holmes will find this subject fully discussed in Dr. Taylor's "Principles of Medical Jurisprudence," published two or three years ago.—F. R. S.

Spontaneous Ignition of Charcoal.—In reply to "W. Holmes," an occurrence of this sort is certainly rare, but it is not unrecorded before. In the *Philosophical Magazine* for 1833 (vol. 3, p. 1) a case is mentioned. In the same volume, p. 91, Mr. Davies attempts to assign the cause of the spontaneous ignition of charcoal. He suggests that during its manufacture a small quantity of potassium is formed, and adduces several reasons why this should be the case.—Scrutator.

Storm Glass.—Will you allow me to ask Mr. Tomlinson for an explanation of the following, which I have often remarked in my storm-glass? If it is left hanging up in my window undisturbed for a long time there gradually collects on the surface an oily-looking layer of a lightish brown colour, extending downwards faintly until it is lost in the pure white of the saline contents of the tube. If the storm glass be inverted, and the contents mixed up, the oily-looking liquid and the colour quite disappear, and some weeks elapse before they again make their appearance on the surface.—A Constant Reader.

Bleaching Palm Oil.—The want of success of your correspondent, Geo. Johnson, in bleaching palm oil appears to be at least partly due to his not using a sufficient quantity of hydrochloric acid. In order to complete the decomposition of one part of "Bichrome," 1·93 parts of HCl are requisite, the decomposition taking place according to the reaction,

$$K_2O_2CrO^3+14HCl=7H_2O+2KCl+2CrCl_3+Cl_6;$$

but as the strongest liquid muriatic acid only contains about 40 per cent. of HCl, and as the commercial acid is frequently much weaker, at least 4·8 or five parts of acid should be used for one of "bichrome," in order to obtain the whole available bleaching powder. In practice, a mixture of sulphuric and hydrochloric acids is frequently employed; about the following proportions have given good results: 1 lb. bichrome, 4 to 5 lbs. yellow muriatic acid, ¾ to 1 lb. sulphuric acid. Instead of agitating by paddles, air may be blown through the mass with advantage, as a better intermixture is thereby obtained, and labour saved; probably also the air assists in oxidising the colouring matter.

Sulphuric acid alone with bichrome has the advantage of not causing deleterious chlorine fumes, to the great comfort of the workman; it is, however, said to be less efficacious for some kinds of oil than the mixture of acids, and to require a longer time. The cheapest method appears to be exposure of the oil in a layer of about 1½ to 2 inches on the surface of water heated to 100° C. by steam to the combined influence of air and sunlight; it requires, however, a much longer time than the bichrome plan. By passing superheated steam, alone or mixed with air, at a temperature of 110° to 112° C. through the oil, the colour is almost entirely destroyed. Your correspondent will find further information in "Richardson and Watts's Chemical Technology," vol. i. part 2, p. 410; also in "Ure's Dictionary of Arts," etc., iii., 390.

In performing the bichrome bleaching process, the green liquid left after the first batch of oil has been bleached may be mixed with fresh bichrome, and a less quantity of acid than that first used added, and the whole used for a second batch: any bichrome left in the green liquor is thus utilised. By addition of excess of lime-paste to the spent chromic chloride, a precipitate of lime and chromic hydrate is obtained; this, on drying and roasting with access of air, furnishes a chromate of lime, which may be used instead of fresh bichrome, thus diminishing, with careful workmen, the cost of the process.—C. R. A. Wright, B. Sc.

Palm Oil.—Where can I find instructions for isolating the colouring matter of palm-oil; or how can it be effected?—S.

Perfect Vacuum.—"Electron" will find a paper by Dr. Andrews on a method of obtaining a perfect vacuum, in the Philosophical Magazine, series 4, vol. iii., p. 104. I believe Mr. Cetti now makes vacuum tubes so perfect that they will not allow an induction current to pass. I believe the process for making these has also been published, but I cannot say where.—W. Thompson.

Chloride of Barium.—Your Paris Correspondent, in a recent number of your valuable journal, states that Messrs. Burgoyne and Co. manufacture chloride of barium in a pure state, and that they are selling the same at £30 per ton. To my thinking £30 is a very high price to ask for this article; I wish therefore to state that I have made large quantities of chloride of barium, nearly absolutely pure, disposing of the same (of course at a profit) at £9 per ton.—J. H. Swindells.

Clarifying Caustic Soda.—Would any of your correspondents be good enough to instruct me what method I must adopt to clarify caustic soda solution of the dark-brown substance, and other impurities, which are sometimes associated with it. When I make the solution and causticise with lime, the carbonate of lime falls to the bottom, leaving a solution, deeply coloured red or brown (instead of colourless and clear), which is exceedingly inconvenient.—George Johnson.

Granular Effervescing Citrate of Magnesia.—The medicine known as "Effervescing Citrate of Magnesia" contains no citrate of magnesia of the chemist at all. It is nothing more than citro-tartrate of soda of the British Pharmacopœia with an addition of sugar and four or five per cent. of sulphate of magnesia. The magnesia can be detected by adding to a solution of the granules, ammonia, and tribasic phosphate of soda, when a precipitate of ammonia-phosphate of magnesia is almost immediately formed. The presence of the sulphuric acid in combination with this base can be detected by a solution of chloride of barium acidulated with hydrochloric acid, which gives an insoluble precipitate of sulphate of baryta.—C. Umney.

Granular Effervescing Citrate of Magnesia.—"A Surgeon" is quite right in supposing that there is no magnesia at all in the above preparation, indeed in many instances the two particular substances which are conspicuous by their absence are *magnesia*, and *citric acid* (!) while some varieties I have examined I found to contain bisulphate of sodium in place of the vegetable acid. One of these last samples was "warranted to contain no acid besides citric," an assertion which was of course in one sense true. I have always believed that the absence of magnesia from the so-called "citrate" was "widely known," if not, the sooner the facts are published the better.—Wentworth L. Scott.

Bleaching Palm Oil.—Referring to No. 414 (*Am. Reprint, Jan.* 1868, page 59) of your valuable journal, I beg to acknowledge with grateful thanks to Mr. C. R. A. Wright his full and clear description of the method of bleaching palm oil by chromic acid. I have been greatly more successful since applying the proportion of the bleaching agents which he prescribes. I have also discovered the desirability of keeping the materials well up together while the bleaching is going on; and as he suggests a method of agitating superior to the employment of paddles, viz., "to blow air through the mass," I wish Mr. Wright to be good enough to inform me the description of apparatus necessary for this purpose, and also if the temperature of the air must be elevated to correspond with that of the oil.—George Johnson.

Spontaneous Ignition of Charcoal.—I cannot agree with "Scrutator" that this phenomenon is certainly new, as I have met with several instances of it during the preparation of the "Granular Charcoal," recently noticed in your columns; lamp-black, after having been carefully washed and afterwards dried again, is very liable to ignite spontaneously, and on this very account is less used in pyrotechny than formerly. I have lately inspected some enormous stills used for the generation of sulphurous acid; the charcoal employed, if taken out for any purpose, is almost certain to undergo spontaneous combustion, sooner or later, unless quickly and carefully excluded from the air.—Wentworth L. Scott.

Spontaneous Ignition, or Combustion.—I beg to remind Mr. Holmes that a great many porous substances, *e. g.* finely divided metals, so readily absorb oxygen from air as to become visibly incandescent in daylight: not only powdered wood charcoal, but that article in bulk, equally so animal charcoal, and pitcoal, are not unfrequently known to have got ignited, the cause being due to the rapid absorption of oxygen from the air aided by the bad conducting power for heat these substances are endowed with. To the same category belong the phenomena of the spontaneous ignition of hay, straw, flax, and saw-dust in large heaps; also, and this is a frequent cause of fires, the extreme danger of cotton or linen rags, tow, and even some kinds of paper saturated with oil and grease. A few years ago a remarkable instance of spontaneous ignition took place in ships anchored in the roads of Batavia. On board these vessels among the cargo was a quantity of Turkey-red dyed cotton, which, as is well known, is obtained by the use of oil as mordant. It was clearly made out by careful experiments made by a scientific committee appointed to inquire into the cause of these fires, which were at first taken to be due to malice, that actually the spontaneous ignition of the Turkey-red dyed cotton was the true cause of the fire. The spontaneous ignition of charcoal has often been fatal to gunpowder works, while equally, the spontaneous ignition of animal charcoal has caused fires in sugar refineries.—Dr. A. A.

ANSWERS TO CORRESPONDENTS.

NOTICE.—The Printing and Publishing Offices of the Chemical News *are Removed from Wine Office Court, Fleet Street, to*

BOY COURT, LUDGATE HILL, E.C.

where all Communications are requested to be addressed.

H. Hallet.—The largest rough diamond on record was the "Great Mogul;" originally it weighed more than 6 ounces, but was reduced by cutting to two ounces. It is now lost.

Lux.—Manufactured on the large scale, oxygen would not be dear. It could be made for much less than 20s. per 1000 feet. The great objection to its introduction would be the necessity of having two sets of mains in all the thoroughfares.

Querist.—*Must* and *musk* are very different things: the former is the technical name given to the unfermented juice of the grape, the latter is an animal perfume. From the context it is evident that your correspondent has made a clerical error and refers to the former, not to musk.

A. P.—Write to Messrs. Wells and Hall.

A Constant Reader.—You must be very careful how you try the experiment. The apparatus will almost certainly explode.

F. O. X.—You must use slaked lime. Quick lime will not absorb carbonic acid. This is a sufficient reason for your failure.

Bellum.—Bell metal is an alloy of copper and tin; the proportions vary, but they are very near 76 of copper, and 24 of tin.

J. Barnes.—To answer your question it would be necessary to write a complete treatise on the science. We must refer you to the article "Crystallography" in Watts's Dictionary.

Olympic Company.—The more complex the composition of a glass, the more stable it is supposed to be. A simple silicate is readily devitrified.

W. Schofield.—Received.

G. Johnson.—A letter is waiting at our office. Please forward address where it is to be sent.

F. R. S.—The lecture appeared in full in the Chemical News, but was not published separately. We do not know the other works named.

T. Tyrer.—We regret that we cannot give the information required. Hunt's Mineral Statistics would probably give what you want.

N. Thompson.—No other place except the one referred to.

W. B. G.—Wöhler's researches on the nitrate of boron may be found in the *Chemical Gazette*, vol. vii., p. 234. We shall be glad to hear particulars of your experiments.

Student.—There is no cheaper process for producing chlorine on a large scale than the reaction of sulphuric acid and salt, or of hydrochloric acid, on binoxide of manganese. This is the process universally adopted in chemical works.

T. W.—Asks how to precipitate double chloride of platinum and aluminium from a solution of the two metals in *aqua regia*. No such compound is known. It might possibly be obtained by careful evaporation of the acid solution referred to, but it could not be *precipitated*.

P. Holland.—Received with thanks; we shall be glad to hear further from this correspondent.

J. Johnstone.—The article has been in type for several weeks. It shall be inserted as soon as we have room for it.

James O.—Precipitate the silver in a plate of copper.

Olinthus.—Mr. Campbell has detected arsenic in the beds of some rivers.

Chromo-technist.—An account of Mr. Sorby's researches on micro-spectroscopic analysis appeared in our columns some time ago.

A Subscriber.—We have asked the publisher for an explanation respecting the missing sheets in the Dictionary.

Communications have been received from J. H. Swindells; Professor A. H. Church, M.A.; H. Wallis; D. Cameron; Walter Hall (with enclosure); H. Seward; W. Hill (with enclosure); J. H. Dickenson; W. Hoare (with enclosure); J. R. Smith; M. E. Taylor; J. Bird (with enclosure); J. Pain; S. Barnes; M. Willis; R. Newman; W. Delcomyn; G. King; Nicholas Pugh; C. R. A. Wright, B. Sc. (with enclosure); Dr. Odling, F.R.S.; Rev. Edwin Smith, M.A.; W. M. Bywater; E. Bennett; Jesse Fisher; J. Thompson; H. Baden Pritchard; C. F. Burnard; J. Cliff; W. Spalding (with enclosure); F. O. Ward (with enclosure); A. E. Sansom; G. Griffith; D. Forbes, F.R.S. (with enclosure); J. H. Howard; T. Stevenson (with enclosure); R. M. Hands; W. H. Harrison; C. Manby; J. Forrest; Dr. A. E. Nordenskiold (with enclosure); W. Schofield (with enclosure); F. Zeuner; J. Collins (with enclosure); W. Gibbs; S. Longman (with enclosure); Dr. E. Rohrig; W. Bremmer; Dr. R. Angus Smith, F.R.S.; W. Tobister; C. R. A. Wright; D. Day, F.R.S. (with enclosure); W. Hall; Professor Heaton; Dr. Letheby; T. Bourne; T. Miller; Professor Weltzein; Yates; M. Murphy; T. Fox (with enclosure); Dr. Gilbert, F.R.S. (with enclosure); T. Bournes; T. Miller; W. Schofield; J. Smith (with enclosure); W. M. Bywater (with enclosure); Rev. C. W. Kett; F. Armstrong; W. Lovatt; J. Walkden; H. Russell (with enclosure); Professor Gustavus Hinrichs, Iowa (with enclosure); Dr. W. Allen Miller, F.R.S.; S. Wright; J. A. Brand; J. Samuelson; S. Chambers (with enclosure); W. Gillett; J. Brown (with enclosure); F. Bromley; W. Wingrove; Dr. R. Angus Smith, F.R.S.; W. Isbister; Dr. A. E. Sansom; F. O. Ward (with enclosure); J. Spiller; I. Baggs; Professor Gamgee; G. W. Simpson; H. Kinnaird Yorke; M. Moncreiff Pattison; C. E. C. Tichborne; S. E. Phillips (with enclosure); H. N. Draper; J. Cliff; Kingsbury & Co. (with enclosure); J. Heywood; W. Hall; Liebig's Extract of Meat Co.; Dr. Tilbury Fox; E. W. Bartlett; W. H. Harris; Dr. F. Wehelms; W. Hoe; C. Tomlinson.

Books Received.—"Practice with Science, a Series of Agricultural Papers." Vol. i. London, 1867. Longmans & Co. "On the Qualifications and Duties of an Officer of Health," by Dr. Letheby. London, 1867. "Introductory Lecture to the opening of the Eighty-third Medical Session at the London Hospital," by Dr. Letheby. London, 1867. "The Journal of Materia Medica," by Dr. J. A. Bales and H. A. Tilden. New Lebanon, New York. "Scientific American." "American Artisan." "Journal of Gas Lighting." "The present State of Manufacture of Iron in Great Britain," by J. Lowthian Bell. "Catalogue of American and Foreign Scientific Books for Sale," by D. Van Nostrand. New York. "Mining and Petroleum Standard and American Gas-Light Journal." New York. "Hardwicke's Science Gossip," November. "A Peep at the Pyrenees," by a Pedestrian. London: Whittaker & Co. "The Microscope in Geology," by D. Forbes, F.R.S. London: Hardwicke. "Undersokning af Selenmineralierna fran Skrikerum," af A. E. Nordenskiold. "American Artisan." "American Journal of Mining." "Pharmaceutical Journal" for November. "Scientific Mining Press." "A Dictionary of Chemistry," Part 42, by H. Watts, B.A., F.R.S. London: Longmans. "The Industrial Partnerships' Record," for November. "The Scientific American." "The World of Science." "Le Moniteur Scientifique." Paris. "Mining and Scientific Press." "Scientific American." "American Artisan." "American Journal of Mining." "The Journal of Materia Medica." "The Journal of Gas Lighting." "Moniteur Scientifique." "The Colonial Mail." "The Produce Markets' Review." "Naquet's Modern Chemistry," translated by W. Cortis and revised by T. Stevenson, M.D. London: Henry Renshaw. "A Programme of Atomechanics, or Chemistry, as a Mechanics of the Panatoms," by Gustavus Hinrichs. "The Microscope in Geology," by David Forbes, F.R.S. "The Bible and Science," by William Allen Miller, M.D., LL.D., Treas. and V.P.R.S. "The Darwinian Theory Examined," by a Graduate of the University of Cambridge. London: James Nisbet and Co., Berners Street.

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ON MONO-CARBON COMPOUNDS.*

BY DR. ODLING, F.R.S.

The principal compounds of carbon with hydrogen, oxygen, and nitrogen, which contain only one atom of carbon in their respective molecules, are the following: The existence of methylen H_2C, is very doubtful.

H_4C	Marsh-gas or methene.
H_4CO	Wood-spirit or methyl alcohol.
H_2CO	Formic Aldehyd.
H_2CO_2	Formic Acid. CO Carbonous oxide.
H_2CO_3	Carbonic Acid. CO_2 Carbanhydride.
HCN	Prussic or hydrocyanic Acid.
$HCNO$	Cyanic acid.

Formic acid furnishes only one class of salts, which are consequently monobasic, as exemplified by sodium formiate $NaHCO_2$, for instance. Carbonic acid furnishes two classes of salts, acid and entire, or mono- and dibasic, as exemplified by the two sodium carbonates, $NaHCO_3$ and Na_2CO_3, for instance. Carbonous oxide and carbanhydride gases may be regarded as dehydrated forms of the formic and carbonic acids respectively. Carbonic acid, indeed, H_2CO_3, is not known as an isolated compound, but only in the state of aqueous or dissolved anhydride.

In addition to the above formulated mono-carbon compounds, various derivatives of them exist, in which the constituent oxygen of certain of them is represented by sulphur, and the constituent hydrogen by chlorine or a congener, as in chloroform Cl_3HC, methyl mercaptan H_4CS, phosgene Cl_2CO, sulphocyanic acid $HCNS$, etc.

All the above normal compounds are susceptible of mutual metamorphosis, especially through the intervention of their chloro- and sulpho- derivatives, but the two simple oxides are alone directly procurable from elementary carbon. The principal metamorphic relations of the several compounds are as follows:

α. **Methene**, H_4C, is procurable from methyl alcohol, H_4CO, indirectly, through the intervention of various methyl-compounds, such as methyl-iodide, mercury methide, etc., usually formed from the alcohol. The hydrocarbon and alcohol are more especially associated by means of methyl-chloride, which is both producible from and transformable into either one of them. Prepared from methyl-alcohol, for instance, it furnishes marsh-gas by the action of nascent hydrogen, thus:

$$ClH_3C+H_2=H_4C+HCl$$

Methene is further procurable from formic acid H_2CO_2, by its transmission over ignited baryta, which serves to absolve the resultant carbanhydride:

$$4H_2CO_2=2H_2O+3CO_2+H_4C$$

Also, from the sulpho-derivative of carbanhydride, namely disulphide of carbon CS_2, by its reaction, conjointly with sulphuretted hydrogen, upon ignited metallic copper:

$$CS_2+2H_2S+Cu_8=H_4C+4Cu_2S$$

β. **Methyl-alcohol**, H_4CO, is procurable from marsh gas H_4C, indirectly, by decomposing with potash its derivative methyl-chloride, a compound obtainable from marsh-gas, as already mentioned, by its gradual reaction with chlorine:

$$ClH_3C+KHO=H_4CO+KCl$$

γ. **Formic aldehyde**, H_2CO, a compound of very recent discovery, is obtainable by effecting the aerial oxidation of methyl alcohol vapour, by means of a coil of ignited platinum wire:

$$H_4CO+O=H_2CO+H_2O$$

δ. **Formic acid**, H_2CO, is procurable from marsh-gas H_4C, by decomposition of its derivative chloroform with potash:

$$Cl_3HC+4KHO=3KCl+KHCO_2+2H_2O$$

Also from methyl-alcohol H_4CO, by its reaction with heated soda-lime; and, with intermediate production of formic-aldehyd, by various oxidations of the alcohol, including its exposure to air under the influence of platinum black:

$$H_4CO+HNaO=NaHCO_2+2H_2$$
$$H_4CO+O_2=H_2CO_2+H_2O$$

Also from carbonous oxide CO, by its combination with heated caustic potash, to produce potassium formiate:

$$CO+KHO=KHCO_2$$

Also from carbonic acid H_2CO_3, by its reduction with metallic sodium, to produce sodium formiate:

$$H_2CO_3+Na_2=NaHCO_2+NaHO$$

Carbonous oxide CO, is producible by the dehydration of formic acid H_2CO_2, with oil of vitriol, for instance; and by the deoxidation of carbanhydride CO_2, with ignited zinc, iron, carbon, etc.

ε. **Carbonic acid**, H_2CO_3, is procurable from marsh-gas H_4C, methyl-alcohol H_4CO, and formic acid H_2CO_2, by various oxidations, as with moist chlorine for instance:

$$H_4CO+3Cl_2+2H_2O=H_2CO_3+6HCl$$

Also from carbanhydride CO_2, by its solution in water, or its fixation by caustic alkali, to form a mono- or dibasic carbonate:

$$CO_2+KHO=KHCO_3$$
$$CO_2+2KHO=K_2CO_3+H_2O$$

Also from carbonous oxide CO, indirectly, through the intervention of phosgene, by decomposition of this compound with water or alkali:

$$Cl_2CO+2K_2O=K_2CO_3+2KCl$$

Carbanhydride CO_2; is producible by the mere desiccation of carbonic acid H_2CO_3; and by the direct oxidation of carbonous oxide CO, as with air and spongy platinum for instance, or by different metallic oxides at a red-heat.

The formic and carbonic acids, with their anhydrates, have corresponding relations of metamorphosis to hydrocyanic acid and cyanic acid respectively.

Hydrocyanic acid HCN, when heated with oil of vitriol, absorbs one atom of water, with production of carbonous oxide and ammonia; and, when heated with potash, absorbs two atoms of water, with production of formic acid and ammonia:

$$HCN+H_2O=CO+H_3N$$
$$HCN+2H_2O=H_2CO_2+H_3N$$

Under similar treatment, **cyanic acid** $HCNO$, absorbs one and two atoms of water, with production of ammonia, and of carbanhydride and carbonic acid respectively:

$$HCNO+H_2O=CO_2+H_3N$$
$$HCNO+2H_2O=H_2CO_3+H_3N$$

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* Dr. Odling has kindly permitted us to publish occasional Chapters from Part II. of his "Manual of Chemistry," which we are glad to be able to announce is now in the press.—Ed C. N.

The ammonia, formed in the oil of vitriol reactions, appears of course as sulphate of ammonia, and the formic and carbonic acids, produced in the potash reactions, appear as potassium salts.

Independently of their origin from one another, and from cyanic compounds, as above described, the leading members of the methyl-formic families are obtainable from the following principal sources.

Methene occurs naturally as fire-damp, marsh gas, etc., and forms an important constituent of ordinary coal-gas. It is usually made from acetic acid, the vapour of which undergoes decomposition, at a red heat, into equal volumes of methene and carbanhydride :

$$H_4C_2O_2 = H_4C + CO_2$$

Methyl alcohol, or hydrate, which forms the chief constituent of commercial pyroligneous spirit or wood-naphtha, is also obtainable from the methyl salicate occurring in gaultheria oil, from the methyl acetate found together with the hydrate in crude wood-naphtha, and from the methyl-chloride or iodide produced by the action of hydrochloric or hydro-iodic acid upon narcotine.

Formic acid exists naturally in the juice of red ants, etc. It can be procured by heating starch and similar organic substances with different oxidising mixtures; but it is usually made by the decomposition of oxalic acid, a compound also resorted to as a source of carbonous oxide :

$$H_2C_2O_4 = H_2CO_2 + CO_2 = H_2O + CO + CO_2$$

Carbonic acid or anhydride is readily procurable from its different salts, whether found native, or produced artificially by the combustion of organic matter, coal, etc., in actual or virtual presence of bases. It is also a very frequent product of the oxidation and decomposition of organic compounds. Many organic acids, for instance, as already exemplified by acetic acid, undergo, when heated either alone or with caustic alkali, a decomposition into carbanhydride and some other compound. In this way salicic acid, for instance, yields carbanhydride and phenol :

$$H_6C_7O_3 = H_6C_6O + CO_2$$

The primary compounds of carbon may be conveniently considered as forming three distinct groups, namely, the methylic, typified by marsh-gas H_4C; the formic, typified by formic aldehyd H_2CO'', or marsh-gas, in which two atoms of hydrogen are replaced by one atom of diad oxygen; and the cyanic typified by prussic acid HCN''', or marsh-gas in which three atoms of hydrogen are replaced by one atom of triad nitrogen. The origin and relationship of the various cyanic compounds are discussed after the description of the individual methylic and formic compounds.

METHYLIC COMPOUNDS.

The relationship of methyl-alcohol to methyl-chloride and methylamine respectively, corresponds exactly with that of water to hydrochloric acid and ammonia; and the three bodies may be regarded as derivatives of hydrochloric acid, water, and ammonia, by substitution of an atom of methyl for an atom of hydrogen :

$$\begin{array}{llllll} H\,Cl & (H_3\ \ C)Cl & \text{or} & (Cl) & H_3C & (Cl\ \ )H \\ H_2O & H\,(H_3\ \ C)O & \text{“} & (HO) & H_3C & (HO\ \ )H \\ H_3N & H_2(H_3C)N & \text{“} & (H_2N) & H_3C & (H_2N\ \ )H \end{array}$$

By a further substitution of methyl for the hydrogen of water, there is produced methyl-ether, as above referred to; while, by its further substitution in ammonia, there are produced di- and tri-methylamine.

$$\begin{array}{lllll} H\,Cl & (H_3\ \ C)Cl & & & \\ H_2O & H\,(H_3\,C)\,O & (H_3\ \ \ C)_2O & \text{“} & \\ H_3N & H_2(H_3C)\,N & (H(H_3C)_2 & N & (H_3C)_3N \end{array}$$

By a similar substitution of methyl for the hydrogen of methyl itself, as existing in marsh-gas and methyl alcohol, for instance, there are produced the homologues of marsh-gas and methyl-alcohol, or ethene and common alcohol respectively :—

$$\begin{array}{l|lll} H_4C & H_3(H_3C)C = & H_6C_2 & \text{Ethene} \\ H_4CO & H_3(H_3C)CO = & H_6C_2O & \text{Alcohol} \end{array}$$

And just as marsh-gas and methyl-alcohol are regarded as the hydride and hydrate of methyl, so may ethene and ethyl-alcohol be regarded as the hydride and hydrate of ethyl, and so do they form an ether or oxide corresponding to methyl-ether, and three successive amides or nitrides corresponding to the three methylamines, etc.

$$\begin{array}{l|lll} H\,(H_3C) & H\,(H_5\,C_2) & & \\ H\,(H_3\,C)\,O & H\,(H_5\,C_2)O & (H_5C_2)_2O & \\ H_2\,(H_3C)\,N & H_2(H_5C_2)N & H(H_5C_2)_2N & (H_5C_2)_3N \end{array}$$

Moreover, various mixed or intermediate compounds are also known, such for instance as methyl-ether $(H_3C)(H_5C_2)O$, methyl-diethylamine $(H_3C)\,(H_5C_2)_2N$, etc. Thus the radicals methyl and ethyl, which, for brevity's sake, are often represented by the symbols "ME," and "ET," have the similar property of replacing hydrogen in a great variety of bodies, to form methyl and ethyl derivatives, corresponding closely to one another in constitution, formation, decomposition, and general behaviour; so that to almost every methyl compound there exists a corresponding ethyl compound; and, indeed, from various causes, the ethyl series of compounds has been altogether better studied, and is more complete, than even the methyl series itself. In addition, however, to analogues of the different methyl compounds, the ethyl series, by reason of the greater complexity of its primary hydrocarbon, includes various compounds which have not, and cannot have, true methyl analogues.

By a further substitution of methyl in methyl-methyl or ethene, $H_3C.\,H_3C$ or H_6C_2, there is produced a new hydrocarbon propene $H_3C.H_2C.H_3C$ or H_8C_3, which may also be represented as propyl-hydride $H\,(H_7C_3)$, and of which propyl alcohol, H_8C_3O or $H(H_7C_3)\,O$ is the corresponding hydrate; and so on indefinitely. The earlier terms of this homologous series of hydrocarbons and alcohols, and of the acids resulting from the oxidation of the alcohols, are given in the following table :

Hydrocarbon.		Alcohol.		Acid.	
H_2	Hydrogen	$H_2\ \ O$	Water		
$H_4\,C$	Marsh-gas	$H_4\,C\,O$	Methyl	$H_2\,C\,O_2$	Formic
$H_6\,C_2$	Ethene	$H_6\,C_2O$	Ethyl	$H_4\,C_2O_2$	Acetic
$H_8\,C_3$	Propene	$H_8\,C_3O$	Propyl	$H_6\,C_3O_2$	Propionic
$H_{10}C_4$	Butene	$H_{10}C_4O$	Butyl	$H_8\,C_4O_2$	Butyric
$H_{12}C_5$	Eupione	$H_{12}C_5O$	Amyl	$H_{10}C_5O_2$	Valeric
$H_{14}C_6$	Caprene	$H_{14}C_6O$	Capryl	$H_{12}C_6O_2$	Caproic

Methene has some similarity, and at the same time considerable dissimilarity, to another hydrocarbon known as phenene, or hydride of phenyl H_6C_6; and just as ethene is produced by the replacement of hydrogen by methyl in methene, so is benzoene or toluol produced by the same replacement of methyl for hydrogen in phenene. The resulting methyl-methene or ethene, and methyl-phenene or benzoene, correspond closely with one another, and give rise to strictly analogous compounds and series, thus :—

Hydrocarbon.		Alcohol.		Acid.	
H_6C_2	Ethene	H_6C_2O	Ethyl	$H_4C_2O_2$	Acetic
H_8C_7	Benzoene	H_8C_7O	Benzyl	$H_6C_7O_2$	Benzoic
$H_{10}C_8$	Xylene	$H_{10}C_8O$	Xylyl	$H_8C_8O_2$	Toluic
	etc.		etc.		etc.

METHYL-HALIDES, ETC.

As previously mentioned, methyl-chloride ClH_3C, is procurable from marsh-gas, H_4C, by direct substitution of chlorine for hydrogen, and from methyl alcohol, H_4CO or $(HO)H_3C$, by substitution of chlorine for hydroxyl. It also occurs as a product of the decomposition by heat of the hydrochloride of kakodylic acid $H(H_3C)_2AsO_2.HCl$, and of the reaction of hydrochloric acid with narcotine. It is decomposable by potash, with reproduction of methyl-alcohol (Berthelot):

$$ClH_3+CKHO—KCl+H_4CO$$

By the further action of chlorine upon methyl-chloride, there are produced, in succession, the higher derivatives Cl_2H_2C, Cl_3HC, and Cl_4C; but the decomposition of these compounds by caustic potash leads to the production, not of methylic, but of formic compounds, thus:

$$Cl_2H_2C+\mathit{K}_2\mathit{O}=2KCl+H_2CO$$
$$Cl_3HC+2\mathit{K}_2\mathit{O}=3KCl+KHCO_2$$
$$Cl_4C+3\mathit{K}_2\mathit{O}=4KCl+K_2CO_3$$

The first of these reactions has not been established, owing propably to its incomplete examination. By treatment with nascent hydrogen, perchloride of carbon Cl_4C, is successively reconverted into chloroform Cl_3HC, this into methylen-chloride Cl_2H_2C, this into methyl-chloride ClH_3C, and this last, as already stated, into marsh-gas H_4C. Some halogen derivatives of marsh-gas appear, but are not altogether proved, to exist in two or more isomeric forms. The existence of two chlorides of methyl, and of three chlorides of methylene, is easily conceivable on the assumption that one pair of hydrogen atoms in marsh-gas represents the oxygen of carbonous oxide, while the other pair represents the more loosely combined excess of oxygen in carbonhydride, as indicated by the following formulæ:

$$\mathit{H}_2H_2C\begin{cases} Cl\mathit{H}H_2C;\ \mathit{H}_2HClC; \\ Cl_2H_2C;\ Cl\mathit{H}HClC;\ \text{and } \mathit{H}_2Cl_2C. \end{cases}$$

CONTRIBUTIONS TO THE HISTORY OF METHYLIC ALDEHYDE.*

BY A. W. HOFMANN, LL.D., F.R.S.

"The aldehyde of the methyl-series is not known;" all the chemical manuals say so, and for the last twenty years my students have been duly informed thereof. It will scarcely appear strange that more efforts to become acquainted with that body should not have been made, since the masterly picture which Liebig has delineated of the aldehyde *par excellence* embraced as it were the history of the whole class, and of course also of the aldehyde in question. Nevertheless methylic aldehyde deserves our consideration for more than one reason. As one of the simplest terms of the monocarbon series, occupying a position intermediate between marsh-gas and carbonic acid, as a link of transition connecting methylic alcohol and formic acid, as either aldehyde or acetone, according to the point of view from which we look upon it, the compound CH_2O illustrates a greater variety of relations than any one of the higher aldehydes. But in addition to the interest with which the methyl-compound has thus always been invested, this substance possesses special claims upon our attention at the present moment. Our actual method of treating organic chemistry for the purposes of instruction almost involves the necessity of starting from the methyl-series. The simplest of aldehydes thus acquires quite an especial importance, and all those who, like the author of this note, are engaged in teaching, cannot fail to have sadly missed a compound which is the carrier of such varied and interesting considerations.

The desire which I have frequently felt in my lectures of developing the idea of the genus aldehyde, when speaking of the methyl-compounds, has more than once induced me to attempt the preparation of methyl-aldehyde, but it was only at the conclusion of my last summer course that I succeeded, to a certain extent at all events, in attaining the object of my wishes.

A substance possessing the composition and the properties of methylic aldehyde is formed with surprising facility if a current of atmospheric air, charged with the vapor of methylic alcohol, be directed upon an incandescent platinum spiral.

The bottom of a strong three-necked bottle, of two litres' capacity, is covered to the height of about five centimetres with moderately warm methylic alcohol. The first neck is provided with a tube descending to the very surface of the liquid; into the second is fixed a loosely-fitting cork, which carries the platinum spiral; the third one, lastly, communicates with the upper end of a condenser, the lower end of which is fastened into a two-necked receiver. This receiver is in its turn connected with a series of wash-bottles, and the last of these communicates with a water-jet aspirator, by which a current of air can be sucked through the whole system.

The apparatus being disposed in this manner, the platinum spiral is heated to redness and introduced into the three-necked bottle. After a few minutes the flameless combustion of the methyl-alcohol begins to manifest itself by the evolution of a vapour powerfully affecting nose and eyes. Gradually the temperature of the apparatus rises, and soon droplets of a colourless liquid are condensed in the receiver. The formation of methyl aldehyde is now fairly proceeding, and if the current of air be appropriately adjusted, the platinum spiral remains incandescent for hours, and even for days. There is no difficulty in collecting from 50 to 100 grammes of a liquid rich in methyl-aldehyde.

Instead of establishing the current of air by a water-jet aspirator, a pair of bellows may be conveniently employed. I have often used with advantage the bellows of an ordinary glass-blowing table. This mode of proceeding is more particularly adapted to the requirements of the lecturer, who is thus enabled, by simply accelerating the movement of the foot, to enliven the combustion, so as to keep the whole spiral in a state of incandescence. By thus proceeding it happens, however, occasionally, that the gaseous mixture in the three-necked bottle is fired; but these explosions are perfectly harmless, the whole effect being the forcible ejection of the loosely-fitting cork which carries the platinum spiral.

The liquid which is being collected in the receiver has all the properties which theory assigns to the aldehyde of the methyl-series, or, more properly speaking, to its methyl-alcoholic solution. When re

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dered gently alkaline by a few drops of ammonia, and mixed with nitrate of silver, it yields, on gently warming, a silver mirror of unreproachable perfection, which is indeed more readily and more certainly produced than with the aldehyde of the ethyl-series. The reduction in this case is the result of two consecutive reactions; in the first stage the aldehyde yields formic acid, which in the second stage is converted into water and carbonic acid.

On heating the methyl-alcoholic solution of the aldehyde with a few drops of a fixed alkali, the liquid becomes turbid on ebullition, acquires a yellowish colouration, and soon deposits droplets of a brownish oil possessing in the highest degree the peculiar odour of ethyl-aldehyde-resin.

After the observation which I have mentioned, it was scarcely doubtful that the product of the slow combustion of methylic alcohol contains the aldehyde of this alcohol in considerable proportion. Nevertheless it appeared necessary to fix the nature of this compound by some numbers. The commencement of the vacations being at hand, there was but little hope of preparing the liquid in sufficient quantity for the purpose of obtaining the aldehyde, which will probably be found to be either gaseous at the common temperature, or extremely volatile, in a state of purity for analysis. Under these circumstances I have been compelled to limit myself to the preparation of an easily accessible derivative of methyl-aldehyde possessing a characteristic composition, and the analysis of which would not be less conclusive than that of the aldehyde itself. The slight solubility and the powerfully crystalline tendencies of the sulphaldehyde of the ethyl-series could not fail to indicate the direction in which I had a right to hope that the object which I was aiming at might be accomplished.

If a current of sulphuretted hydrogen be passed through the methyl-alcoholic solution of methyl-aldehyde, the liquid becomes turbid after a few minutes, and on allowing the saturated solution to stand for some hours, a body of an alliaceous odour begins to be separated at the bottom of the flask. If the liquid be now mixed with half its volume of concentrated hydrochloric acid, and heated to ebullition, it becomes limpid, and solidifies on cooling into a mass of felted needles of dazzling whiteness. These needles fuse at 218°; they are volatile without decomposition. Slightly soluble in water, they are more readily dissolved by alcohol, and still more so by ether. For the purpose of analysis they were recrystallized from boiling water, in order to exclude free sulphur, with which they might have possibly been contaminated. The numbers obtained in the analysis of the crystals unmistakeably establish their nature. The white crystals, as might have been expected, have the composition of the sulphaldehyde of the methyl-series.

$$CH_2S.$$

The analysis of the sulphur-compound fixes, of course, the presence of the corresponding oxygen compound among the products of the slow combustion of methylic alcohol.

A more minute examination of methylic aldehyde and its derivatives remain still to be made. It will be absolutely necessary to isolate the oxygen-term and to determine its vapor-density, in order to ascertain its molecular weight. If we remember the facility with which the aldehydes are polimerized, the question presents itself, whether the aldehyde formed by the slow combustion of methylic alcohol is represented by the formula

$$CH_2O,$$

or a multiple thereof. A similar remark applies to the sulphur-derivative. It deserves, moreover, to be mentioned that a compound isomeric with methylic aldehyde, the dioxymethylene ($C_2H_4O_2$) of M. Boutelrow, is known already; also that a sulphur-compound of the formula

$$CH_2S$$

has been obtained by M. Aimé Girard, who observed that bisulphide of carbon is reduced by the action of nascent hydrogen with disengagement of sulphuretted hydrogen.

In the course of next winter I propose to perform some former experiments on the product of the slow combustion of methylic alcohol, for the purpose, if possible, of isolating methylic aldehydes in a state of purity, in order to complete this inquiry.

ON THE IDENTITY OF
PHYSICAL WITH SO-CALLED VITAL FORCES.

A few weeks ago we gave our readers, in considerable detail, the remarks of Professor Tyndall on "Matter and Force," addressed to the working men of Dundee. As a sort of sequel to them, and in part an application of these questions to a special series of phenomena, we give in similar detail the words of Dr. Letheby addressed to the students of the London Hospital, at the opening of the current medical session. Prof. Letheby, it will be seen, did ample justice to the great and almost limitless powers of physical research when applied to subjects of biology, before treated mainly by powerful imagination and fruitless conjecture, and which were so settled to the satisfaction only of individual conjectures.

But Dr. Letheby took, as Professor Tyndall did, a still bolder position, as our readers will see, and drew a sharp line of separation between the possibilities and impossibilities of the application of our knowledge of force and matter. The range of possibility allowed by Dr. Letheby to science will be seen on examination to be much smaller than that of Professor Tyndall.

Three such authorities as Professor Tyndall, Huxley, and Letheby rarely lecture upon the aims and limits of science in such close succession, and such lectures will go far to give some definition to the present crude and indefinite popular ideas upon what philosophers think of the range of the various sciences.

Dr. Letheby writes as follows:—"We speak of healthy and unhealthy seasons, and in popular discussion are satisfied to refer the pandemic tendency of disease to the state of the weather. But how are they related? The circumstance of a higher or lower temperature than usual, a wetter or dryer season, the existence of more or less ozone in the air, the fluctuations of the barometer, etc., are, at present, but coincidences of facts; and they offer no explanation whatever of the etiology of disease. The same is also true of the observations which have been made concerning the local peculiarities of epidemics—as the altitude of a place, the condition of its soil, the water level in it, and the presence of putrid effluvia. All these, as in the last case, are but dimly seen to have an influence on disease. We know nothing of their real agency, and yet the wildest theories have been advanced in respect of them

At one time it is dogmatically asserted that epidemics are due to wet in the soil; at another to the water supply of the place. Now, it is the condition of the air which causes them, and then it is our filthy habits. Out of this confusion order must come, and the first step must be towards the investigation of the real nature of specific contagia. When this is known, and the laws which govern their action are determined, the caprices of epidemics will be explained.

"Nor are we much more advanced in the interpretation of healthy phenomena. The fiction of an Archœus, and the mechanical and chemical theories of life, have given place to the dogma of a vital force; but the recent progress of physical science has done much to dissipate our illusions concerning fictitious entities and mysterious forces. The study of physical phenomena, from a dynamical point of view, has led to the recognition of the fact that there is a definite correlation or mutual dependence of physical forces—'that the various imponderable agencies, or the affections of matter, which constitute the main objects of experimental physics, namely, heat, light, electricity, magnetism, chemical affinity, and motion, are all correlated, or have a reciprocal dependence; that neither taken abstractedly, can be said to be the essential or proximate cause of the other, but that either may, as a force, produce or be convertible into the other; thus heat may mediately, or immediately, produce electricity, electricity may produce heat, and so of the rest.' 'I believe,' says Mr. Grove, from whom I am quoting, 'that the same principles and mode of reasoning might be applied to the organic as well as the inorganic world, and that muscular force, animal and vegetable heat, etc., might, and at some time will, be shown to have similar definite correlations.' This was said almost a quarter of a century ago, and yet we are only just beginning to recognise the truthfulness of his hypothesis. A former teacher in this school, Dr. Carpenter, whose large acquaintance with physiology and physics especially qualify him for a searching examination of this subject, has fully confirmed the views of Mr. Grove. He has established the fact that there is not only a mutual relation between the so-called vital forces, which are concerned in the growth, multiplication, and transformation of tissues, in secretion, in muscular and other organic motion, and in nervous action, but that there is also a like relation between vital and physical forces. Believing with Mr. Grove that all these forces are but different modes of action of one and the same agency, he contends that the differences of action are due to the material substratum or medium through which it acts; 'that operating through inorganic matter it manifests itself in electricity, magnetism, light, heat, chemical affinity, and mechanical motion; but that when directed through organized structures, it effects the operation of growth, development, chemico-vital transformations, and the like; and is further metamorphosed through the instrumentality of the structures thus generated, into nervous energy and muscular power.' So that it is the speciality of the material substratum, thus furnishing the medium or instrument of the conversion or metamorphism of force, that marks the differences between physical and vital phenomena. I believe that the time is fast approaching when all special entities for the explanation of physical and physiological phenomena will be dispensed with, when the fundamental conceptions of matter and motion will be found sufficient for their explanation.

"In the case before us, it is not necessary to suppose that a living cell or primordial germ contains within itself, in a latent form, the whole organizing force which is required to build up the future plant or animal. Nor is it necessary to believe that vital force exists in a dormant condition in all matter that is capable of being organised, and that the living cell, in growing and multiplying, evokes and utilises it. It is enough that the cell is the medium for the conversion or metamorphosis of external physical forces into what are called vital actions.

"The external force which the cell chiefly converts is heat; and Dr. Carpenter lays so much stress on the dependence of the organizing forces of both plants and animals, on the continual agency of heat, that he regards their vital action as the correlation of it. It might, indeed, almost be said that the special and distinctive attribute of a living organisation is the power of converting heat into vital force. In plants it is entirely exercised in the growth and transformation of tissue; but in animals it is also rendered subservient to the production of nervous and muscular forces; and these manifestations of action are always exhibited in tissues, which retain their original cellular constitution. It is remarkable, too, that no cell has the power of performing two different operations at the same time: thus, says Dr. Carpenter, the *assimilating cells*, whose function it is to convert the raw material supplied by food into organisable *plasma*, exercise little or no chemical transformations; they do not undergo change of form; they do not exert any mechanical or nervous power, and they do not reproduce their kind. So again, the cells which are specially endowed with the powers of *multiplication*, as well as those which are engaged in *reproduction*, have in each case no other vital endowment. It is the same with the *secreting* cells and with the cells that are concerned in the production of *mechanical movement*, as the contractile cells of muscular fibrillæ, the ciliated cells of respiratory and other passages. Perhaps, also, it is the same with the cells, or cell-nuclei of the *ganglia*, and extremities of nerves which, in all probability, are the agents of *nerve-force*. This faculty of exercising but one function at a time is a marked peculiarity of physical forces. If, for example, the force derived from chemical action in a galvanic battery be made to act on a fine platinum wire, it will show itself as *heat;* or, if it be conducted through a coil of wire placed around a piece of iron, it will exhibit itself as *magnetism;* or, if it be conveyed through acidulated water, it will appear as *chemical action;* but all these manifestations of it cannot be fully exerted at the same time. There is, consequently, a certain quantivalence of action; in fact, the idea of the correlation and mutual dependence of forces, involves the necessity of a certain definite ratio or equivalence of action; for, as force cannot be created or destroyed, it must ever be acting in some fixed proportion. If, therefore, the heat and light force received by the plant be converted into vital force during the growth and transformation of tissue, there must be a quantivalence of action, and the force must endure. While the cell lives it is exerted in the manifestations of vital phenomena, but when it dies and decays it is converted into chemical action, and then into heat and sometimes light. In the case of the plant the transformation of heat force is chiefly concerned in the production of tissue; and so also in the animal during the processes of growth and repair; but when the latter engages in the main duties of its existence—the development of motion—it resorts to the affinities of its food, and as these are the embodiments, so to speak, of the light and heat, through whose agency they were formed, it may be said that the functions of the animal body are performed through the agency of cosmical force. The plant is the machine or medium whereby light and heat are converted into the force which forms tissue, and the animal is the machine or medium for changing the affinities of the tissue into other manifestations of force, and finally into heat. The one is accompanied by processes of deoxidation, and the building up of compounds; the other by processes of oxidation and pulling down, but throughout the whole of these changes there is the same force operating through different media.

"It follows from this, that there must be some definite relation between food and animal force. Hitherto it has generally been thought that the nitrogenous element of food is the exponent of its value, and that there is a direct relation between the waste of muscular tissue and the amount of work performed. Attempts have, therefore, been made to estimate the relation by observing the amount of nitrogen excreted, as urea, during exercise of different degrees of activity. The results, however, have shown that, under the best circumstances, the actual work performed exceeds that produced by the oxidation of the nitrogenous constituents of the food and worn-out muscle by more than thirty per cent.; and in some experiments which were made in 1866, by Drs. Fick and Wislicenus of Zurich, when they ascended the Foulhorn, which is 2,000 feet above the Lake of Brienz, in Switzerland, it was ascertained that the amount of work done in climbing the mountain, exceeded by more than three-fourths that which it would have been theoretically possible to realise from the oxidation of muscle, as indicated by the quantity of urea in the urine. Consequently, they conclude that muscular force is chiefly, if not entirely, derived from the carbo-hydrogen of our food, and that the muscle is no more than the machine for the production of motion. Like a steam-engine, it converts the affinities of the oxidised fuel into heat, and then into visible motion. Like it, also, its movements must cause decay and necessitate repair. The nitrogenous constituents of our food are chiefly concerned in this last process, and it is very doubtful whether as much force is not expended in this process as is afterwards produced by the oxidation of the worn-out tissues. In the consideration of this subject, however, we must not lose sight of the fact that there is a difference between sustained and temporary muscular activity. The herbivora, as the horse, the chamois, the stag, etc., are capable of great temporary exertion, but they are not equal to the carnivora for sustained energy; and with our own domestic animals, we find that they are capable of performing most work when they are supplied with vegetable food containing much nitrogen.

"Lastly, I will remark, in illustration of the general tendency of physiological pursuits, that great efforts are being made to determine the constitution as well as the composition of the fluids and tissues of the living body; for there has long been a desire to understand the way in which the common affinities of matter are controlled by the living cell. Broadly, the chemist has ascertained that the chemical functions of the plant are those of reduction or de-oxidation, whereby carbonic acid and water lose oxygen, the residual elements with the nitrogen of ammonia forming tissues. The functions of an animal, as I have said, are of an opposite nature, for instead of building up they pull down; and in using the tissues of plants as food they oxidise them, and finally restore them to nature as carbonic acid, water, and ammonia. 'The two extremes of these changes are,' to use the words of Gerhardt, 'carbonic acid, water, and ammonia, at one end; albumen, gelatine, fat, and cerebral matter at the other;' but the transitions to these extremes are countless, and are as yet almost beyond the ken of science. Who can tell us by what series of transformations the carbonic acid and water, received by the plant, are converted into vegetable acids, sugar, and fat? And still more mysterious are the phenomena which accompany the formation of tissues. Why is it that a living-cell, as we call it, possesses the power of transforming cosmical forces, light and heat, into cell-force; and in aggregating matter, how is it that it keeps common affinities at bay? That when it dies, as we express it, the matter so aggregated during life, decays, and comes again within the reach of ordinary affinities? What answer can we give to these questions? No other than that organic matter is the designed or appointed medium of these changes; and we can no more explain the phenomena than we can say why it is that mineral matters are the appointed media of other manifestations, as light, heat, magnetism, and electricity. Such questions are beyond the scope of the human intellect, and mark the limit of our understanding.

"Apart from these questions, however, the chemist has hope that he will penetrate the mystery of organic changes, in so far as the chemical combinations are concerned. Already he has found the clue to many of the vegetable processes of reduction, and has been able to produce a large number of organic compounds from carbonic acid, water, and ammonia, and even from the elements themselves: in fact, of the three great classes of alimentary substances, as Dr. Odling says, the production of the oleaginous is quite within his reach; the saccharine is almost within it; but the albuminous is still far beyond it. He has thus proved that the dogma of a vital-force, which has tyrannised so long over men's minds, has no foundation; for a host of organic compounds can be made artificially.

"And then with regard to the processes of oxidation which characterise the functions of animals, he has been able to imitate them to a much larger extent; for, by subjecting organic compounds to chemical transformations, he has produced a multitude of secondary products like those of the living world; recognising the fact that all secondary products of tissue-transformation are compounds of comparatively simple molecules, from which the elements of water have been eliminated—in other words, that they are composed of the residues of other molecules, the chemist has been able, not only to classify them into certain definite groups, but he has also been able to construct them after the fashion of organic nature. *Stearin* has been produced by joining the residues of glycerine and a fatty acid. *Sarcosine*, which is a muscle-product, from putting together the residues of acetic acid and methylamine. *Hippuric acid*, from the residues of benzoic acid and glycosine. *Taurine*, which is a constituent of bile, from the residues of isethionic acid, and ammonia. *Urea*, from the residues of carbonic acid and ammonia, and so of many others.

"In this way 'organic chemistry has achieved,' as Dr. Odling truly observes, 'a great *analytical* success. The compounds so elaborately built up by the living organism, it has pulled to pieces, and the pieces themselves it had arranged into natural series and groups of associated bodies.' It has also effected *synthetical* processes, and has built up organic compounds from mineral elements; and its hopes are that the entire processes of organic nature, short of making tissue, will ere long be seen and imitated. Presumptuous man! it may be thought, are you not striving for the impossible? In these explorations of vital phenomena, are you not trespassing upon holy ground? Can the finite measure the depths of the infinite? Already we have done so. 'The being of a day has pierced backwards into primæval time, deciphering its subterranean monuments, and inditing its chronicle of countless ages. In the rugged court and shattered pavement of our globe, he has detected those gigantic forces by which our seas and continents have changed places: by which our mountain ranges have emerged from the bed of the ocean; by which the gold and silver, the coal and the iron, and the lime, have been thrown into the hands of man as materials of civilisation, and by which mighty cycles of animal and vegetable life have been embalmed and intombed.' 'He has ascended the Empyrean, and by steps of physical research, has reached the visible boundaries of the universe, and has scanned with eagle-eye the mighty creations in the bosom of space. He has marched intellectually over the mosaics of the sidereal systems, and has followed the adventurous Phæton in a chariot which cannot be overturned.' 'Ideas like these, when first presented to a mind thirsting for knowledge, are apt,' says Sir David Brewster, from whom I am quoting, 'to disturb its equilibrium and unsettle its convictions. Should this be the mental condition of any one of you, be not alarmed for its results, for this species of scepticism is the infant condition of the uncurbed and generous intellect. There can be no convictions where there have been no perplexities and doubts; and that faith which comes in the train of early scepticism will finally rest upon an immovable foundation.'

"'Credulity, on the contrary, is a disease of feeble intellects and ill-regulated minds. Believing everything, and investigating nothing, the mind accumulates errors, till its overgrown faith o'er-masters its untutored reason. Such a facility of belief may in some cases claim the sympathy even of philosophy; but when it spurns the strict demands of inductive truth, and plants imagination at the door of the temple of science, it cannot be too severely reprobated, or too sternly shunned.'"

ON GAS ANALYSIS.

BY DRS. GRANDEAU AND TROOST.

(Concluded from Amer. Reprint, Jan. 1868, page 27)

IV. Mixture of Hydrosulphuric Acid, Carbonic Acid, and Nitrogen.

$HS \quad CO_2 \quad N$

The mixture is measured into a graduated tube standing over mercury. Introduce a solution of sulphate of copper, and agitate. The diminution of volume represents the amount of hydrosulphuric acid present.

Professor Bunsen recommends in preference for the absorption of the hydrosulphuric acid, a ball of binoxide of manganese impregnated with phosphoric acid. [To obtain a ball of binoxide of manganese which does not tend by reason of its porosity to absorb other gases besides hydrosulphuric acid, M. Bunsen prepares by levigation a fine powder, which is formed into a thick paste by a little water. This paste is then pressed into a mould round a platinum wire, the extremity of which is twisted into a spiral. The mould is then dried at a gentle heat, when the ball of binoxide is readily detached; for greater precaution, the sides of the mould may be smeared with a little oil. The ball is then moistened several times with a syrupy solution of phosphoric acid].

The remaining gas, transferred to an appropriate tube, is then submitted to the action of caustic potash, which absorbs the carbonic acid. The residue is nitrogen.

V. Mixture of Hydrochloric Acid, Hydrosulphuric Acid, Carbonic Acid, and Nitrogen,

$HCl \quad HS \quad CO_2 \quad N$

When the exact volume of the mixture has been accurately measured in the tube over the mercury, the hydrochloric acid is absorbed by means of a fragment of hydrated sulphate of soda fixed to the extremity of a platinum wire. [To obtain these fragments it is sufficient, according to M. Bunsen, to fuse ordinary sulphate of soda in its water of crystallisation, and to dip in several times the end of a platinum wire. There attaches to the wire a small lump of the sulphate which augments in volume with each fresh immersion].

Then remove the sulphate of soda, and measure the volume again. The diminution observed will represent the volume of hydrochloric acid gas.

The hydrosulphuric acid is absorbed by a ball of binoxide of manganese soaked in phosphoric acid, and the carbonic acid is afterwards absorbed by potash. The residue gives the nitrogen.

VI. Mixture of Sulphurous Acid, Carbolic Acid, Oxygen, and Nitrogen.

$SO_2 \quad CO_2 \quad O \quad N$

(Gas issuing from Craters of Solfatara).

The volume of the mixture being measured dry in a graduated tube over mercury, the sulphurous acid is absorbed by a ball of binoxide of manganese impregnated with phosphoric acid. After having removed this ball and noted the diminution of volume a fragment of potash is introduced to absorb the carbonic acid. The second diminution of volume will give the carbonic acid.

The oxygen can then be absorbed by potash and pyrogallic acid; or it may be estimated eudiometrically as described in No. 1. The nitrogen will remain as residue.

VII. Hydrosulphuric Acid, Carbonic Acid, Hydrogen, and Nitrogen.

$HS. \quad CO_2 \quad H \quad N$

(Fumeroles of Volcanoes)

Commence by absorbing the hydrosulphuric acid by introducing into the mixture a ball of binoxide of manganese impregnated with phosphoric acid. The absorption of the carbonic acid is then effected by means of a fragment of moist caustic potash.

The hydrogen is then estimated as at No. 2, either by the eudiometer, or by passing the mixture of hydrogen and nitrogen into a curved tube and introducing compact oxide of copper into the upper part of the bend. By heating for a quarter of an hour the part of the tube containing this oxide, the complete absorption of the hydrogen is effected. The nitrogen will form the residue.

VIII. Gas from Blast Furnaces where Wood is used.

$CO_2 \quad CO \quad H \quad N$

After having accurately measured the volume of the mixture over mercury, absorb the carbonic acid with a fragment of caustic potash.

Then estimate the carbonic oxide by introducing into the graduated tube a solution of subchloride of copper in hydrochloric acid, agitate, and the absorption will be complete.

Instead of introducing the liquid itself it will be better, as M. Bunsen advises, to introduce a ball of papier maché impregnated with this acid solution of subchloride of copper.

This experiment should be made over another separate mercurial trough, for the subchloride of copper attacks and fouls the mercury.

In withdrawing the ball impregnated with chloride, before reading off the volume, it is necessary to remove the hydrochloric acid vapours given off by the chloride.

The estimation of the hydrogen can then be effected as at No. 2, either by the eudiometer, or by absorption with oxide of copper. The nitrogen remains as a residue.

IX. Gas from the Mud of a Pond.

CO_2 CO H C_2H_4 N

First estimate the carbonic acid by means of potash then with a ball of papier maché introduce into the mixture a concentrated solution of subchloride of copper in hydrochloric acid. After the absorption has terminated, withdraw the ball of chloride and replace it by a ball of potash to remove the vapours of hydrochloric acid given off by the acid chloride. If the mixture which contains carbonic oxide also contains oxygen, the latter gas is determined first by pyrogallic acid and potash. The estimation of the hydrogen and carburetted hydrogen is then effected eudiometrically as in No. 3.

X. Coal Gas.

HS CO_2 CO C_4H_4 C_2H_4 H N

The mixture is first accurately measured in a graduated tube standing over mercury. The hydrosulphuric acid is then estimated by means of a ball of binoxide of manganese impregnated with phosphoric acid.

After removing the binoxide of manganese and measuring the remaining volume, introduce a ball of caustic potash, which absorbs carbonic acid.

The carbonic oxide is determined by means of acid subchloride of copper.

To determine the bicarburetted hydrogen introduce into the residue a fragment of coke soaked in a concentrated solution of anhydrous sulphuric acid in monohydrated sulphuric acid. The absorption of the bicarburet takes place very rapidly; the coke is then withdrawn and the acid vapours absorbed by potash.

The estimation of the hydrogen and proto-carburetted hydrogen is then performed as at No. 3. The nitrogen remains as a residue.

The bicarburetted hydrogen, as well as the protocarburet and the hydrogen, may also be estimated by the eudiometer. To effect this, pass the mixture of these three gases and the nitrogen into the eudiometer with three times its volume of oxygen, and pass the spark. The free hydrogen, as well as that of the carburets, combine with oxygen to form water, whilst the carbon becomes carbonic acid. Then pass the residue of the combustion into a graduated tube, and estimate the carbonic acid with potash and the excess of oxygen with potash and pyrogallic acid. The residue left after this double absorption gives the nitrogen.

The volumes of bicarburetted hydrogen, protocarburetted hydrogen, and hydrogen, may then be obtained by a simple calculation. The reactions which take place may be represented by the formulæ

$$\underbrace{H}_{\text{2 vols.}} + \underbrace{O}_{\text{1 vol.}} = HO$$

$$\underbrace{C_2H_4}_{\text{4 vols.}} + \underbrace{8O}_{\text{8 vols.}} = 4HO + \underbrace{2CO_2}_{\text{4 vols.}}$$

$$\underbrace{C_4H_4}_{\text{4 vols.}} + \underbrace{12O}_{\text{12 vols.}} = 4HO + \underbrace{4CO_2}_{\text{8 vols.}}$$

These equations show us:—1. That the combustion of bicarburetted hydrogen requires thrice its volume of oxygen, that of bicarburetted hydrogen double its volume, and that of hydrogen half only. 2. That the bicarburet produces double its volume of carbonic acid, and the protocarburet its own volume exactly.

Therefore, calling x, y, z the volumes of the bicarburet, the protocarburet, and the free hydrogen, of which the sum is known and represented by c, we have—

$$3x+2y+\frac{z}{2}=a$$

$$2x+y=b$$

$$x+y+z=c$$

a and b are the volumes of oxygen employed, and of the carbonic acid produced, volumes of which have been determined by experiment.

To find the values of x, y, and z, subtracting the first equation from the sum of the two others, we find—

$$\frac{z}{2}=c+b-a, \text{ whence, } z=2(b+c-a),$$

Then subtracting the last from the second we find—

$$x-2(b+c-a)=b-c, \text{ whence } x=3b+c-2a,$$

The second equation then gives—

$$y=4a-5b-2c.$$

ON THE

ACTION OF LIGHT ON CHLORIDE OF SILVER.

BY M. MORREN, DEAN OF THE UNIVERSITY OF SCIENCES OF MARSEILLES.

Take a glass tube 3 centimetres in diameter, and from 45 to 50 centimetres in length, close one end and introduce two bulbs, one containing nitrate of silver, the other chloride of potassium, in equal equivalents; fill the tube with a concentrated solution of chlorine in water, then carefully seal it before the blowpipe. Break the bulbs by agitation, when the result will be chloride of silver deposited in an excess of chlorine water. If the tube be exposed for several days to the rays of the sun, the following facts may be observed. 1st. As long as the liquid preserves the yellow colour given to it by the chlorine, the chloride of silver remains white. 2nd. When this yellow colour disappears by the action of the chlorine on the water under the influence of light, the chloride of silver slowly assumes not the very deep violet which we see in the reactions of photography, but a red brown, which at first only appears gradually and on the surface, but in time penetrates the entire white mass, provided the tube is sufficiently agitated, and submitted to the action of a bright sun. 3rd. The tube being placed, if not in obscurity, at least in the diffused light of the laboratory, the brown colour disappears gradually, and the chloride of silver reassumes, in all its intensity, its original white aspect. Replace the tube in the sunlight, and the colouration takes place afresh, to disappear again when the tube is returned to the shade, and so on indefinitely. The interesting questions involved in these successive evolutions have occupied me much, and I intend to devote still more attention to them.

ON THE

ARTIFICIAL PRODUCTION OF BENZOIC ACID FROM NAPHTHALIN.*

BY DR. ADOLF OTT.

Benzoic acid is a crystallizable, soft, white body, inodorous when pure, but smelling like gum benzoin when gently warmed; it usually has a faint aromatic odour, sweetish taste, but produces a burning sensation in the throat. Litmus is feebly reddened by it; it fuses at 250° Fahr., sublimes at 300° Fahr., and boils at 462° Fahr., yielding a vapour of the sp. gr. 4·27. It exists in benzoin, in the tolu balsam, in the gum of *Xanthorrœa hastilis*, in castor. and has also been met with in the urine of man and herbivorous animals, etc. Benzoic acid is also found by the oxidation and decomposition of oil of bitter almonds, protochloride of benzoil, hippuric acid, etc., and further by the action of a solution of caustic baryta on populin and other organic compounds. It has hitherto, nevertheless, only been prepared from the gum benzoin, either by subliming the same—a process existing since 1703 (*vide* Turquet de Mayerne, "Pharmacopœia in Oper. medio." London)—or by a process, due to Wœhler, which we will here not further describe, as it can be found in nearly any larger hand-book of chemistry. We only will mention that it is used in medicine to some extent, viz., against affections of the throat; largely in the manufacture of aniline blue; and for the preparation of tobacco sauces.

This interesting substance, of which the chemical formula is $C_{14}H_6O_4$, has now lately been produced, at a rate allowing the manufacturer large profits, from a substance which, in its crude state, can be got for less than one cent a pound. In the following I will proceed to describe the process, but first mention something about naphthalin:—

This hydrocarbon (formula $C_{20}H_8$) was discovered in 1820, by Garden, in the coal-tar of the gas works, and studied by Liebig, Faraday, Wœhler, and others. It is a colourless, inflammable solid, of a burning aromatic taste, and peculiar smell. Its specific weight is 1·048, its melting point 175°, and its boiling point 428° Fahr. It sublimes unaltered in laminæ, and can also be obtained in rhomboidal crystals from its alcoholic solution. It is inflammable, and burns with a very smoky flame. Its derivates have principally been studied by the celebrated Laurent.

The process now by which this hydrocarbon is transformed into benzoic acid is the following:—

(1.) *By the Process of Laurent.*—The naphthalin is transformed into a modification of the bi-protochloride of naphthalin, $C_{20}H_8\ 2Cl_2$.

(2.) The bi-protochloride of naphthalin is converted by oxidation into phtalic acid, $C_{16}H_4O_6\ 2HO$, and the latter into phtalate of ammonia, $C_{16}\ H_4\ O_6\ 2NH_3$.

(3.) We obtain then the phtalamid, $C_{16}\ H_5\ NO_4$, simply by subjecting the phtalate of ammonia to distillation.

(4.) By distilling the product obtained by process 3 with hydrate of lime, benzonitril, $C_{14}H_5N$, is formed.

$$C_{16}H_5NO_4 + 2CaO = C_{14}H_5N + 2CaO, CO_2.$$

(5.) In boiling the latter with a solution of caustic soda, benzoate of soda is formed, from which benzoic acid is precipitated by hydrochloric acid.

This is, in short, the process as followed by John Castelhay, of Paris, and of which Menier, the Secretary of Class 44 of the International Exhibition of Paris, says that it is the most important discovery made in technical chemistry since the London Exhibition of 1862.

CARBOLIC OR PHENIC ACID AND ITS PROPERTIES.*

BY DR. F. CRACE CALVERT, F.R.S., ETC.

Gentlemen,—I have readily accepted the friendly invitation of your illustrious president, M. Dumas, to submit to your notice some facts relative to carbolic acid. But before doing so, allow me to express publicly the feelings of gratitude which I owe to France for having opened to me the way to the profession which I pursue with such pleasure. In truth, it is to the sympathy of scientific men of this country, to the friendly assistance of one of your scientific celebrities, M. Chevreul, and to the liberality of your institutions, that I owe the knowledge I have acquired, the elements of which I gained at the Gobelins, and at the Museum of Natural History, during the stay I made there. After these remarks I will proceed with the subject of my lecture.

No doubt most persons present are aware that when coals are submitted to the action of a dull red heat, in a retort, products are obtained which may be grouped into four classes.

1. Gaseous products, commonly called coal gas, and which are now employed in so general a manner as means of illumination, sources of heat, and motive power.
2. Water, containing ammonia and ammoniacal salts, substances which chemistry purifies, modifies, and which are then utilised in agriculture, manufactures, and medicine.
3. There distils with the above products a black, sticky substance, of an unpleasant odour, called tar.
4. There remains in the retort a solid, porous body, which is known to us all as coke.

When the above-mentioned product called tar is submitted to distillation, water first comes over, then there distil jointly with this fluid liquid carburetted hydrogens, which, being lighter, float on it and are therefore called light oils of tar; and, lastly, compounds heavier than water are collected, which bear the name of heavy oils.

It is these heavy oils which were the first tar products utilised in manufacture. Their consumption made such rapid progress in England, that special manufactories were established for their preparation, and these works were, for a long period, the only ones in which tar products were produced. Most of them were established between 1837 and 1847, for the production chiefly of coal naphtha, used for many purposes, and heavy oils, employed for the preservation of railway sleepers,

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* A paper read before the Polytechnic Association of the American Institute.

* Lecture before the Society for the Encouragement of National Industry in France.

by a process discovered by Mr. John Bethell, by means of which they are preserved twelve or fifteen years; whilst without it they decay after three or four years. I have much pleasure whilst on this subject, in calling your attention to a very remarkable and very complete work upon the creosoting of wood, by M. Forestier, chief engineer of the department of La Vendée, assisted by M. Marin. These gentlemen have made numerous experiments, the result of which is that wood thus treated is preserved from decay in water as well as under ground, and, what is important, wood is no longer destroyed by that very destructive insect the *teredo*.

Lastly, there remains in the still (after the heavy oils and semi-solid substances have distilled off) a product which is fluid at the high temperature at which this operation is conducted, but which, when exposed to the natural temperature of the atmosphere, becomes hard and brittle, and is known under the name of pitch. This product, as you are aware, is largely employed in Paris under the name of asphalte, bitumen, etc., to make the foot pavements and public walks, as well as for the manufacture of a sort of concrete, called in England patent fuel.

After this rapid sketch of the products given off during the distillation of coal and tar, allow me to call your attention to *carbolic* or *phenic acid*, also called carbolic or phenic *alcohol;* in fact the latter is the most appropriate name for this substance, as its properties are not those of an acid, but that of an alcohol.

It is now twenty years since Laurent, the eminent chemist, first pointed out an easy method of extracting carbolic acid from coal tar. It consisted in submitting the light oils to a fractional distillation, and then treating with a concentrated solution of potash those products which had distilled at a temperature between 160° and 200°, separating the alkaline solution from the hydrocarbons which floated on it, and then neutralising the alkali by an acid which liberated the carbolic acid.

Such was Laurent's method of preparing carbolic or phenic acid, but pure carbolic acid was only there in a very small proportion: it was, in fact, a mixture composed chiefly of different liquids similar in properties and composition to carbolic acid, and, though Laurent succeeded in obtaining solid carbolic acid, still the process devised by him was too expensive to answer on a manufacturing scale; and, besides, his method of operating was too complicated.

In 1847, Mansfield, and towards 1856, M. Bobœuf, made known processes which, in fact, were only a modification of Laurent's, for they consisted principally in employing caustic soda instead of potash, and in treating the whole of the light oils instead of a special portion of them, as Laurent had done; still, by these processes, a very impure acid was obtained, from which it was very difficult, as experience has shown us, to extract pure carbolic acid; however, in a commercial point of view, the process of these gentlemen was a step in the right direction. This method was followed by Mr. Clifts in manufacturing some carbolic acid for me about the year 1847; and it was this impure acid which was employed by several chemists who, like myself, studied the properties of this substance, and who were endeavouring to apply it usefully; and I succeeded at about that time in applying it to the production of picric acid, or in preventing the transformation of tannic acid into gallic acid, in tanning substances, or finally, in the preservation of subjects for the dissecting-room. M. Bobœuf also made use of it in preserving organic bodies from putrefaction, a property which has received of late very important applications.

In 1859, M. Marnas, of the firm of Guinon, Marnas, and Bonnet, of Lyons, came to Manchester, and asked me to furnish him with a purer carbolic acid than had been as yet manufactured. He showed me a white and crystalline product, which he gave as a specimen. It was then necessary to make new experiments, and we (F. C. Calvert and Co.) discovered that the best mode of preparation was not by treating light or heavy oils or tar with concentrated alkalies, but, on the contrary, by treating the impure benzines of commerce or naphthas with weak alkaline solutions. By this means a semi-fluid, blackish product was obtained, a little heavier than water, of a density of 1·06, and which contained 50 per cent. of real carbolic acid, which acid we managed to separate in part by careful distillation; and it was this product which was employed by Messrs. Guinon, Marnas, and Bonnet, and others, till 1861, for the manufacture of colours derived from carbolic acid. At this period the colours obtained from aniline were so fine and brilliant that, to keep up a comparison with them, it was necessary to improve those derived from carbolic acid. To effect this it was necessary to improve the quality of the carbolic acid then manufactured, and, after some trials, we produced carbolic acid in white detached crystals, melting at between 26° and 27°; and this is the product which is now generally employed in commerce and industry, as witness the specimens which are to be seen at the present Universal Exhibition. In 1863 this relative purity was insufficient, and the same firm which had required the improvements which I have before named asked us to try and make it still purer. We again set to work and produced commercially Laurent's phenic alcohol or carbolic acid; that is to say, a substance melting at 34°C., and boiling exactly at 186°. This became a very important commercial product for us, and we delivered large quantities monthly.

From this time I made many efforts to draw the attention of the medical profession to the really remarkable therapeutic properties of carbolic acid, but the tarry and sulphuretted odour which it still possessed was a serious obstacle to its application. I soon succeeded in overcoming this difficulty, and towards the end of the year 1864 our firm was in a position to deliver in considerable quantities, carbolic acid deprived of sulphuretted compounds, and therefore fit for all medicinal uses. But I am glad to say that the series of improvements in the manufacture of pure carbolic acid, or phenic alcohol, did not stop there, for towards the end of last year I discovered a process which now enables me to show you a product completely deprived of all disagreeable odour and tarry flavour, and, in fact, as pure, though extracted from tar, as if it had been produced artificially by the help of the reactions recently discovered by Messrs. Wurtz and Kékulé, based upon the direct transformation of benzine into carbolic acid, or by the well-known changes by which it may be obtained from salicylic acid, or nitro-benzoic. This new phenic or carbolic acid is distinguished from Laurent's in being soluble in 20 parts of water, whereas the latter requires 33. It is fusible at 41°, instead of 34°, and boils at 182°, instead of 186°, but it gives, like Laurent's, the blue colour described by M. Berthelot when mixed with ammonia, and to the solution is added a small quantity of a hypochlorite; the same effect is also produced when you expose to the vapours of hydrochloric acid a chip of deal soaked in this pure carbolic acid.

It was supposed that, as Laurent's phenic acid had a constant boiling and crystallisation point, it was a pure and definite substance. Now, the production of our new acid shows it is nothing of the kind, the product of Laurent being only a combination of our pure carbolic acid and a liquid homologue; for when to the acid of Laurent is added a certain proportion of water, and the mixture is exposed to a temperature of 4° C., it deposits a crystalline substance in large octahedrons; which is a hydrate of carbolic or phenic alcohol, that it to say, carbolic acid combined with an equivalent of water of crystallisation. This fact is important in a chemico-theoretical point of view, for it exhibits the only example known of an alcohol which, combining with water, forms a crystallised hydrate. By removing from this hydrate the equivalent of water, which it contains, carbolic or phenic acid is obtained in its purest state.

We will now rapidly glance at the applications which have been made of this remarkable substance for sanitary purposes, in medicine, agriculture, and manufactures.

The disinfecting or rather antiseptic properties of carbolic acid are very remarkable. The beautiful researches and discoveries of M. Pasteur [*] have shown that all fermentation and putrefaction is due to the presence of microscopical vegetables or animals, which, during their vitality, decompose or change the organic substances, so as to produce the effects which we witness, and as carbolic acid exercises a most powerful destructive action upon these microscopic and primitive sources of life, carbolic acid, therefore, is an antiseptic and disinfectant much more active and much more rational than those generally in use.

It is necessary that I should here make a few remarks, explanatory of the distinctions between *deodorizers*, *disinfectants*, and *antiseptics* :—

Deodorizers.—All substances merely acting as such are neither disinfectants nor antiseptics, as they simply remove the noxious gases emitted from organic matters whilst in a state of decay or putrefaction, without having the property of arresting decomposition or fermentation. For it has been proved that the source of infection or contagion is not due to noxious gases or bad smells (being merely indicators of its probable existence), but, as we shall see presently, to microscopic spores floating in the atmosphere, and which by their ulterior development and propagation, are believed to be the true source of contagion.

Disinfectants.—Under this head may be classed bleaching powder, or chloride of lime, sulphurous acid, and permanganate of potash; they first act as deodorizers, and then as disinfectants, but they must be employed in large quantities, to thoroughly oxidize or act upon organic matters, so as to prevent them from again entering into decomposition; but still it is known that if the organic substances so acted upon are exposed to the atmosphere, they will again experience decay and putrefaction; they are, in fact, more destructive agents than disinfectants, and they are never antiseptics.

Antiseptics.—Antiseptics, such as corrosive sublimate, arsenious acids, essential oils, carbolic acid, etc., act as such by destroying all source of decay and decomposition, that is to say, they destroy or prevent the formation of the germs of putrefaction and fermentation, without acting upon the mineral or vegetable matters present. The advantage of their use is, therefore, that they act, when used in small quantities, upon the primary source of all organic matters in a state of decay; further, they are deodorizers, for they prevent the formation of offensive odours, and consequently they are antiseptics, disinfectants, and deodorizers. The great advantages which carbolic acid possesses over all other antiseptics are, that it cannot be used for any illegal purpose, as arsenic or corrosive sublimate.

And allow me further to add that disinfectants, such as chlorine, permanganate of potash, or Condy-fluid operate by oxidizing not only the gaseous products given off by putrefaction, but all organic matters with which they may come in contact; whilst carbolic acid, on the contrary, merely destroys the causes of putrefaction, without acting on the organic substances. The great difference which therefore distinguishes them is, that the former deals with the effects, the latter with the causes. Again, these small microscopic ferments are always in small quantities as compared to the substances on which they act, consequently a very small quantity of carbolic acid is necessary to prevent the decomposition of substances; therefore its employment is both efficacious and economical.[*] Moreover, carbolic acid is volatile; it meets with and destroys, as Dr. Jules Lemaire says,[†] the germs or sporules which float in the atmosphere, and vitiate it; but this cannot be the case with Condy's fluid, chloride of zinc or iron, which are not volatile, and which act only when in solution, and are mere deodorizers. This is why carbolic acid was used with such marked success, and therefore so largely, in England, Belgium, and Holland during the prevalence of cholera and of the cattle plague.[‡] Mr. W. Crookes, F.R.S., not only states: "I have not yet met with a single instance in which the plague has spread on a farm where the acid has tbeen freely used;" but he has also proved, by a most interesting series of experiments, that the gases exhaled from the lungs of the diseased cattle contained the germs or sporules of the microscopic animals discovered by Mr. Beale in the blood of such animals; for Mr. Crookes having condensed on cotton wood these germs, and having inoculated the blood of healthy cattle with them, they were at once attacked with the disease. As to the value of carbolic acid for preventing the spread of cholera, among many instances which I could cite, allow me to mention two special instances: First, Dr. Ellis, of Bangor, says:—I have in many instances allowed whole families to return to cottages in which persons had died from cholera, after having had the cottages well washed and cleansed with carbolic acid, and in no case were any persons so allowed to enter such purified dwellings attacked with the disease. My friend, Professor Chandelon, of Liége, has stated to me that out of 135 nurses who were employed to attend upon the cholera patients —and they must have been numerous, for 2,000 died— only one nurse died, but the nurses were washed over and their clothing sprinkled with carbolic acid. In fact the antiseptic properties of carbolic acid are so powerful that 1-1000th, even 1-5000th will prevent the decomposition, fermentation, or putrefaction for months of urine, blood, glue solution, flour, paste, fæces, etc., etc.,[§] and its vapour alone is sufficient to preserve meat in confined spaces for weeks; and even a little

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[*] See "Comptes Rendus de l'Academie des Sciences" for several years past, and my lecture at the Society of Arts, 1863-65.

[*] See remarks at the end of lecture.

[†] See "Comptes Rendns," 1867.

[‡] See the Third Report of the Royal Commissioners on the Cattle Plague, and especially the valuable report of Mr. W. Crookes, F.R.S.

[§] See private reports made by Dr. Allen Miller, F. R. S., and Mr. Crookes, to Messrs. F. C. Calvert and Co.

vapour of this useful substance will preserve meat for several days in ordinary atmosphere, and prevent its being fly-blown; lastly, 1-10,000th has been found sufficient to keep sewage sweet, for Dr. Letheby states, in a letter addressed to me, that through the use of such a quantity of carbolic acid in the sewers of London during the existence of cholera last year, the sewers of the City were nearly deodorized. And I am proud to say, that the British Government have decided to use exclusively our carbolic acid (as an antiseptic and disinfectant) not only on board Her Majesty's ships, but in other Government departments; and that no other deodorant or disinfectant, such as chlorides of zinc or iron, permanganate of potash, or any disinfecting powder, shall in future be used for such purpose. Although questions of public health are the province of medicine, still permit me to say a few words on the medicinal properties of carbolic acid. This question deserves to be treated thoroughly, for phenic acid is susceptible of so many applications in this direction, its properties are so marked, so evident, and so remarkable, that they cannot be made too public, and it is rendering a service to mankind to make known some of the employments of so valuable a therapeutic agent.

I wish all who are listening to me were medical men, for I could show, by numerous and undeniable facts, the advantage they might derive from pure carbolic or phenic acid, and if my testimony was not sufficient to convince them, I would invoke the authority of men justly esteemed amongst you. I would recall to you the words of the good and learned Gratiolet, and those of Dr. Lemaire, showing that carbolic acid is the most powerful acknowledged means of contending with contagious and pestilential diseases, such as cholera, typhus fever, small-pox, etc.* Maladies of this order are very numerous, but in carbolic acid we find one of the most powerful agents for their prevention; for besides many instances which have been cited to me, I may add that I have often used it in a family in which there were eight or ten children, and that none of the family have suffered from those diseases except those who were attacked previously to the employment of carbolic acid about the dwellings in which such diseases existed. Besides its antiseptic action, the caustic properties of carbolic acid are found useful; most beneficial effects are obtained from it in the treatment of very dangerous and sometimes mortal complaints, such as carbuncle, quinsy, diphtheria, etc., as shown by Dr. T. Turner, of Manchester; and also in less severe affections, such as hæmorrhoids, internal and external fistulas, and other similar complaints. But what must be especially mentioned is the employment of carbolic acid in preserving in a healthy state certain fœtid purulent sores, and preventing the repulsive odour which comes from them, an odour which is the symptom of a change in the tissues, and which often presents the greatest danger to the patient. The services which carbolic acid renders to surgery can be judged of by reading several most interesting papers on compound fractures, ulcers, etc., lately published in the *Lancet* by J Lister, F.R.S.; and allow me to draw your special attention to the following paragraphs which are to be found in his paper published in that journal of the 25th September, 1867:—"The material which I have employed is carbolic or phenic acid, a volatile organic compound, which appears to exercise a peculiarly destructive influence upon low forms of life, and hence is the most powerful antiseptic with which we are at present acquainted. The first class of cases to which I applied it was that of compound fractures, in which the effects of decomposition in the injured part were especially striking and pernicious. The results have been such as to establish conclusively the great principle that all the local inflammatory mischief and general febrile disturbance which follow severe injuries are due to the irritating and poisoning influence of decomposing blood or sloughs. These evils are entirely avoided by the antiseptic treatment, so that limbs which otherwise would be unhesitatingly condemned to amputation may be retained with confidence of the best results. Since the antiseptic treatment has been brought into full operation, and wounds and abscesses no longer poison the atmosphere with putrid exhalations, my wards, though in other respects under precisely the same circumstances as before, have completely changed their character; so that during the last nine months not a single instance of pyæmia, hospital gangrene, or erysipelas has occurred to them." My hearers can also witness the same remarkable results by visiting the two sick wards of Dr. Maisonneuve, at the Hotel Dieu. Further, I must not overlook the valuable application made of it to gangrene in hospitals by the eminent physician, James Paget, Esq.; and lastly, it has been used by many of the most eminent medical men with marked success in those scourges of humanity, phthisis and syphilis.

In agriculture our firm has stimulated the employment of the carbolic acid for the cure of certain diseases very common to sheep—scab, for example. The method of treatment customary in similar cases was very imperfect as well as dangerous, whilst with carbolic acid this malady is cured, and without danger to the animal, by dipping it for a minute, often only for some seconds, in water containing a small quantity of carbolic acid. For this purpose pure acid would be too expensive, and is not used, nor concentrated acid, which ignorant men who have the care of sheep would not know how to use, but by the help of soap an emulsion of carbolic and cresylic acids is made. After having shorn the sheep it is dipped in this mixture; a single immersion in a bath containing 1-60th of it is sufficient to effect a cure. After scab, the foot-rot is one of the worst and most frequent complaints. Carbolic acid is also for that an efficacious remedy. For this a mixture is made of the acid and an adherent and greasy substance, capable of forming a plaster, which is made to adhere to the animal's foot for two or three days, preventing the contact of the air, allowing thereby time for the application to have its effect. But if the flock be numerous, it would take a long time to dress the four feet of each animal one after another,; so, to make it more easy, a shallow tray is made of stone—a sort of trough; this is filled with the medicated mixture, and the sheep made to pass through it; their feet being thus impregnated with the required substance. Permit me also to state that cattle cease to be annoyed with flies, etc., if washed with this solution, or a weak solution of carbolic acid; and a first-rate salve can be prepared by adding 10 per cent. of carbolic acid to butter, or any other fatty matters used for such purpose.

Manufacturers have not yet availed themselves of one tithe of the valuable properties of carbolic acid, and in this direction a new field is open to its use; still I may cite a few instances. The preservation of wood has been already referred to, and thanks to carbolic acid, the great trade in skins and bones from Australia, Monte Video, Buenos Ayres, etc., will ultimately be

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* See Mr. Lemaire's work on Phenic Acid. Paris 1865.

benefited. Wild animals living there in herds are slaughtered by thousands. Formerly they came to us in a bad state, half putrid, emitting an insupportable odour, and only fit for manure; in this state their price was not more than 150 francs the 1,000 kilogrammes, now, with carbolic acid treatment, they arrive perfectly preserved; they can be employed for all the uses to which green or raw bones are usually applied, and the value of bones is raised as much as from 250 to 300 francs. Hides also often arrive putrid, although they have been dried rapidly in the sun or salted, which latter process, as you are aware, necessitates long and costly manipulations; whilst it is only necessary to immerse them for twenty-four hours in a solution of 2 per cent. of carbolic acid, and dry them in the air, to secure their preservation. It is probable that in a short time the blood, intestines, and other parts of these animals will be, by means of carbolic acid, converted into manure, and imported into this country. In England carbolic acid is used in the preservation of guts at the gut-works, for keeping anatomical preparations, and the preservation of all animal matter. Carbolic acid is also utilized in preventing the decomposition of the various albumen, flour, and starch thickeners used in calico printing, as well as gelatine or bone size, employed for sizing fustians and other cotton goods.

One of the most interesting chapters in the history of carbolic acid is certainly that which relates to the production of colouring materials; they alone enter into comparison with those derived from aniline, and often enter into successful rivalry with them. Amongst the colouring matters derived from carbolic acid, the most important is, without fear of contradiction, picric acid.

The discovery of this acid dates back to a distant period; it was studied by Welter, and was called Welter's bitter. But it was my illustrious master, M. Chevreul, who in 1807 discovered the real chemical composition of picric acid, and who demonstrated that picric acid was often produced when organic matters were acted upon by nitric acid. Further, M. Chevreul discovered in the products of the oxidation of organic substances through nitric acid two different compounds, which he called *amer au minima* and *amer au maxima*, the latter being picric acid. This acid was again examined by Laurent in 1841, when he demonstrated that the true generator of picric acid was phenic acid; that in the action of nitric acid on the latter it formed three nitrogenated compounds, mononitrophenic acid, binitrophenic acid, trinitrophenic acid, the latter being also picric acid.

These interesting results of Laurent would perhaps have remained for a long time without any commercial value, if picric acid had not been applied to dyeing, in 1847, by M. Guinon, sen., of Lyons. Since then the use of this acid, producing magnificent yellows, has been much extended, and what has contributed to its employment is that, conjointly with indigo, it gives ordinary greens, or of *vert Lamiere*, with Prussian blue, so that its consumption may be valued at from 80 to 100,000 lbs. annually; our firm alone produces more than 300 lbs. weekly; and when it is considered that 1 lb. of picric acid dyes to an intense shade 70 to 100 lbs. of silk, or 40 to 50 lbs. of wool, the enormous quantity of textile materials dyed by this single product may be appreciated.

The processes followed for the preparation of picric acid are still those which Laurent indicated in 1841; but instead of using carbolic acid loaded with the heavy oils of tar, as M. Guinon had done, I sought to diminish the quantity of nitric acid, employed in mere waste, on the heavy oils of tar, which were then mixed with carbolic acid, and I am glad to say that I succeeded in doing so in 1849 by employing carbolic acid containing only some of its liquid homologues. In 1856, M. Bobœuf took out a patent in France for making picric from carbolic acid. But picric acid was still at a high price, and it is only since our firm has manufactured cheap carbolic acid that picric acid can be produced free of resinous materials which prevent its purification and its being sold at a low price; in fact, owing to our pure carbolic acid, picric acid is now obtained chemically pure; this product, which was sold some years since at 15s. to 20s. per lb., is now sold at the rate of 3s. Further, I may add that to apply it in a quick and economical manner it is desirable to add to the dye bath a small proportion of sulphuric acid; this method of manipulation, which is not generally known, is very important, for it is only in this way that the textile materials can be readily dyed and the baths exhausted.

I shall now have the pleasure of calling your attention to the production of two new colouring substances derived from picric acid:

1. Picramic acid was, in the first instance, obtained by Wœhler; by making sulphate of iron act upon picric acid, and neutralising with caustic barytes, when a deep brown salt was produced from which he separated the baryta by the help of sulphuric acid, and by these reactions Mr. Wœhler obtained an acid to which he gave the name of nitro-hematic acid; but it is to Mr. Aimé Girard that we owe the practical process by means of which we are able to manufacture great quantities of picramic acid. This acid imparts to silk a beautiful series of brown tints similar to those obtained from catechu.

2. Isopurpurate of ammonia. It is with much pleasure that I noticed, at the Exhibition, in M. Casthelaz's case, a coloured substance, known in the trade by the name of soluble garnet, which, I am informed, is used especially by M. Chalamel, of Puteaux; this substance is particularly remarkable, as it is isomeric with the *purpurate of ammonia or murexide.* Although the preparation of this material was first pointed out by M. Carey, still it is really due to a previous observation by Hlasiwitz, who called attention to the reaction of cyanide of potassium upon picric acid, and to which chemical reaction we owe the knowledge of manufacturing the isopurpurate for industrial purposes.

Before taking leave of picric acid it may not be without interest that I should state a curious application which has been made of the explosive property of its salts. During these last few years the picrate of potassium has been employed in great quantities by Mr. J. Whitworth, for charging the bombs for destroying the iron plating of ships. When the projectiles thus prepared strike the iron masses, the enormous propelling force with which they are expelled from the gun is instantaneously converted into heat, and to such an extent that the ball becomes red-hot, the heat decomposes the picrate of potash, and a violent explosion ensues, owing to the enormous quantities of vapours and gases which are thus produced in an instant of time. Whilst the alkaline picrates are endowed with such formidable properties, they also possess others which are useful for the alleviation of human misery. Picric acid is an efficacious remedy in intermittent fevers. Persons affected with such types of fever, upon whom quinine has lost all its beneficial effects by continuous usage of it—and this is the case with some of our soldiers who return from India—derive, I am glad to say, wonderful benefit from the use of picric acid and picrates, as Dr. Aspland has proved to be the case at the military hospital at Dukinfield. The knowledge of this fact may be useful in districts in which poor populations exist, for it affords them a cheap febrifuge; and, moreover, picric acid is not dangerous, as arsenical preparations are, nor does it derange the stomach like quinine.

To return to the colours derived from carbolic acid, allow me to remind you that when, in 1834, Runge discovered phenic acid amongst the products of coal-tar, he observed the existence of two colouring substances, to which he gave the name of rosolic acid and brunolic acid.

I will not detail here the processes by which Runge extracted these substances from the residue of coal-tar by means of lime, nor the method adopted by Messrs. Smith, Dussart, and Jourdin, for producing these substances by direct oxidation of phenic or carbolic acid, but will describe rapidly the process which we now use to manufacture rosolic acid, and which should not be attributed, as is generally believed, to M. Kolbe, as it is due to M. Jules Persoz, the son of the celebrated professor of tinctorial chemistry in the Conservatoire des Arts et Métiers. His process consists in making oxalic acid act upon sulphophenic acid at a temperature of about 160°, and the product which results from it has the bronze green appearance of cantharides. To render it suitable for employment in dyeing, it is only necessary to wash it so as to separate from it all the sulphuric acid with which it is contaminated. It is then sold under the name of yellow coralline or aurine. It was our firm who first, in 1863, discovered that rosolic acid thus prepared could be employed directly as a dye, and introduced it to dyers under the name of aurine. This substance gives to silk and albumenised cotton magnificent orange colours, like those of basic chromate of lead or of turmeric. In 1860, M. Persoz, jun., discovered also that if rosolic acid was heated under pressure with ammonia it gave rise to a red substance, which he called *péonine*. Messrs. Guinon, Marnas, and Bonnet perfected the manufacture of *péonine*, and gave it the name of red coralline. This colouring substance gives to silk and worsted a flame-coloured tint and brilliant scarlets. This firm was also the first to produce and introduce, towards the end of 1860, a blue dye, derived from carbolic acid, or, more so, rosolic acid, which they called azuline. Azuline is obtained by heating for several hours, at a temperature of about 180°, a mixture of rosolic acid and aniline. It is only necessary then to treat this product with sulphuric acid, and to wash it with benzine, to produce a beautiful blue colouring matter, which presents, when dry, a red mass with gold-coloured tints. Azuline, although discovered before the aniline blues, which have since become formidable rivals to it, is still, I may add, manufactured in competition with them.

To Messrs. Guinod, Marnas, and Bonnet is also due the first production of a green derived from coal-tar products. It was manufactured in 1863, therefore some months before the appearance of an aniline green, known as *vert d' Uzebe*, which, however, with the exception of the iodine greens, is the only one now employed in dyeing. Viridine was obtained by this firm from a mixture of aniline, benzoic, and resolic acids.

Phenicienne, discovered in 1863 by M. Roth, is another colouring matter derived from phenic acid; it produces fast colours, from a deep garnet red to a golden buff. Phenicienne is produced by the action of nitro-sulphuric acid upon carbolic acid.

I will now, with your permission, gentlemen, leave for a few seconds the products derived from phenic acid, in order to place before you certain claims to some inventions not sufficiently recognised by writers on aniline colours. In 1860, Messrs. Clift, Lowe, and I, took out a patent for the direct production on prints, of a green called emeraldine, and the deep blue called azurine, a blue which resembles indigo, and which really, when printed in a concentrated form, may be confounded with a black. And although I do not desire to deprive Messrs. Lightfoot, Carlos Kœchlin, and Lauth, of any of the merit which belongs to them for the production of the beautiful black which every one must have admired in the Exhibition, still I may be permitted to remark that their process is based upon the oxidation of aniline by chlorate of potash, and is therefore based on our patent, previously secured to their discoverers. The difference between their process and ours consists in the addition of a salt of copper, which addition is so important that I have no hesitation in saying it has decided the success of a black which now stands unrivalled.

I cannot conclude this retrospective view without calling your attention to a fact which seems to have escaped my colleagues; it is that the majority of the beautiful colours obtained from aniline are due to the industrial application of a discovery made by your illustrious president, M. Dumas, more than thirty years ago. The discovery I mean is the principle, so rich and fruitful, which he named the law of substitutions —a law which has thrown so bright a light on modern chemistry, and which has prepared the way for such brilliant achievements, and which, I say, has also been the foundation of the production of the beautiful colouring substances which we all so much admire. Thus, in order to obtain aniline blues, violets, and greens, produced by the methods devised by the illustrious chemist, Dr. Hofmann, we substitute for a certain proportion of the hydrogen of rosaniline, an equivalent quantity of the alcoholic radicals, called phenyl, ethyl, methyl, and amyl. Further, this celebrated chemist has also shown that the blue obtained by Messrs. Girard and Delaire is also due to the same laws.

I am far, I regret to say, gentlemen, from having named all the remarkable properties and applications of carbolic or phenic alcohol; but I trust I have succeeded in making you share my enthusiasm for this valuable agent, which, after having rendered important services to most of the world's industries, still offers to chemists and to manufacturers a wide field for new applications.

CRYSTALLOGRAPHY AND THE BLOWPIPE.

BY W. A. ROSS, CAPTAIN, R.A.

I have the pleasure to inform you of a new feature in blowpipe manipulation lately discovered by me, which I am sanguine enough to hope may add another to the many results obtained in chemical and mineralogical analysis by this invaluable instrument.

It struck me that, as the decomposition of light by inflection or diffraction is analogous to that by absorption and transmission through a prism, and as the latter operation has been applied so successfully to the recognition of blowpipe flames of metals, as lithium, barium, etc., in the spectroscope, small *bladders* or *bubbles* of borax of sufficient tenuity to diffract rays of light, if containing certain metals or oxides in solution, would afford, when cool, corresponding chromatic rays in daylight. It is true that Sir I. Newton attributed the different colours obtained by diffraction solely to the relative thickness or distance between the reflecting surfaces, but, as Sir D. Brewster remarks, he "inferred that they were produced by a singular property of the particles of light, in virtue of which they possess, at different points of their paths, *fits* or *dispositions* to be reflected from, or transmitted by transparent bodies. Sir Isaac does not pretend to explain these *fits*, but terms them *fits of transmission* and *fits of reflection*." Now why should not these "fits" be caused by some peculiarities in the composition of the reflecting body? The colours on a soap bubble are surely not solely attributable to its thickness and thinness in different parts; and we must all recollect as boys how not only the quantity but quality of the soap s was *sine quâ non* of success.

The following table, which I have prepared from actual experiment in every case, will, I think, fairly show that a richer play of colour is yielded by metallic solutions than in the pure *vesicula* of borax, and it appeared to me very remarkable that such a highly colouring agent of borax as cobalt should yield nearly transparent *vesicula* showing little or no iridescence. I remarked also that a copper vesicle gave almost the same play of colours as is seen in the ore called "peacock copper" in which a rich blue is very predominant, while in several vesicles of bismuth which I made, no blue at all could be observed, but a great predominance of pale green.

Carbonate of baryta,	very slightly iridescent
Borax............	" " "
Microcosmic salt....	" " "
Oxide of copper....	" highly "
" cobalt.....	nearly clear vesicle
" nickel.....	slightly iridescent
" bismuth...	very highly "
" tin.......	highly "
" manganese	very highly "
Nitrate of potash...	highly "
Iodide of potassium.	very highly "

I made no attempt to classify the colours reflected by different metals in solution, but they always appeared in prismatic bands of blue, green, yellow, orange, red, etc.

Next day a new feature in these *vesiculæ* attracted my attention, which I hope will add to their importance as analytical agents, and develope besides an elegant application of the science of crystallography to blowpipe examination which it has not yet received.

Observing that a cloudy film containing white spots had gathered over the vesicle holding *carbonate of baryta* in solution, I examined the spots with a microscope, and found them to be round radiated crystals having a dark nucleus or centre.

I then made a borax vesicle containing *nitrate of baryta*, and allowed it to stand for several hours, after which a film displaying similar crystals, but with a *white* nucleus or centre, appeared. A vesicle containing carbonate of potassa showed, after a time, a film without crystals exactly like finely-ground glass, *Carbonate of magnesia* appeared in beautiful crystals, like the flowers of a convolvulus, having six petals, with a double rim or edge, and a dark nucleus. *Sulphate of magnesia* displayed similar flower-like crystals without the rim or nucleus.

I now found to my surprise and pleasure that every one of thirteen borax vesicles I had placed on cotton over night was covered more or less with similar crystals peculiar to itself, and strongly differing from those of the other vesicles, unless, as in the case of magnesia, they contained a common base. Thus *nitrate of silver* appeared in almost perfect stars, radiating from an extremely white central point, which again was surrounded by a dark aureola. *A silicate of lithia* (lithionglimmer) also appeared in stars, but of a perfectly different shape, having no nucleus and large cups of unequal length. *Oxide of cobalt*, the vesicle containing which was colourless when newly made, now appeared in a series or net-work of transparent crystals like the glass manufacture termed "imitation ice." *Strontianite* seemed to crystallise in octohedral planes having large dark nuclei, while *bichloride of platinum* produced nearly circular flowerets having an inner ring but no nucleus, and veined like the wing of a fly. The *teroxide of bismuth*, on the contrary, appeared in crystals like extremely white and small feathers.

Thus it will be seen that, whatever the cause, different solutions of metals or their oxides in borax, produce, when blown into *vesiculæ* and allowed to stand for a night, different and peculiar crystals; a fact which I have allowed myself to hope, will enable the careful observer to determine the composition of these substances more rapidly, certainly, and easily, than by any other method, as this one is effected by the operation of the laws of nature herself. All that is required seems to me to be a careful representation and classification of these crystals obtained by means of a powerful microscope, and as the vesicles, with the exception of the crystals upon them, are, in dry weather, generally transparent, these latter might almost, I should think, be magnified and represented by means of the polariscope.

I have made sketches of thirteen crystals observed by me, but although they convey perhaps a general idea of the shape, they are very far from showing, in the least degree, the beautiful detail and variety observable in the structure of each of these crystals. I observed also that these vesicles develop electricity, some in a greater degree than others, which may be referable to their different composition. Seeing that two of them adhered together so fast that they could not be separated without breaking one of them, although quite dry and smooth on the outside, I applied a piece of rubbed sealing-wax to several, and attracted most of them vigorously, but some, on the other hand, did not show the least attraction.

Some of the saliferous vesicles—notably the nitrate of potassa, and carbonate of soda—deliquesced so rapidly that although they crystallised, I failed to observe the shape of the crystals,* and indeed in damp weather, like the present, the general results of this system are bad—I cannot to-day obtain a second set of crystals of carbonate of soda, the vesicle containing it deliquescing into holes before the crystals can be even formed.

When the phenomena of these vesicles have been observed, they can be remelted into beads by holding the platinum wire sustaining them in the gas or spirit lamp flame, in which process the thin shell of the vesicle

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* I obtained and sketched crystals of these afterwards.—W. A. R.

effloresces in a manner apparently peculiar to the salt which it contains. Thus oxide of bismuth has a *frothy*, nitrate of silver a *creamy*, efflorescence. If a newly made vesicle be applied to the gas flame, no efflorescence at all takes place, but the borax melts transparently down to its bead.

It now only remains for me to explain how the process of vesiculation is carried out. I perform it as follows:—I fuse a bead of borax on the platinum wire in usual way, and charge it with the salt or oxide of which the crystals and diffractive colours are desired. After holding it for some time in the *reducing* flame, I withdraw both the bead and the blowpipe from the flame, keeping the latter still pressed against my mouth and blowing through it; I then bring the jet of the blowpipe rapidly opposite the ring of the platinum wire (which should be of the size of a pin's head and as round as possible), through which the stream of air issuing from it blows out a vesicle or bladder formed of the fluid bead of borax and the substances contained in it.

Woolwich, Dec. 2, 1867.

ON A NEW CLASS OF BODIES HOMOLOGOUS TO HYDROCYANIC ACID.*

BY A. W. HOFMANN, LL.D., F.R.S.

THE typical transformation which hydrocyanic acid undergoes when submitted, under appropriate circumstances, to the action of water, is capable of assuming two different forms when accomplished in its homologues.

If the hydrocyanic molecule be found to fix the elements of two molecules of water, yielding ultimately formic acid and ammonia, it is obvious that the atom group which in the homologues of hydrocyanic acid we assume in the place of hydrogen, may be eliminated when these homologues are decomposed by water in conjunction either with formic acid or with ammonia. To take an example:—When acting with water upon the simplest homologue of hydrocyanic acid (upon cyanide of methyl), we may expect to see the methyl-group separating either in the form of methyl-formic, *i.e.* acetic acid, or in the form of methyl-ammonia, *i.e.* of methyl-amine. The difference of the two reactions and their relation to the metamorphosis of hydrocyanic acid itself are exhibited by the following equations:—

$$\underbrace{CHN}_{\text{Prussic acid.}} + 2H_2O = \underbrace{CH_2O_2}_{\text{Formic acid.}} + H_3N.$$

$$1.\ \underbrace{C_2H_3N}_{\text{Cyanide of methyl }\alpha.} + 2H_2O = \underbrace{C_2H_4O_2}_{\text{Methylformic (acetic) acid.}} + H_3N.$$

$$2.\ \underbrace{C_2H_3N}_{\text{Cyanide of methyl }\beta} + 2H_2O = \underbrace{CH_2O_2}_{\text{Formic acid.}} + \underbrace{CH_5N.}_{\text{Methylamine.}}$$

The former one of these processes of transformation is familiar to chemists from the study of the hydrocyanic ethers or nitriles. The first member of this remarkable group of bodies (cyanide of ethyl) was discovered by Pelouze; the general character of their transformation was subsequently established by the beautiful investigations of Kolbe and Frankland on the one hand, and by those of Dumas, Malaguti, and Le Blanc on the other.

Researches in which I have been engaged during the last few weeks have proved that the second process of transformation does not less frequently occur. Indeed I have found that there correrponds to each of the hydrocyanic ethers or nitriles known hitherto a second body of precisely the same composition but of absolutely different properties. These substances, when changed by water, undergo the transformation which is exhibited by the last one of the three above equations.

A happy experiment has led me to the discovery of this new class of bodies. In a lecture, I wanted to exhibit the interesting transformation of ammonia into prussic acid by means of chloroform, which was first observed by M. Cloez, and which illustrates so well our present views on quantivalence. When the two substances alone are allowed to act upon one another, this reation can be rapidly accomplished only at a high temperature and consequently under pressure. In order to shorten the process (in one word, in order to exhibit this important reaction in a lecture-experiment), I had added potash to the mixture for the purpose of fixing the newly formed prussic acid, and was delighted to find that a few seconds' ebullition was sufficient to yield a considerable amount of cyanide of potassium, so as to furnish, after the addition of the two salts of iron, a large quantity of Prussian blue. On subsequently repeating the experiment with some of the derivatives of ammonia, more especially with several primary monamines, I was astonished to observe in each case a powerful reaction, giving rise to the evolution of vapours of an almost overwhelming odour, strongly recalling that of prussic acid. But few experiments were necessary for the purpose of isolating the odoriferous bodies. The compounds thus formed are the substances isomeric with the hydrocyanic ethers or nitriles hitherto examined.

From the host of bodies which were thus suddenly thrown into view, it was necessary to single out the compound of a particular series in order to determine by accurate experiments the nature of the new reaction. The facility of procuring the necessary material, as well as old predilections, suggested the phenyl-series as the one to be examined in the first place. I beg leave to submit to the Royal Society a brief account of the mode of preparation, and of the principal properties, of the new derivative of aniline.

Cyanide of Phenyl.

A mixture of aniline, chloroform, and alcoholic potash yields on distillation a liquid of a powerfully aromatic but, at the same time, hydrocyanic-acid-like odour. The vapour of the liquid gives rise to a peculiar bitter taste, and causes, moreover, in the throat the suffocating sensation so characteristic of hydrocyanic acid. On redistilling the liquid, alcohol and water pass first, and ultimately an oily body is procured, which, in addition to the smelling substance, still contains a large amount of aniline. The latter is separated by oxalic acid, when the powerfully smelling compound remains in the form of a brownish oil. Freed from water by hydrate of potassium, and purified by distillation, the new body presents itself as a mobile liquid, exhibiting a greenish colour in transmitted, and a beautifully blue colour, in reflected light. This colour does not disappear by distillation even in a current of hydrogen.

The analysis of the blue oil has established the formula

$$C_7H_5N.$$

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* Paper sent to the Royal Society during the recess.

The compound is thus seen to be isomeric with benzonitrile, discovered by Fehling, from which it differs, however, in all its properties. In order to distinguish the new compound from benzonitrile I will call it *cyanide of phenyl*, without intending, however, by selecting this name to express any particular view as to its constitution. The formation of cyanide of phenyl is represented by the following equation:—

$$\underbrace{C_6H_7N}_{\text{Aniline.}} + \underbrace{CHCl_3}_{\text{Chloroform.}} = \underbrace{C_7H_5N}_{\text{Cyanide of phenyl.}} + 3HCl.$$

Cyanide of phenyl cannot be volatilized without undergoing decomposition. During distillation the thermometer marks for some time the constant temperature of 167°, which may be taken as the boiling-point of cyanide of phenyl. Then the temperature rises rapidly to from 220° to 230°. The brown liquid which now distils is destitute of odour, and solidifies on cooling to a crystalline mass, easily purified by solution in alcohol, but not yet more minutely examined. Cyanide of phenyl is remarkable for the facility with which it combines with other cyanides. The compound with cyanides of silver is particularly beautiful. The behaviour of cyanide of phenyl with acids is more especially characteristic. Scarcely changed by the action of alkalies, it cannot be left in contact even with moderately dilute acids without undergoing alteration. When submitted to the action of concentrated acids, the liquid bursts into ebullition, and the solution, after cooling, contains only formic acid and aniline.

$$\underbrace{C_7H_5N}_{\text{Cyanide of phenyl.}} + 2H_2O = \underbrace{CH_2O_2}_{\text{Formic acid.}} + \underbrace{C_6H_7N}_{\text{Aniline.}}$$

Benzonitrile, isomeric with cyanide of phenyl, is known to be slowly attacked by acids, but to be rapidly transformed by alkalies into benzoic acid and ammonia.

$$\underbrace{C_7H_5N}_{\text{Benzonitrile}} + 2H_2O = \underbrace{C_7H_6O_2}_{\text{Benzoic acid.}} + H_3N.$$

The transformation of benzonitrile into benzoate of ammonium, as, indeed, the transformation of the nitriles into the ammonium-salts of the respective acids generally, is not accomplished in one single bound. By fixing only one molecule of water, benzonitrile is first converted into benzamide.

$$\underbrace{C_7H_5N}_{\text{Benzonitrile.}} + H_2O = \underbrace{C_7H_7NO}_{\text{Benzamide.}}$$

Nor is the corresponding term of the isomeric series wanting. This substance has long been known as phenyl-formamide or formanilide.

$$\underbrace{C_7H_5N}_{\text{Cyanide of phenyl.}} + H_2O = \underbrace{C_7H_7NO}_{\text{Phenyl-formamide.}}$$

But, in addition to phenyl-formamide, there figures in this series a second intermediate compound, the analogue of which among the derivatives of benzonitrile is not yet perfectly known.* This compound is the well-defined base which some time ago I described as methenyldiphenyldiamine, and which may be looked upon as formed by the association of a molecule of cyanide of phenyl with a molecule of aniline. The successive changes which cyanide of phenyl undergoes when submitted to the influence of water are thus exhibited by the following series of equations:—

$$\underbrace{C_{14}H_{10}N_2}_{\text{2 mols. cyanide of phenyl.}} + 2H_2O = \underbrace{CH_2O_2}_{\text{Formic acid.}} + \underbrace{C_{13}H_{12}N_2}_{\text{Methenyldiphenyldiamine.}}$$

$$\underbrace{C_{13}H_{12}N_2}_{\text{Methenyldiphenyldiamine.}} + H_2O = \underbrace{C_7H_7NO}_{\text{Phenyl-formamide.}} + \underbrace{C_6H_7N}_{\text{Aniline.}}$$

$$\underbrace{C_7H_7NO}_{\text{Phenyl-formamide.}} + H_2O = \underbrace{CH_2O_2}_{\text{Formic acid.}} + \underbrace{C_6H_7N}_{\text{Aniline.}}$$

A glance at these formulæ shows that the metamorphosis of phenylic cyanide is perfectly analogous to that of phenylic cyanate, which I have studied at an earlier date.

$$\underbrace{C_{14}H_{10}N_2O_2}_{\text{2 mols. cyanate of phenyl.}} + 2H_2O = \underbrace{CH_2O_3}_{\text{Carbonic hydrate.}} + \underbrace{C_{13}H_{12}N_2O}_{\text{Diphenyl-urea.}}$$

$$\underbrace{C_{13}H_{12}N_2O}_{\text{Diphenyl-urea.}} + H_2O = \underbrace{C_7H_7NO_2}_{\text{Anhydrous carbonate of aniline.}} + \underbrace{C_6H_7N}_{\text{Aniline.}}$$

$$\underbrace{C_7H_7NO_2}_{\text{Anhydrous carbonate of aniline.}} + H_2O = \underbrace{CH_2O_3}_{\text{Carbonic hydrate.}} + \underbrace{C_6H_7N}_{\text{Aniline.}}$$

Cyanide of Ethyl.

After having fixed in the phenylic group the general characters of the reaction, my attention was very naturally directed to the ethylic series. For this purpose, it was first necessary to procure ethylamine in rather considerable quantities. Happily in this case the liberal co-operation, so often experienced, of my friend Mr. E. C. Nicholson, was again at hand. Interesting himself with a cordiality, for which I cannot sufficiently thank him, in the continuation of my researches on the ethylic bases, Mr. Nicholson had placed at my disposal the product of the action of ammonia on iodide of ethyl produced in a single operation performed in one of his great autoclaves on 20 kilogs. of iodide of ethyl.

Thanks to the happy alliance between science and industry, which characterises our times, I was thus enabled to study the transformation of ethylamine under the influence of chloroform on a rather large scale.

On gradually introducing a mixture of an alcoholic solution of ethylamine and chloroform into a retort containing powdered potassic hydrate, a most powerful reaction takes place; the mixture enters into ebullition, and a liquid distils over, the penetrating odour of which surpasses anything that it is possible to conceive. Besides the odoriferous body, the product of the distillation contains ethylamine, chloroform, alcohol, and water, and a considerable number of rectifications are required in order to isolate the cyanide of ethyl from this mixture.

As the substance is rather volatile, the frequently repeated fractional distillations become a most painful operation, and more than once, while I have been engaged in these experiments, my laboratory has been

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* Shortly before his death, Gerhardt was engaged in experiments on the action of pentachloride of phosphorus on the amides, a brief account of which was subsequently published by M. Cahours. Among other substances, I find that, by acting with pendachloride of phosphorus upon benzanilide, Gerhardt obtained a chloride, $C_{13}H_{10}NCl$, which yields with ammonia a crystalline substance. It can scarcely be doubted that this compound is the derivative of benzonitrile corresponding to methenyldiphenyldiamine,

$$C_{13}H_{10}NCl+H_3N=C_{13}H_{12}N_2+HCl.$$

almost inaccessible. Thus with a temperature of 30° I have found it desirable to interrupt for the time the preparation in the pure state of the cyanide of ethyl, and to resume it at a more favourable season.

I was nevertheless curious to study, even now, a true homologue of cyanide of ethyl, in order to compare its properties with those of cyanide of phenyl. The boiling-points of the amylic compounds being within convenient limits, I was induced to select the amyl-series as presenting the greatest chance of success.

On submitting amylamine to the action of chloroform, the same phenomena are observed as in the analogous reaction between chloroform and aniline. One molecule of amylamine and one molecule of chloroform contain the elements of one molecule of cyanide of amyl and three of hydrochloric acid:—

$$\underbrace{C_5H_{13}N}_{\text{Amylamine.}} + \underbrace{CHCl_3}_{\text{Chloroform.}} = \underbrace{C_6H_{11}N}_{\text{Cyanide of amyl.}} + 3HCl$$

The cyanide of amyl is a transparent colourless liquid lighter than water, insoluble in water, but dissolved by alcohol and ether, of an oppressive odour, resembling at the same time that of amylic alcohol and of hydrocyanic acid. Its vapour possesses, in a still higher degree than that of the cyanide of phenyl, the property of producing on the tongue an insupportably bitter taste, and of giving rise in the throat to the sensation of suffocation, so characteristic of hydrocyanic acid.

The cyanide of amyl may be distilled without decomposition. It boils at 137° C., that is, at a temperature 8° lower than the boiling-point of its isomer, capronitrile. It will be remembered that the boiling-point of cyanide of phenyl is lower than that of benzonitrile.

Under the influence of alkalies and acids, the cyanide of amyl behaves in the same manner as the phenylic cyanide. Though only slightly attacked by alkalies, it is decomposed by acids with a violence which is almost explosive; a short ebullition with water is sufficient to transform it into formic acid and amylamine:—

$$\underbrace{C_6H_{11}N}_{\text{Cyanide of Amyl.}} + 2H_2O = \underbrace{CH_2O_2}_{\text{Formic Acid.}} + \underbrace{C_5H_{13}N.}_{\text{Amylamine.}}$$

In order to fix this equation by numbers, I have carried out the reaction by means of dilute sulphuric acid. The formic acid was then distilled off and transformed into a sodium-salt, and analysed as formate of silver; the residue in the retort furnished, on addition of an alkali, amylamine in considerable quantities. It was identified with that obtained from cyanate of amyl, both by the determination of its boiling-point and by the analysis of the platinum-salt.

The transformation of the cyanide of amyl, like that of the cyanide of phenyl, does not take place at a single step; intermediate combinations corresponding to methenyldiphenyldiamine and to phenylformamide are produced, but I have not yet obtained them in a state of purity.

I have designated the body described in this note by the name of *cyanide of amyl;* I am of course aware that the same name has been given to the substance produced by the action of cyanide of potassium on the sulphamilates; but as the latter compound, in consequence of its transformation into caproic acid and ammonia, has a right to the name *capronitrile*, I have thought it desirable to distinguish, provisionally at least, the new product by the name of cyanide of amyl.

The examination of the cyanides of amyl and phenyl establishes in a positive manner the existence of a group of bodies isomeric with the nitriles derived from the ordinary alcohols and phenols.

I have not as yet pursued more minutely the study of the other terms of these groups; in fact the field opened by these new observations presents questions much more attractive. The existence of the new homologues of hydrocyanic acid allow us to foresee the formation of quite another series of homologues of cyanogen. These bodies will be produced by the action of chloroform on the diamines. Ethylene-diamine, for example, will thus be transformed into the dicyanide of ethylene:—

$$\underbrace{C_2H_8N_2}_{\text{Ethylene-diamine.}} + \underbrace{2CHCl_3}_{\text{Chloroform.}} = \underbrace{C_4H_4N_2}_{\text{Dicyanide of ethylene.}} + 6HCl$$

The new cyanides isomeric with the nitriles are not formed exclusively by the action of chloroform on the primary monamines. On perusing the papers describing the examination of the organic cyanides, we see at a glance that the chemists who investigated them have had in their hands at the same time the isomeric cyanides with which I am engaged.

In fact every one who has distilled mixtures of sulphomethylate, sulphethylate, or sulphamylate of potassium with the cyanide of the same metal, will remember the repulsive odour possessed by the products so obtained. This odour only disappears in proportion as the product is purified, and especially after its treatment with acid, in order to remove the ammonia, and with oxide of mercury to separate the hydrocyanic acid.

Dumas, Malaguti, and Le Blanc, in their researches on the nitriles, mention the insupportable odours possessed by the cyanides obtained by the cyanide-of-potassium process; while the products obtained by the dehydration of the ammoniacal salts by means of phosphoric anhydride have a very agreeable aromatic odour.

In a research made by Mr. Buckton and myself on the transformations of the amides and nitriles under the influence of sulphuric acid, we repeatedly had occasion to prepare acetonitrile (cyanite of methyl) and propionitrile (cyanide of ethyl) by the distillation of a sulphomethylate or sulphethylate with cyanide of potassium. In our paper we mention substances of a formidable odour which appeared in these reactions, and we describe the efforts we made in order to isolate them. But as they are only formed in small quantity, we had to give up the attempt.

Mr. E. Meyer,* who has also been occupied with cyanide of ethyl, but who employed another method of preparation, encountered the same bodies. By acting on cyanide of silver with iodide of ethyl in sealed tubes, he obtained, together with iodide of silver, an unstable compound of cyanide of silver and cyanide of ethyl; and there was formed in the same reaction a liquid of an overwhelming odour. This latter on distillation, presented the characters of a mixture from which it was impossible to isolate a product with a constant boiling-point. When treated with an acid the odour disappeared, and the solution contained ethylamine which was identified by the analysis of the platinum-salt. These are certainly the characters of the cyanides formed by the action of chloroform on the primary monamines; and it cannot be doubted that Mr.

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* Journal für praktische Chemie, vol. lxvii. p. 147.

Meyer has had in his hands the ethyl term of the series of cyanides which I am studying both in the combination with cyanide of silver and in the complex liquid which accompanied it.

If such results did not particularly attract the attention of chemists, it was owing to the fact that the author failed in ascertaining the complementary product of ethylamine, namely, formic acid. Mr. Meyer, besides, states that his research remained unfinished; and thus it will be understood how experiments otherwise so carefully carried out should have fallen into an oblivion from which neither the author nor any other chemist has endeavoured to recall them during the many years which have elapsed since their publication.

In consequence of the examination of the bodies produced by the action of chloroform on the primary monamines, these old experiments have acquired a new interest; and it appeared to me, for more than one reason, that it would be desirable to repeat them, making use of the experience gained by my late researches.

For this purpose I have submitted cyanide of silver to the action of several organic iodides.

The iodides of methyl and ethyl act very slowly on cyanide of silver at the ordinary temperature; but the reaction takes place at the temperature of boiling-water.

After a digestion of about ten hours, the transformation is complete; a brown solid matter is formed, having the appearance of paracyanogen, together with a yellowish oily layer possessing in a marked manner the odour of the isomers of the nitriles.

As several preliminary experiments gave indications of a rather complicated reaction, and as it would have been difficult for me readily to obtain sufficient substance by operating in sealed tubes, I performed the experiment in the amylic series, supposing that the higher boiling-point of the iodide of amyl would render it more easy of attack. My expectation was indeed fulfilled; two molecules of cyanide of silver and one molecule of iodide of amyl act on one another with extreme violence at the boiling-point of the latter. It is convenient to operate on a moderate scale, so as to be carefully protected from the escaping gases, which consist of equal volumes of amylene and hydrocyanic acid, mixed with a small quantity of the cyanide of amyl.

The experiment was made in a retort adapted to the lower end of a condenser, the upper end of which was connected with a series of washing-bottles. In the first a small quantity of cyanide of amyl was condensed; the second contained water intended to absorb the hydrocyanic acid; the third one water and bromine in order to transform the amylene into bromide, of which I was thus enabled to collect a considerable quantity during my researches.

After an hour's digestion, the reaction is finished, and the residue in the retort consists of a dark viscous mass, becoming almost solid on cooling; this is a mixture of iodide of silver and a combination of cyanide of silver and cyanide of amyl. The reaction then takes place according to the equation:—

$$\underbrace{C_5H_{11}I}_{\text{Iodide of amyl.}} + \underbrace{2AgCN}_{\text{Cyanide of silver.}} = AgI + \underbrace{AgCN, C_5H_{11}CN.}_{\substack{\text{Compound of cyanide of}\\ \text{silver and cyanide of amyl.}}}$$

But simultaneously a certain quantity of cyanide of amyl splits into amylene and hydrocyanic acid:—

$$\underbrace{C_5H_{11}CN}_{\text{Cyanide of amyl.}} = \underbrace{C_5H_{10}}_{\text{Amylene.}} + \underbrace{CHN.}_{\substack{\text{Hydrocyanic}\\ \text{acid.}}}$$

This secondary transformation depends principally on the manner in which the operation is conducted; it may give rise to very great loss if the action be rather tumultuous.

It was now necessary to separate the cyanide of amyl from the residue in the retort. Up to the present time I have found no other means of effecting this than by submitting the residue to dry distillation; in this operation a further quantity of hydrocyanic acid and amylene is disengaged, and a liquid distils over, which on rectification boils between 50° and 200°. By submitting it to fractional distillation it was found that the first part still contained a quantity of amylene, whilst the latter products had become inodorous. The intermediate portion, rectified several times, finally exhibited a constant boiling-point between 135° and 137°.

The liquid which distils at this temperature is perfectly pure cyanide of amyl. It possesses all the properties which I have described in my previous communication, and is characterised especially by its odour and by the facility with which under the influence of hydrochloric acid, it splits into formic acid and amylamine. I have not yet completely examined the products boiling at a higher temperature, but everything seems to show that they consist, partly, at least of caponitrile.

The experiments which I have just described show, in a positive manner, that the same bodies can be obtained by the action of chloroform on the primary monamines, and by the treatment of cyanide of silver with the alcoholic iodides. In the latter process many secondary products are obtained; but by a more complete study perhaps it may be modified so as to diminish their quantity.

However this may be, the study of the action of the alcoholic iodides upon silver-salts deserves to be resumed; and it is very probable that in many cases it will be found that the bodies so produced will be but isomeric with those obtained by the ordinary processes.

For the special researches in which I am engaged at the present time, the observations just described have a particular interest; they permit us, in fact, to produce the isomeric cyanides without first preparing the primary monamines; they are especially important with reference to the generation of the polycyanides. The polyamines, in fact, are little, if at all, known up to the present, whilst the iodides of methylene and ethylene and iodoform are easy to procure.

If I have not yet succeeded in preparing a dicyanide of ethylene $C_4H_4N_2$, isomeric with Mr. Maxwell Simpson's cyanide, it is because I have not had at my disposal a sufficient quantity of ethylene-diamine. I now hope to obtain this body by submitting cyanide of silver to the action of iodide of ethylene.

In conclusion, I may be permitted to announce as very probable the existence of a series of bodies isomeric with the sulphocyanides. Already Mr. Cloez has shown that the action of chloride of cyanogen on ethylate of potassium gives rise to the formation of an ethylic cyanate possessing properties absolutely different from those belonging to the cyanate discovered by Mr. Wurtz. On comparing, on the other hand, the properties of the methylic and ethylic sulphocyanides with those of the sulphocyanides of allyl and phenyl, we can scarcely doubt that we have here the representatives of two groups entirely different, and that the terms of the methylic and ethylic series, which correspond to oil of mustard and to the sulphocyanide of phenyl, still remain to be discovered. Experiments with which I am now engaged will show whether these bodies can be obtained by the action of the iodides of methyl and ethyl on sulphocyanide of silver.

I must not conclude this note without expressing my thanks to Messrs. Sell and Pinner for the hearty co-operation that they are giving me in these researches.

FOREIGN SCIENCE.

(FROM OUR OWN CORRESPONDENT.)

PARIS, Dec. 4, 1867.

Chalices and Petera of Aluminium Bronze—Alloy of Silver and Aluminium, "tiers-argent"—Induction Coil Experiments—Reaction of Iodine, Mercury, and Zinc—Eruption of Vesuvius.

THE Bishop of Dreux Brize formerly applied for chalices of aluminium, either pure or mixed with other metals, to be used on account of their lightness, beauty, and solidity. This demand was not responded to favourably at first, but M. Paul Morin, after many attempts, succeeded. Chalices and patera in aluminium bronze, forbidden by a decree of 5th August, 1865, have been accepted on the condition that the cups of the chalices and the patera be first silvered and then gilt in the portions prescribed by the rubric. We do not doubt that this favour will be soon extended to chalices and other objects in aluminium, alloyed with a third of silver, which gains ground every day, and will become very popular in spite of the obscurity in which the jury of the Exposition have left it. It is, as its name indicates, "tiers-argent," an alloy of one-third silver with two-thirds of aluminium, that has been rendered homogeneous, at first with much difficulty, but now of easy fabrication. The selling price is 90 francs the kilogramme, and the old metal is re-purchased at 75 francs. The spoons, forks, and salvers of this alloy leave nothing to be desired; it possesses a hardness superior to that of silver, and it can be more easily engraved. If we are well informed, the idea of the "tiers-argent" belongs to M. Alfred Taloureau, the inventor, along with his brother of bituminised tubes celebrity. M. De Ruolz, associated at a later period with this new industry, remains the possessor of it, and carried on the business with M. Moustet, successor of Lebrun, 116, Rue de Rivoli.

M. Rondel, of Brive, communicates to us the following:—If, while the current of a pile passes through the primary wire of a coil, one of the extremities of the secondary wire is brought near one of the extremities of the iron core, sparks can be drawn of remarkable intensity and brilliancy; if, at the same time, the other end of the secondary wire is put in communication with one of the poles of the pile, a great increase takes place in the brilliancy of the spark. Then, on touching with the hand the iron core, and placing the free end of the wire in contact with the skin, a redness takes place, and a smart stinging sensation is felt. This last experiment was made upon a coil the core of which, completely isolated in a tube of varnished glass, was eight millimètres in diameter.

M. Rondel made the same experiment with another bobbin, the soft iron of which was 12 centimètres long, 5 centimètres wide, and 8 millimètres thick. The sparks were produced with detonations. A single Bunsen element of small size was sufficient to produce these phenomena.

When two recipients are charged with mercury and water, and fragments of iodine are added, we do not perceive any effect. But if a small piece of zinc is allowed to fall into the mercury, the fragments of iodine are instantly set in motion, and are rapidly dissolved. This solution poured off clear serves for many uses. M. Rondel has employed it concentrated for the supplying of a pile mounted in a closed flask, also for the preparation of a fine red iodide of mercury.

Signor Giordano writes to us from the Naples University, with respect to the present eruption of Mount Vesuvius:—

"There is a new conflagration of Vesuvius; I have made several excavations there, and I have just returned at this moment; and faithful to my old habits I hasten to describe what has passed from the first instant of the irruption to the present time.

"After the irruption of last year the volcano remained in the most perfect tranquillity. But on the 13th of this month, at 1 o'clock in the morning, without any previous warning, and only with a slight noise, the mountain commenced to project, at first incandescent stores, and afterwards liquid matter by four igneous openings, or craters properly so-called. Neither I nor any other person could positively affirm whether these craters were opened simultaneously or one after the other, and in what order, as no person was on the volcano at that hour, and the first visitors only ascended at an early hour on the following day. The first of these craters is placed at the east of the two cones of last year; the second at half the height of the great cone to the S.E. on the side of the village of Boscoviale; the two other smaller ones on the current of the lava of last year. Only the second of these craters rejected melted matter, that is to say, a current of lava which by degrees spread and filled up the cavities of the plateau at the summit of the mountain.

"If we judge by the effects produced, the commencement of the irruption from the first instant must have been very strong, although neither the sound nor the earthquake were prolonged to a great distance, because the whole of the surface of the great cone presented long crevasses in different directions. Thus, it was feared that the irruption would be neither feeble nor of short duration. And, in reality, in three days all the crater was filled with lava up to the night of the 16th and 17th, when it commenced to flow over in the currents on the external side of the cone towards the north and north-west; thus were three currents flowing at the same time, uniting together at 20 or 30 meters distance from the edge. These currents encumbered the passages most convenient for ascending and descending the mountain, and the visitors were of course more obstructed than ever.

"At present the stream of lava does not advance any further, but the four above-mentioned cones, and a fifth, which has just appeared, are in violent eruption. The central cone has gained much in height, so as to attain the ten meters which separate its base from the edge of the great crater, so that it can be plainly seen from Naples, towering above the summit of the mountain. The substance of the lava is always the same, that is to say, *augitophyre*, and all the ground in the environs of the small cones is covered with the ordinary chlorides of different colours."

F. MOIGNO.

PARIS, Dec. 18, 1867.

Demonstration of Fact that Electricity will not pass through a Vacuum—Electrolysis of Salts—Colliery Explosion—Construction of Roofs—New Paint for Houses.

MM. ALVERGNIAT, FRERES, have contrived a new apparatus for demonstrating the fact that the electric spark does not pass through a perfect vacuum. They create a nearly absolute vacuum by means of a mercurial pneumatic machine in the tube which serves for the experiment; this contains two platinum wires, placed at a distance of two millimètres one from the other. Half an hour is sufficient to obtain the necessary degree. At this moment they heat the tube to dull redness, either over charcoal or, more conveniently, by the special lamp employed by M. Berthelot for organic analyses; when the tube arrives at a dull red, the vacuum is continued to be made, and the electric spark is passed until it ceases to pass through the interior of the tube. It is then hermetically sealed, and the tube is separated from the machine.

In a tube thus prepared, in spite of the slight distance between the two platina points (two millimètres) electricity absolutely ceases to pass.

The above gentlemen now have apparatus at the disposition of professors who would wish to show that electricity does not pass through a perfect vacuum.

Mr. Bourgoin has published a memoir on the electrolysis of organic acids and their salts. He has found by experiment that the action of the electric fluid is nothing in reality than a fundamental action on all acids and salts, whether mineral or organic; it separates the basic element, which goes to the negative pole, while the elements of anhydrous acid and oxygen, which answer to basic hydrogen or to metal, fly to the positive pole. Such is the fundamental action of the electric current.

The first volume of the Abbé Moigno's "*Lectures on Analytic Mechanics*" (statical part) has just been published. They are based upon the method of Augustin Cauchy, and are extended to the most recent operations of modern times. This work is in 8vo, consisting of 767 pages, and two well-executed copper engravings. Three other volumes or parts are to follow, viz.:—Dynamics, Industrial Statics, and Industrial Mechanics.

MM. Sainte Claire-Deville and Pasteur have been named Professors of Chemistry in the Faculty of Sciences of Paris, in the place of MM. Dumas and Balard.

.M Sappey has been appointed Professor of Anatomy of the Faculty of Medicine of Paris, in the room of M. Tarpevay. M. Volneuil has been named Professor of Surgical Pathology at the same Faculty, in the place of M. Ricbet. M. Moris is appointed Professor of Anatomy at the Faculty of Medicine of Strasburg.

We have the sad task of recording a fearful colliery explosion in France. On the 12th instant, at 11 a.m., a fire-damp explosion took place, accompanied with the partial falling in of the pit at No. 5 shaft in the Blanzy mine (Soane et Loire). Eighty bodies have been recovered from the ruins. These melancholy tidings afflict us the more, as M. Chagot, the superintendent, and his co-operators were continually searching for the best means of avoiding these dreadful accidents. The first steps taken by them were the purchase of the excellent apparatus of M. Ansell, which, unfortunately, was not placed throughout the pit. On this occasion, we seriously enjoin M. Guen, of Valenciennes, to carry out as soon as possible the benevolent project of M. Dubrunfaut, who has generously offered a reward of £4,000 and a series of graduated premiums, to be awarded to the inventor of a system which will resolve in the most complete manner the problem of security against fire-damp, or to those who give a partial solution of this vital question. To retard any longer this necessary organisation, and, on the part of coal-mining companies, to defer any longer to answer to the appeal of M. Dubrunfaut, who put down his name for £4,000, would be a great mistake, if not a crime. Our friend, M. Ansell, whose apparatus is constructed in France by M. Salleron, 24, Rue Pavée, Paris, is prepared to make all the experiments and installations of his instrument that may be required, on demand.

The materials with which roofs are generally covered should be paid special attention to by insurance companies. The 29th of last November, at 1 p.m., a violent fire broke out in a hay and corn store, No. 25, Rue du Rocher; in spite of all endeavours, the forage and the premises were rapidly destroyed. The building was roofed with *tiles*. The next day another fire broke out on the Quai St. Cloud, in a house roofed with slates. It commenced in the staircase, and so rapid was the progress of the flames that an English family, inhabiting the house, was obliged to jump out of window. It seems that the principal cause of the rapidity of the spreading of the fire in these cases was principally due to the formation of currents of air through the joints of the roof. Experiments were made by M. Maillard, No. 28, Rue Jean Gougon, in the quarter of the Champs Elysées, which proved that his new substance, mineral carton, was much safer than either tiles or slates or zinc in case of fire. Models on a large scale having been erected in the Avenue Montaigne, they were filled with combustible matter which was set on fire. The zinc roof fell in 7 minutes, the tile roof in 17 minutes, while that covered with mineral carton, after 40 minutes' exposure to flame, was sufficiently strong to bear the weight of a man; the reason of this is that the mineral carton prevents any possible ascensional current of air, without which timber will not burn, the effect being only the slow charring of the beams.

A new preparation of house paint has been invented by M. Hugoulin, principal chemist to the Imperial Navy, by which one can prepare, in a few hours, as much paint as is required, without any other utensils than simple small tubs of metal or wood of sufficient capacity. The best paints usually employed in house painting, and which preserve best the wood, have for bases white lead, minium, oxides of zinc, and lamp black. They are not, as in other paints, simple mixtures of drying oil and mineral substances in powder, but intimately blended compounds, in which the elements are combined without chemical double decomposition.

To demonstrate this, the inventor forms, in any vessel, a liquid paste, perfectly homogeneous, with water and a certain quantity in powder of the substances indicated in the following table:—

For 1000 grammes of zinc white we can employ. .	300 or 350 or even 400 grammes.
For 1000 grm. of grey oxide of zinc	150 to 180 "
" white lead	150 to 180 "
" red lead	50 to 60 "
" lamp black	1000 (thereabouts).

In this mixture the necessary quantity of linseed oil is added, in order to form a body colour worked up by the ordinary means.

F. Moigno.

REPORTS OF SOCIETIES.

CHEMICAL SOCIETY.

December 5th, 1867.

Dr. Warren de la Rue, F.R.S., *President, in the Chair*.

The minutes of the previous meeting were read and confirmed, the donations to the library announced, and the notice convening a special or general meeting of members for the consideration of a proposed alteration of the first by-law was again read. The President explained the nature of the amendments which it was contemplated to make, and, after taking the vote by show of hands, declared them to have been unanimously agreed to. By a second resolution it was decided that they should take effect from the first meeting in the new year. The by-law as amended stands thus:

"Every candidate for admission into the Society shall be proposed according to a form of recommendation (No. I., Appendix) subscribed by five Fellows of the Society, to three, at least, of whom he should be personally known, and such certificate shall be read and suspended in the Society's rooms, or place of meeting, for three ordinary meetings."

A verbal modification in the heading of the form of recommendation was likewise acceded to.

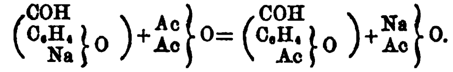
The Secretary then read for the first time the names of the following candidates for admission into the Society, viz., Captain Alexander Walker, Royal Artillery, Bengal Presidency; Gilbert W. Child, M.D., St. Giles. Oxford; Mr. Edward Chapman, Lecturer in Natural Science at Merton College, Oxford (Frewen Hall, Oxford); M. G.

Mason, Eserick, near York; and Peter Greiss, Burton-on-Trent.

For the second time were read the names of Alfred E. Fletcher, Inspector of Alkali Works, Johnston, near Prescot; and William Frank Smith, M.D., Lond., Lecturer at the Sheffield School of Medicine, Glossop Road, Sheffield.

The names of the undermentioned candidates were read for the third time, and from the results of the ballot all were declared to have been duly elected, viz., Thomas Hall, B.A., Lond., Lecturer on Chemistry and Natural Philosophy at the City of London School; Charles Walter Maybury, Teacher of Chemistry, 90, King Street, Manchester; George Lunge, Ph.D. (Breslau), 10, Albert Terrace, South Shields; Facundo J. R. Carulla, Chemist to the Atlas Steel and Iron Works, 59, Gell Street, Sheffield; Charles Meymott Tidy, M.B., The Hollies, Cambridge Heath, Hackney; Augustus A. Wood, Cheapside; Alfred Coleman, Plough Court, Lombard Street; Walter W. Fiddes, Sothernhay, Clifton; Robert R. Tatlock, Kyles of Bute; Phipson Beale Barrister-at-law, Stone Buildings; and Alexander Crum Brown, M.D., D.Sc., 4, Rillbank Terrace, Edinburgh.

Mr. W. H. Perkin read a paper "*On the Artificial Production of Coumarine, and Formation of its Homologues.*" Referring to the hydride of aceto-salityl as a body isomeric with coumaric acid, and to his failure in obtaining this body by the method of Cahours, the author attempted its preparation by the action of acetic anhydride upon the hydride of sodium-salicyl, thus,



$$\left(\left.\begin{array}{l}\text{COH}\\ C_6H_4\\ \quad\text{Na}\end{array}\right\}O\right)+\left.\begin{array}{c}\text{Ac}\\ \text{Ac}\end{array}\right\}O=\left(\left.\begin{array}{l}\text{COH}\\ C_6H_4\\ \quad\text{Ac}\end{array}\right\}O\right)+\left.\begin{array}{c}\text{Na}\\ \text{Ac}\end{array}\right\}O.$$

Instead, however, of getting at once the expected product, a substance was obtained which differed from it by containing $C_9H_6O_2$, *i.e.*, deficient by the elements of one atom of water. This formula coincides with that of coumarine, with which, indeed, it proves to be absolutely identical. Its odour resembles that of the Tonquin bean, and the fusion and boiling-points are the same. The author has exactly determined the physical characters of coumarine, both from natural and artificial sources, and calls in question the accuracy of some of the recorded observations.

By employing the anhydrides of other acid-radicals, Mr. Perkin obtains a series of homologous coumarines, which are fully described in the paper; particularly, he has formed a crystalline butyric coumarine, $C_{11}H_{10}O_2$, fusing at 72—73° C., and submitted it to the action of alkalies, bromine, etc. The valeric coumarine was likewise prepared, and an ingenious method of purification resorted to for the purpose of procuring a sample fit for analysis. Its formula is $C_{12}H_{12}O_2$, and fusion point 56°C.

The author concludes his paper with a survey of the probable constitution of this class of bodies, and conceives that the reaction quoted above actually expresses the *first stage* of the interchange involved in the production of the coumarines, since by very careful working he procured in one instance some of the hydride of acetosalicyl which he found to possess the properties both of an aldehyde and acetate; but he promises to give shortly a fuller account of this substance.

In proposing a vote of thanks to Mr. Perkin, reference was made, both by the President and by Dr. A. W. Hofmann, to the original research conducted in the laboratory of the Royal Chemistry by Dr. Bleibtreu, who, in 1846, established the identity of coumarine of the Tonquin bean with the aromatic principle contained in the German "Maiwein," which is prepared from the little forest plant known as woodruff (asperula odorata).

Professor A. H. **Church** then made a statement respecting the nature of the red (or crimson) colouring matter which is commonly found on the primary and secondary pinion feathers of the wings of Cape Lory (*Turacus albo-cristatus*). The author's attention was called to this matter by the editor of the *Field* newspaper, and he had since had opportunities of studying the habits and experimenting upon the feathers of some little birds reared in this country by a celebrated ornithologist. They were popularly supposed to be stained with blood, since it has been noticed that some of the colour is washed out by the rain. The colouring matter is, however, only very slightly soluble in pure water; but if a trace of alkali be added, it then freely dissolves out, forming a magnificent crimson solution. Hydrochloric or other mineral acid added to this liquid at once precipitates the red substance. [Experiment shown.] The remarkable feature of the whole case was the fact that analysis proved the existence of copper, apparently in some organic form of combination. [This important observation was exhibited at the meeting by showing with the aid of a platinum wire the bright green tinge imparted to a gas jet when a small quantity of the substance, moistened with hydrochloric acid, was held in the flame of a Bunsen burner.] The web of the feather and parts not coloured did not contain a trace of copper, and only sixteen feathers in all showed this peculiar phenomenon. The total amount of colouring matter was therefore very small, only 1·6 grains being procurable from each bird at the cost of half a guinea. The author's experiments were, for the present, interrupted for want of material; but he was satisfied that the organic body in combination with the copper contained nitrogen, with carbon, of course, and, he believed, sulphur. The substance may be dissolved in concentrated sulphuric acid, and reprecipitated unaltered upon the addition of water. None of the copper is thus separated; but if nitric acid be used the organic elements are destroyed, and then copper may be detected even in the diluted solution. [A bird and several plumes were exhibited, besides a small quantity of the colouring matter in a separate form.

The **President** remarked upon the singular character and mode of occurrence of Mr. Church's new substance. For his own part he was not aware of the existence of any crimson dye containing copper.

In answer to Dr. Gladstone, the author stated that he had made an optical examination of the substance, and compared its absorption spectrum with that of arterial blood. There was a general similarity so far as regards the fact of their both showing two bands: but in the present instance they were nearer to the yellow end of the spectrum, and the darker band almost coincided with the fixed line D. Professor Church further stated that he failed in detecting copper in the red plumage of humming birds.

A "*Note on the Preparation of Urea,*" by Mr. **John Williams**, was next read. The author proceeds, in the first instance, to prepare cyanate of potassium by fusion of the cyanide (best commercial quality, containing 90 per cent.) with red oxide of lead, keeping the temperature as low as possible. This product is dissolved in cold water, mixed with nitrate of barium to precipitate the carbonate which it usually contains, then thrown down as lead-salt by adding a solution of the nitrate. The cyanate of lead is easily purified by washing, and is then dried at a gentle heat. For the preparation of artificial urea equivalent amounts of sulphate of ammonia and cyanate of lead are digested together in warm water, the insoluble sulphate filtered off, and the solution when evaporated yields a product of unusually good quality, and of larger amount than by the ordinary plan. Mr. Williams finds that the process is applicable to the preparation of the compound ureas, using the corresponding sulphate instead of the simple ammonia salt.

Dr. A. W. **Hofmann**, who was welcomed in a most enthusiastic manner, then gave an account of his recent discovery of the "methylic aldehyde," and, with Mr. M'Leod's assistance, performed an experiment by means of which the method of production of this interesting compound was conclusively demonstrated. The main facts of Dr. Hofmann's research have been already communicated to our readers in an article specially devoted to their consideration last week, and it is now only necessary to report the latest statements of the eminent author regarding its constitution.

Dr. **Hofmann** remarked that a "more minute examination of methylic aldehyde and its derivatives remains still to be made. It will be absolutely necessary to isolate the oxygen term, and to determine its vapour density, in order to ascertain its molecular weight. If we remember the facility with which the aldehydes are polymerised, the question presents itself whether the aldehyde formed by the slow combustion of methylic alcohol is represented by the formula—

$$CH_2O,$$

or a multiple thereof. A similar remark applies to the sulphur derivative. It deserves, moreover, to be mentioned that a compound isomeric with methylic aldehyde, the dioxymethylene ($C_2H_4O_2$) of Boutlerow, is known already; also that a sulphur compound of the formula—

$$CH_2S$$

has been obtained by M. Aimé Girard, who observed that bisulphide of carbon is reduced by the action of nascent hydrogen with disengagement of sulphuretted hydrogen."

Dr. **Hofmann** then proceeded to describe the leading features of another research, which will be printed *in extenso* in our columns, and which has resulted in the production of "*A New Series of Bodies Homologous to Hydrocyanic Acid.*" The action of alcoholic ammonia upon chloroform, in the presence of a fixed alkali, was shown experimentally to give rise to the formation of a cyanide (afterwards indicated by the Prussian blue test); and in like manner a mixture of aniline, chloroform, and alcoholic potash furnished an aromatic oil of powerful cyanic odour which the author believes to be the cyanide of phenyl

$$C_7H_5N.$$

This compound differs in every respect from Fehling's benzonitrile, with which it is isomeric. A great number of similar reactions were indicated by the speaker, and a general principle enunciated which will take a long time fully to exhaust. Already the author has made good progress towards examining the action of chloroform upon the diamines.

Professor **Abel**, who at this stage of the proceedings occupied the chair, said that at so late an hour it would be impossible to enter upon a discussion of Dr. Hofmann's interesting results. He would, however, invite the members to give expression to the formal proposal which he had now the pleasure of moving. [The vote of thanks was received with loud acclamation.]

Dr. **Hofmann**, in acknowledging the kind welcome with which he had been received, testified to the pleasure he felt in the circumstance that a meeting of the Chemical Society happened to coincide with the period of his short visit to London. He was delighted to meet at once so many friends, and could assure them that he felt so greatly the want of similar encouragement in Berlin that within the last few weeks he and his colleagues had inaugurated a Chemical Society in the Prussian capital, founded on the model of the parent Society in London—(Applause).

The **Secretary** announced the title of a paper to be read at the next meeting, 19th instant, viz.:—*Analogies in the Cooling of Water and Bismuth.*" by Mr. Alfred Tribe. The meeting in January (16th proximo) would be devoted to a lecture "*On Water Analysts,*" by Dr. E. Frankland; and Mr. Siemens had promised to give a lecture, of which the date was not yet fixed, "*On the Production of Steel direct from the Or* .'

The meeting, at which there was a very full attendance, both of Fellows and visitors, was then adjourned.

Thursday, December 19.

Dr. Warren de la Rue, F.R.S., etc., *President, in the Chair.*

The minutes of the previous meeting were read and confirmed. Messrs. Alfred Coleman and A. A. Wood were formally admitted Fellows of the Society; and Dr. W. F. Smith, Glossop Road, Sheffield, and Mr. Alfred E. Fletcher, Inspector of Alkali Works, Johnston, near Prescot, were duly elected.

The names of candidates read for the first time were—Herbert M'Leod, Assistant Chemist in the Royal School of Mines, 61, Bridge Street, Southwark; Thomas Charlesworth, Leicester; Robert Schenk, 10, Hanover Place, Kennington; and John Wallace Hozier, B.A., Oxon, Lieutenant 2nd Dragoon Guards, Staff College, Sandhurst.

For the second time were read the names of the following:—Peter Greiss, Burton-on-Trent; Gilbert W. Child, M.D., St Giles', Oxford; Edward Chapman, Lecturer in Natural Science at Merton College, Oxford, (Frewen Hall, Oxford); William George Mason, Escrick, near York; and Alexander Walker, Captain Royal Artillery, Bengal Presidency, 18, Sussex Place, South Kensington.

Mr. **Alfred Tribe** then read a short paper "*On the Freezing of Water and Bismuth.*" The author quoted an extract from Professor Tyndall's "Heat as a Mode of Motion" (Page 84), in which it is asserted that the anomalous expansion of water in the act of cooling below 4°C. is by no means an isolated instance of the kind, but that other bodies, and particularly molten bismuth, participate in this extraordinary property of expanding near the point of solidification. After making experiments upon this subject, Mr. Tribe arrives at the conclusion that the analogy between water and bismuth is imperfect, since in the case of the molten metal there is no perceptible range of temperature through which it expands on cooling. The act of solidification is itself accompanied by an increase in bulk, but there is no evidence of this expansion taking place prior to the act of crystallisation.

After a few words in support of Mr. Tribe's conclusion had been offered by Dr. J. H. Gladstone, the President, at an early hour, moved the adjournment of the meeting for the purpose of enabling the members to be present at the delivery of the Bakerian lecture before the Royal Society. Dr. Roscoe, the lecturer, described his recent "*Researches on Vanadium.*"

At the next meeting, January 16, Dr. Frankland will deliver a discourse "*On Water Analysis,*" and on February 6, Dr. Russell, "*On Gas Analysis.*"

PHARMACEUTICAL SOCIETY.

Wednesday, December 4, 1867.

T. H. **Hills**, Esq., *Vice President, in the Chair.*

After the minutes of the preceding meeting had been read and confirmed, and the thanks of the meeting given for several donations to the library and museum,

Dr. **Attfield** referred to the paper in the November number of the *Pharmaceutical Journal*, "*On a New Kind of Kamela,*" which was obliged to stand over from their last meeting. Since then he had seen Prof. Anderson, the discoverer of rottlerine, and had referred to the great difference between the amount of impurity he had found in the samples he had examined and that of Foibourg, who had found about 28 per cent. of impurity, while the only impurity Anderson had found was 3½ per cent. of ash. Dr. Anderson had worked upon a sample collected for him with great care by Dr. Cleghorn, while it was possible that Foibourg had worked upon a very impure article.

Mr. D. **Hanbury**, F.R.S., thought the explanation of Dr. Anderson scarcely satisfactory. He had written to Dr. Anderson asking him for a specimen of the kamala, for he thought Anderson might have been working on the new kind; but on examination he found it to be the old kind. Foibourg had tried every possible way, and he found it was not highly crystalline, and only in small quantities.

Dr. **Attfield** said that Dr. Anderson had found vegetable as well as mineral impurities.

Mr. Hanbury had found no vegetable substances, excepting remnants of the capsule, or portions of the leaf. The impurities were ferruginous earth and siliceous particles.

Mr. Howden then read an interesting paper "*On the Norwegian* (*Lofoden*) *Cod Fisheries.*" The author commenced by referring to the codfish migrating at certain periods of the year, which he thought was due to the instinct of propagation. He then described the way in which the fish were caught, and the livers preserved till the oil could be separated, giving full particulars respecting the different modes adopted by the fishermen and others.

Mr. Howden also exhibited four samples of oil, the finest being the genuine Lofodon oil imported by Mr. Moller, and the fourth sample a very dark brown oil with a strong disagreeable odour. The livers from which this sample was obtained had been boiled over an open fire for 12 hours, showing the pernicious influence of a strong heat in the separation of the oil.

The Chairman thanked Mr. Howden for the valuable paper, and referred to the oil obtained in England. He knew some very fine oil was obtained, and in considerable quantities, from livers weighing 1lb. and 2lbs. each.

A Member enquired how it was that the Norwegian Pharmacopœia allowed the use of several species.

Mr. Howden said that at the present time a new edition of the Norwegian Pharmacopœia was being prepared, which would be similar to the British Pharmacopœia.

Mr. Ince alluded to the various statements which have been made with reference to the cod fisheries. No two authors were at all agreed. He thought the paper read by Mr. Howden very valuable, as it gave the opinions of gentlemen direct from the spot. He then referred to the geographical position of Lofoden, and the different ways of spelling the word Lofoden. Only a few days since he was reading two pamphlets, both written by persons from the spot, and yet there was a great difference between them; and there had, he believed, been four different articles in the *Pharmaceutical Journal*, all expressing different opinions.

Professor Redwood, in thanking Mr. Howden for his very practical paper, said he believed there were great advantages attaching to the preparations of the oil in Norway, arising from the fact that they had greater facilities for obtaining the livers fresh and exposing them to a low temperature for the separation of the stearine from the oleine; but in England we could obtain very good oil, its intrinsic qualities being equal to Newfoundland oil. Moller's oil was of excellent quality; but there were different opinions as to which of the three kinds of oil were therapeutically the best. On exposure to the air it absorbed oxygen, and the difference between the light and the dark showed the degree of oxidation which had taken place. It probably undergoes a process of oxidation in the system, for persons in the habit of taking cod-liver oil often acquire an odour which the pure oil possesses after long exposure to the air.

Mr. H. B. Brady had prepared a paper entitled "*Supplementary Remarks on the Preparation of Medicated Pessaries and Suppositories;*" but as the time had expired, he postponed the reading of the paper, and only made a few observations on some specimens of moulds and suppositories, etc., which he had laid on the table. In speaking of pessaries, Mr. Brady said there was an objection to their being too large. He thought one dram would be found a much better size than two drams.

The Chairman stated that as the 1st of January would be the first Wednesday in the month, there would be no Pharmaceutical meeting till the 5th of February. He concluded by wishing them all a merry Christmas and a prosperous New Year.

MANCHESTER LITERARY AND PHILOSOPHICAL SOCIETY.

MICROSCOPICAL AND NATURAL HISTORY SECTION.

November 4th, 1867.

J. Sidebotham, Esq., *in the Chair.*

The Rev. J. E. Vize showed and presented to the section four specimens of insects beautifully mounted in balsam by himself.

Mr. Sidebotham read the following "*Note on the Ship-barnacle*":—

On the 28th of September I was at Lytham with my family. The day was very stormy, and the previous night there had been a strong south-west wind, and evidences of a very stormy sea outside the banks. Two of my children came running to tell me of a very strange creature that had been washed up on the shore. They had seen it from the pier and pointed it out to a sailor, thinking it was a large dog with long hair. On reaching the shore I found a fine mass of barnacles, *Pentalasinus anatifera*, attached to some staves of a cask, the whole being between four and five feet long. Several sailors had secured the prize, and were getting it on a truck to carry it away. The appearance was most remarkable, the hundreds of long tubes with their curious shells looking like what one could fancy the fabled Gorgon's head with its snaky locks.

The curiosity was carried to a yard where it was to be exhibited, and the bellman went round to announce it under the name of the sea-lioness, or the great sea-serpent.

I arranged with the proprietor for a private view, took my camera and a collodio-albumen plate, and obtained the photograph I now exhibit. The afternoon was very dull, and the plate would have done with a little longer exposure, but this, along with the specimens I show, will give some idea of the strange appearance of this mass of creatures.

The barnacle is of interest as being the one figured by Gerard as the young of the barnacle goose. As some of our members may not have seen the book and read the quaint description, I have brought my copy of Gerard's Herbal for their amusement.

I may just mention that another mass of barnacles was washed up at Lytham, and also one at Blackpool, the same day or the day following. I did not see either, but, from description, I have brought my copy of Gerard's Herbal for their amusement.

This mass of barnacles was evidently just such a one as that seen by Gerard at the Pile of Foulders. It is rare to have such a specimen on our coasts. The sailors at Lytham had never seen anything like it, although some of them were old men who had spent all their lives on the coast.

PHYSICAL AND MATHEMATICAL SECTION.

November 7th, 1867.

Robert Worthington, F.R.A.S., *President of the Section, in the Chair.*

"*Note on the Colour of the Moon during Eclipses,*" by A. Brothers, F.R.A.S., etc.

On the night of October 4th, 1865, there was a partial eclipse of the moon, when about one-third of the disc was obscured. My time and attention on this occasion were chiefly directed to some photographic experiments, and it will be remembered I then obtained pictures of the eclipse at short intervals, from the commencement to the end. At about the greatest phase I looked at the moon through the telescope with an eye-piece of low power, for the purpose of noticing whether the eclipsed portion showed colour, and at once saw that the part of the moon most deeply within the shadow was of a decided copper colour, such as I had seen some years previously during a total eclipse of the moon.

The eclipse which occurred on the night of the 13th September last, was rather more favourable for detecting the presence of colour, as seven-tenths of the moon were covered by the earth's shadow; the weather being clear, many persons have recorded their observations, and as the colour of the moon is one of the chief features of a lunar eclipse, this point attracted considerable attention.

Mr. Browning says, in a letter which appeared in the "Astronomical Register"—"I looked most carefully for colour both with the 10¼ silvered glass reflector furnished with an achromatic eye-piece of very low power, and also with a five-feet refractor; with neither could I detect a trace."

Mr. Slack, who was also observing with a silvered glass reflector, and in the same locality as Mr. Browning, says:—"After twelve, the eclipsed limb grew noticeably redder, the red coppery tint chiefly affected the lower parts of the obscured limb, but was visible further in, gradually blending with the inky tints presented by the umbra at its advancing edge."—(*Int. Obs.*, October.)

Mr. Weston, observing at Landsdown, near Bath, says,—"The prevailing colours were red-bluish and grey, *and grey:* the redness increased towards the darkened edge of the moon."—(*Monthly Not.*, 9, xxvii.)

Many other observers speak of the presence of colour, but on the other hand a few say they did not notice any, the eclipsed portion of the moon merely having a darkened appearance.

On this occasion I did not make any photographs, as I could not expect results materially differing from the last, and I gave my whole attention to observing the progress of the eclipse through the telescope, which is a refractor of five inches aperture. As to the question of the presence of colour, I can most distinctly say that *colour* gave the moon a very beautiful appearance, and it seemed to me the most interesting feature of the eclipse. The beauty of the moon's surface appeared to increase as the penumbral shadows stole over its surface; and until the shadow itself was considerably advanced, all the details of the lunar surface could be distinctly made out, and during the whole period of the eclipse some of the brighter points of light within the shadow continued visible, as did also the entire disc with many of the details of light and shade. The colour of the eclipsed limb was of a coppery hue, much brighter towards the par most deeply within the shadow. The part of the moon not eclipsed was of a beautiful bluish-grey colour.

That the appearance of colour cannot be caused by the telescope or by peculiarities in the eyes of the observers is, I think, proved by the fact, that the same colours are seen whether refractors or reflectors, either of metal or silvered glass, be used; and, as the majority of observers see colour, the eyes of those who remark the absence of it are perhaps temporarily afflicted with colour blindness;—the bright light from the uneclipsed portion of the moon may be sufficient to produce this in some persons. If an observer, after looking at the moon through the telescope, attempts to look at objects while the other eye remains closed, it will be found that until the retina has recovered from the excess of light everything will appear misty—in fact the eye is partially blinded, and it may be that some eyes are sufficiently sensitive to be affected by the diminished lustre of the moon, and may thus be prevented seeing colour, which there nca be no doubt the lunar surface presents during an eclipse.

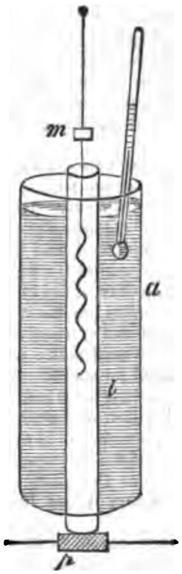
Some observers have remarked on the difficulty of detecting the first appearance of the shadow, and although the exact time is known, the real shadow is not seen until many seconds or perhaps minutes after the predicted time. In the present instance, I watched very carefully for the first appearance of the shadow, and having previously ascertained the error of my watch, I took my position at the telescope, at the same time I requested a friend to take particular notice of the time at the moment I saw the shadow. The result thus obtained was within twenty seconds of the time given in the "Nautical Almanac," clearly showing that under favourable conditions, and if the attention be given carefully to the subject, the real shadow may be detected very near the predicted time.

Ordinary Meeting November, 26th, 1867.

Edward Schunck, F.R.S., etc., *President, in the Chair.*

"*On a Thermometer unaffected by Radiation,*" by

Dr. J. P. Joule, F.R.S., etc.



In the annexed figure *a* is a copper tube about one foot long, and has a tube open at both ends in the centre. Water is poured into the space between the two tubes. In the centre tube there is a spiral of fine wire suspended by a filament of silk, and having a mirror at *m*. There is a lid at *p* which can be removed at pleasure from the lower end of the tube. When *p* is situated as in the figure, there can be no draught, and consequently the spiral with its mirror is at zero of the scale. But when *p* is removed, there is a current of air wihch turns the spiral, if the air in the tube has a different temperature from that of the outside atmosphere. In my apparatus, one degree F. produces an entire twist of the filament. I find that the temperature in the tube is generally warmer than in the outside atmosphere of a room, which must be owing to the conversion of light and other radiations into heat on coming into contact with the copper tube. I have tried the apparatus in the open air on a still day, with the same result. Of course when there is wind the effect is masked, but I feel confident that by increasing the length of the tube, making it thirty feet for instance, and using certain precautions, this difficulty may be overcome.

ACADEMY OF SCIENCES.

December 9, 1867.

Stellar Spectra—Ozonometry—Dialysis of Induction Currents—Electrolysis of Organic Salts.

This day's meeting of the Academy was opened with the announcement of the death of M. Flourens, the perpetual secretary.

Father Secchi contributed a note on the spectra of stars and meteors.

An interesting memoir was brought forward by MM. Berigny and Salleron, in the form of a reply to a note recently addressed to the Academy by M. Poey, on the ozonoscopic colourations produced in iodide of potassium and starch, and on the ozonometric scale of M. Berigny.

M. Poey's note treated of two distinct questions—(1) The various colourations taken by the test-paper under different atmospheric conditions; (2) the insufficiency of M. Berigny's scale.

Without affirming the reaction to be irreproachable, they consider some of the sources of error to proceed from the mode of experimentation which has been in use up to the present time. When the test-paper is exposed for twelve hours in an atmosphere charged with ozone, the reagents undergo complicated decompositions. MM. Berigny and Salleron believe it would be otherwise if the method of making the experiment were modified; they describe at some length a process by which they hope to eliminate sources of error.

One point to which attention is drawn, is the immersion of the test-paper in distilled water immediately a definite tint is reached. When the paper is exposed in an atmosphere containing much ozone the reagent mixture is rapidly decomposed, and if not plunged into water until some hours afterward the iodide of starch possibly undergoes alteration, and does not then produce normal tints; this is especially the case when the air is very moist. This perturbing cause, however, is inherent and is recognised.

With regard to the ozonometric scale, they propose some changes—instead of determining the amount of ozone from the colour the test-paper acquires during twelve hours, they prefer to measure the time necessary for the paper to be exposed to produce a determinate shade of colour. Suppose on a certain day it requires an hour's exposure to obtain the tint, and the next day two hours' exposure is necessary, evidently only half as much ozone is present on the second day as there was the first day. Thus the quantity of ozone may be said to be inversely proportional to the duration of exposure.

By a mechanical contrivance a band of test paper, twelve centimètres in length, is moved at a definite rate from protection to exposure to the action of the atmosphere. How the test paper is protected from the action of the atmosphere for some hours in the instrument was not explained, and this to your correspondent seems the most difficult and at the same time the most essential point to secure. However, if the band be made to move at the rate of one centimètre per hour, at the end of twelve hours a band of paper twelve centimètres in length will have passed; at one end of it a centimètre will have been exposed for twelve hours, the next for eleven, and so on down to one. The band of paper having thus passed the instrument, it is plunged into distilled water, and colours varying between white and violet are displayed. It is now only necessary to compare this with the standard colour, and to measure the position on the band where the tints are identical. It is then known how many hours it has been exposed.

They choose a faint tint as the standard, as this will be applicable in all latitudes, the fourth tone of the first violet in M. Chevreul's chromatic circles.

A physical paper on the dialysis of induction currents by M. Bouchotte was presented by M. Becquerel. At the same meeting there was also a paper on the electrolysis of acetic acid by M. Bourgoin. In a former paper the author advanced a theory referring to the electrolysis of organic acids and salts generally. In this he applies it to acetic acid, and gives the detail of experiments.

The apparatus he arranged was the following:—A tube closed at the upper extremity with a caoutchouc cap, and at the lower extremity closed with the exception of a very small hole. Through the cap passes a small syphon tube almost capillary, as well as a platinum wire, which terminating inside the tube in a plate of that metal, forms one electrode. This tube is encircled by a larger one of such capacity, that when the disengaged gas in the interior exerts a pressure of 4 centimètres, the volume of solution in each tube shall be the same. In the annular space formed by these tubes the other electrode is plunged. Experiments were made with a neutral solution of acetate of potash which had been analysed: after submitting it for six hours to the electrolysing action of four elements, a portion of the liquid was drawn off from the neighbourhood of each pole and analysed. The conclusions arrived at are that the decomposition into carbonic acid and carburetted hydrogen is almost *nil*, and that the greatest loss is at the positive pole. Daniell and Miller, M. Bourgoin remarked in his paper, found in the electrolysis of inorganic compounds the negative pole to be that at which the greatest loss occurred. In organic chemistry the only fact known *àpropos* of the subject is the observation due to M. Hittorf, who found in the electrolysis of acetate of silver the greatest loss to proceed from the positive pole.

His observation is therefore confirmed by these more recent experiments.

The author sums up the result of his experiments as follows:—

1. The current acts on acetate of potassium as on a mineral substance.

2. In a moderately alkaline solution the oxygen reacts on the elements of the anhydrous acid, and gives rise to a normal oxidation, whence results carbonic acid and hydride of ethylen:—

$$C_8H_6O_6+O_2=2C_2O_4+C_4H_6.$$

3. A certain quantity of acid is totally consumed under the influence of oxygen furnished either by the salt or by the alkaline water.

4. The two poles suffer unequal losses. Almost the whole of the salt which disappears belongs to the positive pole.

5. The current acts on the free acetic acid in the same manner as sulphuric acid; it concentrates the acid at the positive pole.

The gas evolved during the electrolysis was found to be chiefly composed of oxygen, with some carbonic acid, and a little carbonic oxide. Acetate of potassium and an alkali in equivalent proportions evolved at the positive pole only oxygen. When the amount of alkali was increased, the same result was obtained, but a concentrated solution of 2 equivalents of acetate of potassium and 1 of alkali yielded oxygen, carbonic acid, carbonic oxide, and carburetted hydrogen.

M. Kolb has expressed the opinion that acetic ether and possibly also a small quantity of methylic ether might be formed. M. Bourgoin observed no formation of these products.

DUBLIN CHEMICAL AND PHILOSOPHICAL CLUB.

Dec. 12, 1867.

"*The Action of Ozone on Sensitive Photographic Plates.*" Dr. Emerson Reynolds stated that he had been performing some experiments upon the above subject, and that he had found that when the latent image (*i.e.*, the image before it is developed) was submitted to the action of ozone, it was completely obliterated—not only was it impossible to develop the image, but a second image might be retaken in the camera upon the same plate. The author remarked that this was against the theory which might be called the mechanical theory of photographic images, and proved conclusively that it was due to chemical change in the sensitive film. He also thought that many of the disputes in connection with the length of time dry plates might remain sensitive, was probably owing more or less to the quantity of ozone present in the air.

The ozone used in these experiments was in some cases procured by passing atmospheric air over phosphorus, and in others by the silent discharge, viz., by attaching one of the platinum wires of the reservoir to the prime conductor of a machine, and turning it slowly, the other wire being in communication with the ground.

"*Nitrite of Amyl.*"—Mr. Tichborne brought before the notice of the meeting a statement by Mr. Chapman, which appeared in the *Laboratory* of August 31, namely, that nitrite of amyl was not decomposed by heat, as stated by Mr. Tichborne. The speaker had not considered the matter of sufficient importance to enter into a written dispute with that gentleman, but felt that it was due to the members of the society to prove that Mr. Chapman was in error, more particularly from the fact that the observations, to which Mr. Chapman took objection, were first brought forward at one of their meetings of the previous session. Mr. Tichborne said his note was merely published with a view to correct some erroneous descriptions of nitrite of amyl which were given in the manuals of chemistry. The speaker found that the redness of the vapour (described as a specific property of nitrite of amyl) was due to a partial dis-association of the elements and the consequent elimination of binoxide of nitrogen.

Mr. Chapman, on the contrary, "was of opinion that nitrite of amyl was an ether of great stability." "That when once obtained tolerably pure it would bear distillation without undergoing any appreciable decomposition." Now, although Mr. Tichborne had no doubt that Mr. Chapman's general observations in connection with this substance were very exact, he was compelled to take exception to this point. He found that the perfectly neutral nitrite became instantly acid on ebullition, and gave off binoxide of nitrogen; that this decomposition, which was however comparatively partial, continued during the whole of the distillation, and was as decided at the conclusion of the operation as at the commencement. Also that pure nitrite of amyl by spontaneous decomposition became acid and charged with oxides of nitrogen. Mr. Tichborne then showed an experiment which illustrated and conclusively confirmed his remarks.

A sample of nitrite of amyl was taken which had been made after the plan recommended by Mr. Chapman, viz., by passing nitrous acid through amylic alcohol, the latter having been previously purified by fractional distillation; it had been treated with dry carbonate of sodium, and was therefore quite free from the acid products of decomposition. Half of the specimen was poured into a solution of starch and iodide of potassium; no change was manifested, showing that the specimen was free from any absorbed binoxide of nitrogen.[*] The other half of the specimen was then placed in a small retort, the vapour from which was passed through the same mixture of iodide of potassium and starch. Heat having been then applied to the nitrite of amyl in the retort, the first puff of gas that escaped through the mixture instantly determined a copious deposition of blue iodide of starch.

Dr. Frazer, in showing some interesting minerals, remarked that glycerine would be found to be a very useful material for preserving lanmonite (efflorescing zeolite) and such like minerals that lose their water of hydration. Thus he had kept specimens for years by smearing them with a little glycerine, and what would naturally be destroyed in a short time when placed dry in a cabinet, were by this means kept perfectly safe.

CHEMICAL NOTICES FROM FOREIGN SOURCES.

Sulphochlorbenzolic Acid, and Derivatives of.—R. Otto and L. Brummer. Chlorinated or brominated substitution compounds of benzol or toluolsulphurous acid cannot be obtained by the direct action of chlorine or bromine, but may be prepared by treating chlorinated sulphobenzolic (or tuluolic) chloride with sodium-amalgam. The reaction takes place according to the equation:

$$\left.\begin{matrix}\Theta_6H_4ClS\Theta_2\\ \\ Cl\end{matrix}\right\}+2Na=\left.\begin{matrix}\Theta_6H_4ClS\Theta\\ \\ Na\end{matrix}\right\}\Theta+NaCl$$

Sulphochlorbenzolic acid is obtained by dissolving chlorobenzol in sulphuric acid, neutralising with plumbic carbonate, and decomposing the plumbic sulphochlorbenzolate with sulphuretted hydrogen. The aqueous solution of the acid is evaporated on the water-bath, and the syrupy mass thus obtained becomes crystalline on cooling. It is readily soluble in alcohol, insoluble in ether and benzol. Its formula is:

$$\left.\begin{matrix}\Theta_6H_4Cl\\ S\Theta_2\\ H\end{matrix}\right\}\Theta$$

On acting upon the sodic salt with phosphoric pentachloride, sulphochlorbenzolic chloride,

$$\left.\begin{matrix}\Theta_6H_4Cl\\ S\Theta_2\\ Cl\end{matrix}\right\}$$

is formed; it crystallises well, fuses at 50—51° C., is insoluble in water, soluble in ether and benzol; it dissolves in alcohol with formation of its ether. Fuming nitric acid converts the chloride into nitrosulphochlorbenzolic acid; alcoholic ammonia into the amide

$$\left.\begin{matrix}\Theta_6H_4ClS\Theta_2\\ \\ H_2\end{matrix}\right\}N$$

—nascent hydrogen into chlorphenylic sulphhydrate, according to the equation:

$$\left.\begin{matrix}\Theta_6H_4Cl\\ S\Theta_2\\ Cl\end{matrix}\right\}+6H=\left.\begin{matrix}\Theta_6H_4Cl\\ \\ H\end{matrix}\right\}S+HCl+2H_2\Theta$$

The sulphhydrate forms beautiful large crystals which are soluble in ether, benzol, and hot alcohol, insoluble in water, and fuse at 53—54°. It is converted into chlorphenylic disulphide on being gently heated with nitric acid:

$$2\left\{\begin{matrix}\Theta_6H_4Cl\\ \\ H\end{matrix}\right\}S+\Theta=\left.\begin{matrix}\Theta_6H_4Cl\\ \\ \Theta_6H_4Cl\end{matrix}\right\}S_2+H_2\Theta$$

This sulphide crystallises well, is insoluble in water, soluble in ether and hot alcohol, and fuses at 71°; it may be reconverted into the sulphhydrate by nascent hydrogen. Chlorbenzolsulphurous acid is converted into sulphochlorbenzolic chloride on being treated with chlorine, and into chlorbenzylsulphhydrate (identical with the mercaptan from sulphochlorbenzolic chloride) when subjected to the action of nascent hydrogen.—(*Ann. Chem. Pharm.* cxliii. 100.)

Oxyethylendisulphonic Acid and New Formation of Isethionic Acid.—Th. Meves. The author prepares isethionic acid by the following method:—Equal weights of dehydrated baric sulphovinate and sulphuric anhydride are mixed together, and the mixture is heated on the water-bath after the first violent reaction is over. The black mass thus obtained is dissolved in water, the solution boiled for several hours, and, after dilution, neutralised with baric carbonate, filtered, and the filtrate precipitated with potassic bicarbonate. The filtrate thereof is evaporated to dryness, and extracted with alcohol. which dissolves potassic isethionate. Oxyethylendisulphonic acid is obtained by the action of sulphuric acid on isethionic acid according to the equation:—

$$(C_4H_5O_2)\,[S_2O_4]O.HO+S_2O_6=(C_4H_4O_2\begin{pmatrix}S_2O_4\\ S_2O_4\end{pmatrix}O_2.2HO$$

It is prepared by heating one part of potassic isethionate with three parts of fuming sulphuric acid, dissolving in much water, and neutralising with baric carbonate. The baric salt is converted into the potassic salt, and the latter crystallised out. The free acid obtained from this by decomposition with sulphuric acid forms a syrupy liquid of strong reaction which does not crystallise.—(*Ann. Chem. Pharm.* cxliii. 196.)

Phenylic Acid, and Derivatives of.—Glutz. Phenylic chloride is obtained by heating together in equivalent proportions carbolic acid and phosphoric pentachloride. The phenylic chloride is distilled off and freed from phenol by shaking the mixture with a solution of sodic hydrate. The residue, after distillation, consists chiefly of neutral phenylic phosphate. The boiling-point of the chloride after purification is 130° C. When heated with an excess of sulphuric acid to 100° for several hours, chlorphenyl sulphuric acid is formed, which is purified by being converted into lead salt, separated again from the metal by means of sulphuretted hydrogen. It is a syrupy liquid which slowy crystallizes under the desiccator. It is moderately soluble in alcohol, insoluble in ether. The composition of the lead salt, which crystallizes well, as do most salts of this acid, is

$$\left(C_{12}\left\{\begin{matrix}H_4\\ Cl\end{matrix}\right)\right.[S_2O_4]O.PbO$$

Chlorphenylsulphuric acid is converted into phenylsulphuric acid by the action of sodium-amalgam. The lead salt, treated

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[*] Nitrite of amyl absorbs binoxide of nitrogen very readily, and when first prepared is more or less charged with this gas. "Probably these circumstances induced Mr. Tichborne to regard the ether as decomposable on boiling." *Vide* Mr. Chapman's note in *Laboratory*, August 31, 1867.

[English Edition, Vol. XVI., No. 421, page 322; No. 418, pages 291, 292; Vol. XVII., No. 422, page 10.]

with strong nitric acid, is partly converted into nitrochlorphenylsulphate, partly into nitrochlorphenyl,

$$C_{12}\left\{\begin{array}{l}H_4\\Cl\\NO_4\end{array}\right.$$

The latter may also be obtained from phenylic chloride.

Phenylic phosphate $(C_{12}H_5O)_3\ PO_5$ is insoluble in water, readily soluble in alcohol, ether, and hot sulphuric acid; it is converted into diphenolphosphoric acid when acted upon by strong bases. When heated with bromine in sealed tubes to 180° the neutral ether gives rise to the formation of a white crystalline body of the composition $(C_{12}(H_4Br)O)_3.PO_5$, the reaction being represented by the following equation,

$$(C_{12}H_5O)_3.PO_5+6Br=\left(C_{12}\left\{\begin{array}{l}H_4\\Br\end{array}\right\}O\right)_3.PO_5+3HBr$$

Ann. Chem. Pharm. cxliii. 181.

Toluol, Substitution Compounds of.—F. Beilstein and A. Kuhlberg. Of the four possible isomeric trichlortoluols, $\text{Ɵ}H_2Cl_3(\text{Ɵ}H_3)$, $\text{Ɵ}_6H_5(\text{Ɵ}Cl_3)$, $\text{Ɵ}_6H_3Cl_2$ $(\text{Ɵ}H_2Cl)$, and Ɵ_6H_4Cl $(\text{Ɵ}HCl_2)$, the first (trichlortoluol) and second (benzoterchloride) are already known. The third, dichlorbenzylic chloride, is formed by passing a current of chlorine through benzylic chloride containing iodine, or through boiling dichlortoluol. It boils at 241°C., loses *one* atom of chlorine on being treated with alcoholic potassic hydrate. The fourth, chlorbenzylalic chloride, is obtained by passing chlorine through benzylalic chloride (chloride of oil of bitter almonds) charged with iodine, through boiling chlortoluol. It boils at about 221°, and contains only *one* atom of chlorine firmly attached. The boiling points of the 4 isomers are respectively—235°, 241°, 221°, 218°. Benzylalic chloride dissolves in concentrated nitric acid with formation of a nitro compound, which on being treated with chromic acid is converted into *nitro-benzoic* acid, while the analogous chlor-benzylalic chloride under similar conditions forms *para* chlorbenzoic acid.—(*Zeitschr. Ch.* N. F. iii. 513.)

Cyanacetic Acid.—Th. Meves. Pure cyanacetic acid was prepared in the following manner:—250 grm. of monochloracetic ethide, 300 grm. of potassic cyanide, and 1,200 grm. of water were heated on the oil bath in a retort connected with a reversed Liebig's condenser until the smell of prussic acid had disappeared; an excess of the ether was then distilled off. The dark brown liquid thus obtained was then neutralized, evaporated to half its original volume and filtered, the filtrate further concentrated, and after addition of an excess of sulphuric acid, extracted with ether. The etherial solution on evaporation leaves the crude acid, which is purified by being converted into the lead salt, and the latter decomposed by sulphuretted hydrogen. The salts of this acid, of which the potassic, baric, zincic, cupric, argentic, mercuric, and plumbic, are described, are very soluble in water, with the exception of the two last named. They are obtained either by saturating the free acid with the oxides or by mutual decomposition of ammonic cyanacetate with the neutral metallic salt of the desired radical.—(*Ann. Chem. Pharm.* cxviii. 201).

Chlorsalylic Acid, Preparation of.—Glutz. Gaultheria-oil and phosphoric perchloride in the equivalent proportion of 1 to 2 are mixed together in a large well-cooled flask. The reaction, at first very violent, is completed by heating on the water-bath for several hours. The flask is then connected with a reversed Liebig's condenser, and the boiling continued for a day. After this treatment the (now liquid) products of the reaction are distilled, and the fraction going over above 220° C., poured into a large quantity of boiling water. This dissolves, under evolution of chlorhydric acid, all, except chlorsalylic trichloride, which remains as a heavy brown oil (a solid cake when cold) at the bottom of the vessel. On cooling the chlorsalylic acid crystallizes out from the aqueous solution in white needles.—(*Ann. Chem. Pharm.* cxlii. 194).

Manganese, New Compounds of.—T. Niklès. Fluormanganous acid is formed by adding fluorhydric acid to an etherial solution of manganic perchloride, or by dissolving in the concentrated acid manganic dioxide. The reactions of fluormanganous acid are similar to those of perchlorides. Alkalic fluorides produce rose-coloured precipitates of double fluorides. It likewise combines with organic bases. All these compounds are of comparatively unstable nature, most of them being gradually decomposed when in contact with much water. On adding manganic perchloride to a boiling solution of potassic or ammonic fluoride, precipitates are formed which may be considered as salts of oxyfluormanganous acid, Mn(OFl).—(*Comptes R.* lxv. 107.)

Mimetesit, Artificial Preparation of.—By following the general method adopted by Deville and Caron for the preparation of crystallised chlorophosphates, G. Lechartier has succeeded in obtaining corresponding chlorarseniates. The arseniate and chloride of the same base are fused together, and after cooling the excess of chloride is dissolved out by water, which leaves the crystallised chlorarseniate behind. Mimetesit, $3(AsO_5, 3PbCl)PbCl$ amongst other compounds, has thus been obtained.—(*Comptes R.* lxv. 172.)

Phenolsulphuric Acid, Salts of.—E. Menzner. Phenolsulphuric acid was prepared by heating equal equivalents of phenyl and sulphuric acid on the water-bath, diluting, 24 hours later, with water, neutralising with plumbic carbonate, decomposing the lead-salt with sulphuretted hydrogen, and concentrating the dilute solution first by applying heat and finally in the desiccator over sulphuric acid. The salts are obtained by neutralising the free acid with oxides or carbonates. They are all soluble in water, mostly well crystallized, contain, with the exception of the ammonic salt, water of crystallization, which they lose at 140° C. The plumbic and cupric salts decompose when heated to that temperature. The stability of the acid, which in aqueous solution bears continued boiling, and that of its salts, support the supposition that its constitution is different from that of the normal sulphovinic acids.—(*Ann. Chem. Pharm.* cxliii. 175.)

Colouring Matter of Saffron.—B. Weiss has re-examined the colouring matter of saffron (polychroite) and finds that the discrepancies in the statements of former investigators are due to the ease with which the dye decomposes during the processes of purification. The dye seems to be a glucoside; on being treated with sulphuric acid it splits up into a secondary colouring matter, named by the author crocin, sugar, and an essential oil. The composition of crocin is $C_{32}H_{18}O_{12}$, that of the oil $C_{20}H_{14}O_2$.—(*Journal pr. Chem.* ci. 65.)

Naphtalene, Action of Oxidising Agents on.—F. Lossen. Naphtalene when treated with a boiling solution of potassic permanganate is oxidised to carbonic anhydride and phtalic acid. Phtalic acid is also formed, besides a red resinous body, by the action of potassic bichromate and sulphuric acid, or manganic peroxide and sulphuric acid; in the latter case one of the products of oxidation is dinaphtyl, formed according to the following equation:—

$$=\quad 2\left\{\begin{array}{l}\text{Ɵ}_{10}H_7\\H\end{array}\right\}+\text{Ɵ}=\left.\begin{array}{l}\text{Ɵ}_{10}H_7\\\text{Ɵ}_{10}H_7\end{array}\right\}+H_2\text{Ɵ}$$

Dinaphtyl is a white, crystalline body, which dissolves readily in ether or carbonic disulphide, less in alcohol or benzol, fuses at 150°C. It has been converted into 1. Dibromdinaphtyl, $\text{Ɵ}_{20}H_{12}Br_2$, soluble in benzol, from which it crystallises well, very readily soluble in a mixture of alcohol, ether, and carbonic disulphide, and fusing at 115°. 2. Hexabromdinaphtyl, $\text{Ɵ}_{20}H_8Br_6$, a yellowish resinous mass, soluble in ether; sodium amalgam re-substitutes hydrogen with formation of dinaphtyl. 3. Hexachlordinaphtyl $\text{Ɵ}_{20}H_8Cl_6$,

[English Edition, Vol. XVII., No. 422, pages 10, 9; Vol. XVI., No. 420, page 314.]

resembles closely the corresponding bromo-compound. 4. Tetranitrodinaphtyl $\Theta_{20}H_{10}(N\Theta_2)_4$, an orange-coloured, resinous substance, soluble in alcohol. This body, when treated with tin and chlorhydric acid, yields small quantities of a base of very unstable character.

The resinous matter which is obtained by oxidising naphtalene with potassic bichromate and sulphuric acid, or more readily with manganic peroxide and sulphuric acid, contains an amorphous acid of the composition $\Theta_{20}H_{14}\Theta_4$.—(*Zeitschr. Chem.* N. F. iii., 419.)

Substituted Alcohols and Aldehydes. F. Beilstein and A. Kuhlberg. The following compounds were prepared: Paranitrobenzylic acetate, $\Theta_7H_6(N\Theta_2).\Theta_2H_3\Theta_2$, by adding benzylic acetate to well-cooled, strongest nitric acid, and precipitating with iced water; yellowish needles, fusing at 78°C. readily soluble in hot alcohol. Paranitrobenzylic alcohol, $\Theta_7H_7(N\Theta_2)\Theta$, by heating the former compound with aqueous ammonia in sealed tubes to 100°; white crystals, fusing at 93°, soluble in hot water and ammonia. Paranitrobenzylic oxalate $[\Theta_7H_6(N\Theta_2)]_2.\Theta_2O_4$, by dissolving benzylic oxalate in strong nitric acid. Like the acetate it gives the alcohol on being heated with ammonia. Benzylic oxalate, $(\Theta_7H_7)\Theta_2.\Theta_4$, by the action of chlorbenzyl upon argentic oxalate; crystallises like naphtalene, fuses at 80·5°, insoluble in water, soluble in hot alcohol. Oxidising agents convert the alcohol and all its ethers into paranitrobenzoic acid. Parachlorbenzylic alcohol, $\Theta_7H_7Cl\Theta$, by heating chlorbenzylic acetate with ammonia to 160°; scarcely soluble in boiling water, fusing at 66°, is converted into parachlortoluylic acid, $\Theta_7H_5Cl\Theta_2$, on being treated with oxidising agents. Dichlorbenzylic alcohol, $\Theta_7H_6Cl_2\Theta$, by heating in a sealed tube dichlorbenzylic acetate (obtained by the action of alcoholic potassic acetate on dichlorbenzylic chloride, $\Theta_6H_3Cl_2(\Theta H_2Cl)$ with ammonia. Parachlorbenzoic aldehyde, $\Theta_7H_4Cl\Theta.H$, by boiling chlorbenzylic chloride, $\Theta_6H_4Cl(CH_2Cl)$ with an aqueous solution of plumbic nitrate, combining the insoluble oil with sodic bisulphite, and boiling with sodic hydrate. The aldehyde absorbs oxygen and is converted into parachlorbenzoic acid.—*Zeitschr. Chem.*, N. F. iii., 467.

NOTICES OF BOOKS.

AGRICULTURAL CHEMISTRY.

I. *Report of some Experiments in Agricultural Chemistry carried out in the Laboratory of the Royal Agricultural College.* By Arthur H. Church, M.A. Oxon, F.C.S. (Practice with Science, vol. i., series ii., No. 6).

II. *Report of Experiments on the Solubility of Phosphates.* By R. Warrington, jun., F.C.S., Assistant to the Professor of Chemistry, Royal Agricultural College, Cirencester, (Practice with Science, vol. i., series ii., No. 4).

III. *Notes on some of the Circumstances which determine the Agricultural Value of the Natural Phosphates, with a brief account of the present Method of Analysing them.* By R. Warrington, jun. (Practice with Science, vol. i., series i., No. 7).

IV. *On the Capillary Action of Soils.* By John Wrightson, F.C.S., Professor of Agriculture in the Royal Agricultural College, Cirencester (Practice with Science, vol. i., series ii., No. 3).

We have given, nearly in order of importance, the headings of the various papers which give a summary of the researches that have been carried on, during the last few years, by the different chemists connected with the Cirencester Agricultural College. The papers will be found in full in the first volume of "Practice with Science"*—a work capitally printed, indexed, and arranged, as fit to be placed on the drawing-room table as on the shelf of the chemist's or agriculturist's library. The first volume contains, besides these papers, much valuable matter of a more technical kind. We hope, however, on an early opportunity, to do ourselves the pleasure of noticing separately Professor Church's wheat experiments, by which it is shown how to estimate tolerably, by a cursory inspection and examination, the various ratios of starchy to nitrogenous matter contained in wheat grain.

The highly ammonical Peruvian guano was examined by Professor Church in the three following ways:—1 was a specimen quite fresh; 2 was dried at 100°, when more than 50 per cent. by weight was lost; 3 was examined after being kept for a few months. The ammonia from the 1st experiment was ascertained to be 20·55 per cent., corresponding to 16·92 per cent. of nitrogen calcium; and magnesium phosphates, 16·98, with a quantity of P_2O_5, in the alkaline estimation, equivalent to ·79 per cent. The ammonia lost by experiment No. 2 amounted to 10·46 per cent., leaving thus a percentage of nitrogen of 7·72, as compared with 16·95 given by the 1st experiment. After 12 months' keeping the nitrogen amounted to only 9·08 per cent., as compared with the nitrogen contained in the 1st sample; this shows a loss of 7·87 of nitrogen by 12 months' keeping. Professor Church remarks that this fact points strongly to the probable advantage to be derived from fixation by the sulphating process of such guanos.

In an analysis of acorns, upon which Professor Church informs us horses and sheep will feed readily, the kernels, husks, and entire acorns were examined. The kernels contain 3·08 per cent. of albuminoids, compared with 1·83 in the husks, starch, cellulose, and sugar; 47·17 in the kernels, as opposed to 44·33 in the husks; while the husks gave 26·59 per cent. of insoluble fibre, the kernels 2·58 only. This large proportion of calorifacient food is sufficient, we think, to justify Mr. Church in a series of determinations of their comparative economic use, and also for settling to what cause the harsh flavour of the kernel is to be assigned, and whether this drawback could be removed by any simple process. Acorns and horse-chestnuts may yet, by judicious chemical treatment, be sources of profitable nutriment.

As regards the alleged value of spentgalls from chemical works as a manure for market garden crops, from a sample supplied by Messrs. Hopkin and Williams, Prof. Church does not give much hope. "At present it would be premature to affirm that they are worth purchasing at all." The value seems to be either mechanical upon certain soils, or chemical merely from the carbolic acid evolved during putrefactive decay.

Sugar boilers' scum, on the other hand, affords much calcium phosphate and an appreciable quantity of nitrogen. This scum may be obtained in large quantities and at a cheap rate.

Apropos of Farnham, of workhouse celebrity, our readers will doubtless recollect that before the current month, the place was celebrated for its interesting formations of greensand and gault, and the commercial working of the coprolites there into phosphatic manures. The question of the applicability of these fossil excreta to manure has gained much attention, and thus Professor Church was induced to examine for them the junction of the greensand with the upper chalk in Wiltshire, near Calne. They were found to contain very much calcium carbonate, a larger quantity of sulphuric acid being thus required for their manufacture into manure. It is estimated, in the paper under notice, that these Calne coprolites are worth about 28s. per ton; referred to the standard of those of Cambridgeshire, the latter show a value of 40s. per ton; the phosphates here are equal to about 55 per cent. of tricalcic phosphate.

In the succeeding paper Professor Church finds that from the improved method of extraction of the oil from hard kernels, only about 20 per cent. of oil remains in palmnut-kernel meal (in one case 17 only), as compared with 26 and upwards found some time ago by Dr. Vœlcker.

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* Practice with Science—a series of agricultural papers—vol. i., London: Longmans, Green, Reader and Dyer. 1867.

The last of this interesting series of chemical research is in some respects the most interesting of all. The injurious effects of mangold leaves in feeding young animals induced Professor Church to try and find the cause of this. The method of conducting the experiment is given in detail, and further researches we hope will be instituted; from the value already gained Mr. Church arrives at the determination, that 100 lbs. of fresh mangold leaves contained rather more than 4 ounces of the soluble acid oxalate of potassium; with the remark that in dry seasons this amount may be possibly increased. This must conclude the 1st part of our notice of these interesting chemical papers. They only require an attentive perusal for their great and full value to be immediately recognized.

Micro-Chemistry of Poisons, including their Physiological, Pathological and Legal Relations: Adapted to the use of the Medical Jurist, Physician, and General Chemist. By Theo. G. Wormley, M.D., Professor of Chemistry and Toxicology in Starling Medical College, and of Natural Sciences in Capital University, Columbus, Ohio. With Seventy-eight Illustrations upon Steel. New York: 1867. Baillière Bros.

The chief objects of Professor Wormley's book are stated by the author to be "to indicate the limit of the reactions of the different tests which have heretofore been proposed, as also of those now added, for the detection of the principal poisons, and to point out the fallacies attending the reaction of each; and, also, to apply the microscope, whenever practicable, in determining the nature of the different precipitates, sublimates, etc., and to illustrate these, whenever of practical utility, by drawings."

The plan of the book is therefore peculiar. It does not affect to be a complete treatise on toxicology, for the poisons described are few in number and are principally those which can be readily detected by chemical methods. The list is indeed so circumscribed that we look in vain for many highly important poisons. There is no mention of colchicum, cantharides, or croton oil: the poisonous gases are entirely omitted, and this is even the case with some substances, such as nitrobenzol,* cocculus indicus and the salts of barium, which can be detected by purely chemical methods. We cannot but regard these omissions as an abridgement of the utility of a work which is evidently intended rather for the professional man than the student.

Within its own limits, however, the book is excellent, and we fully admit that for common purposes the limits are quite wide enough. It is divided into two parts, the first devoted to inorganic, and the second to vegetable, poisons. The first part includes the alkalies, the mineral acids, oxalic and hydrocyanic acids (placed here for convenience), phosphorus, antimony, arsenic, mercury, lead, copper, and zinc. The second part is entirely confined to the poisonous alkaloids and the substances from which they are obtained. The following is the order of the chapters in which they are treated. 1. The volatile alkaloids of tobacco and hemlock. 2. The constituents of opium. 3. Nux vomica. 4. Monk's-hood, deadly nightshade, and stramonium (aconitine, atropine, and daturine, the latter of which is justly regarded as identical with atropine). 5. Hellebore and the different species of solanus (veratrine, solanine). It will perhaps give the best idea of the mode in which each poison is described if we select a single case, that of arsenic. First we find an account of the leading properties of arsenic and its principal compounds. Then follows a detailed account of arsenious acid, the symptoms produced by it, the quantity required to cause death, and the mode of treatment proper in cases of poisoning by it. Under this last head some curious experiments are quoted on the action of ferric hydrate as an antidote. The author of these experiments, Dr. Wm. Watt, found that dogs of average size were invariably killed by doses of from 3 to 6 grains of the acid. He then treated twelve more dogs with doses varying from 3 to 8 grains, and followed them up—in some instances immediately, in others when symptoms of poisoning commenced—with doses consisting of two tablespoonfuls of the moist hydrate. In every case the dog recovered without exhibiting severe symptoms. "In another case six grains of the poison in solution were mixed with about fifteen parts by weight of the antidote, and the mixture, after standing twenty minutes, given to a dog; no appreciable effect whatever was observed, although the animal was closely watched for many hours."

Next we come to the chemical properties of the acid. Original experiments are given on the solubility of the acid in water, which appear to indicate that the solubility both of the opaque and crystalline variety depends greatly upon the conditions under which the experiment has been made. A long and elaborate account of the special tests for the acid next follows, and here we notice one of the most valuable characteristics of the work. After the description of each important test, original experiments are given, showing the reaction obtainable with definite quantities of the materials, and thus affording a good and reliable measure of the delicacy of the test. We will give as an instance the experiments upon that form of Marsh's test in which the gas is decomposed in a tube by heat.

"1. 1-2,500th grain of arsenious acid in one hundred grains of liquid, or one part of the acid in the presence of 250,000 parts of fluid, yields in a very little time a very fine deposit, the inner portion of which has a brown colour, while the outer part has a bright metallic lustre.

"2. 1-5,000th grain, under a dilution of 500,000 parts of liquid, yields much the same results as 1.

"3. 1-10,000th grain, under a dilution of 1,000,000, yields a quite good deposit.

"4. 1-25,000th grain, under a dilution of 2,500,000, yields after some minutes, a very satisfactory deposit.

"5. 1-50,000th grain, in the presence of 5,000,000 parts of liquid, yields, after several minutes, a very distinct stain, the outer part of which has a dark metallic appearance, and the inner a brownish colour."

The author remarks that although Marsh's test will reveal the presence of arsenic in a greater state of dilution, Reinsch's test is really more delicate, because so much smaller a quantity can be employed. Used with the nicest care the latter test is capable of giving distinct evidence of the presence of 1-100,000th of a grain of the poison.

It is right to remark, in connection with Marsh's test, that the author has omitted to point out one of the chief difficulties attendant upon its use, namely, the frothing which, as every chemist knows, is generally produced in the presence of organic matter. He even directs us to estimate "the quantity of arsenious acid present in an organic liquid," . . "by introducing the solution into an active Marsh's apparatus, containing just sufficient sulphuric acid to evolve a very slow stream of gas," and to conduct the evolved gas into dilute nitrate of silver. The usual methods for the separation of the poison from organic matter are given, but they do not seem to present any points of novelty. Lastly, there is a useful paragraph entitled "Failure to Detect the Poison," in which the time required for its elimination is considered.

The other poisons are discussed in a manner similar to the above, and a good many useful and perhaps a few useless tests are added to those already employed; but our limits prevent us from giving further extracts. The account of the strychnine tests, their fallacies, and the substances which interfere with them, is particularly good. The substance most likely to be mistaken for strychnine appears to be woorara, the sulphuric acid solution of which gives a violet colour with potassic bichromate; but it may be distinguished by the circumstance that it gives a red or purple colour with sulphuric acid alone, whereas strychnine remains colourless.

We have not yet alluded to the chief characteristic of the work—the characteristic by which its name is justified. It is

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* A slight allusion to nitrobenzol occurs in page 561, where the author refers to Vol. v. of the Chemical News, in which Dr. Letheby's observations are recorded.

accompanied by an atlas of microscopic plates in illustration of the forms assumed by the principal precipitates and sublimates obtained in toxicological analysis. Not only have we the crystalline forms of the alkaloids and their compounds presented to us, but also those of many crystalline mineral precipitates, such as the potassic and sodic platino-chlorides, and even of some which, like baric and plumbic sulphates, and argentic arsenite and cyanide, are not generally recognised as having a crystalline character. These illustrations are executed on steel, and are marvellously beautiful, and, as far as we are able to judge, equally accurate. It is pleasant to learn from the preface that science owes them to the skilful hand of a lady. Professor Wormley may well dedicate the book to his wife, for it owes a very large portion of its usefulness and importance to her work.

It would be hardly fair to the publishers to conclude without remarking that the style in which the book is got up is exceedingly good, that type, paper, and binding are all first-rate, and might almost have excused the publication of a bad book.

The Bible and Science. An Address delivered at the Church Congress, Wolverhampton, Oct. 3, 1867. By William Allen Miller, M.D., LL.D., Treas. and V.P.R.S., Professor of Chemistry in King's College, London.

The old prejudice which existed in the minds of many good persons against the cultivation of science, that it tends to promote scepticism in religion, has not yet died out, and we are bound to admit that the published opinions of some scientific men have given it new life. Of course this is very unreasonable, for it would be as unwise to denounce language because it may be perverted, or money because it may be spent on unworthy objects, as to denounce science because some of its professors are sceptics.

Religion has become so much a matter of dogma, of sect, of party, of antagonistic opinions on comparatively unimportant matters, of literal interpretation, and so forth, that Christianity is liable to be lost under the multitude of its coverings.

Those who regard religion as too sacred for every-day use, look with a kind of horror on the proposition that science is as much the work of revelation, as the religion of faith. The same power that permitted or inspired the Prophets and Apostles of old to reveal the Divine will to man, has permitted and permits man to discover some of the hidden mysteries of the universe. It is indisputable that the laws by which material things are govered have the same Lawgiver as those which regulate the spiritual world. The law of inverse squares is not more wonderful or more easy to grasp in its origin, permanence, and constancy, than the scheme of man's redemption and future happiness: only in the one case men can see and appreciate the induction which led to the discovery of the law, while in the other the faith which converts the doctrine of his redemption into a living vital truth is not so easily commanded.

From time to time we get such evidence as is afforded by the pamphlet before us, that a man may occupy a high position in the scientific world, and yet be a good Christian. Such evidence as this is all the more valuable as coming from a layman; and although the essay was read at a Church congress, and the author belongs to a College which requires all her officers to be members of the Church of England, yet there is nothing churchy or sectarian about it. This will make it acceptable to a large body of cultivated men, who, disgusted at the wranglings and contentions, strifes, and heart-burnings of sects, and the present disorganised state of opinion in the Church of England, turn away from every church, and are disposed to cultivate rationalism rather than religion.

We attach the greatest importance to the non-sectarian tone of this essay; because it helps forward the grand idea that a churchman, or the member of any sect, is not necessarily a Christian, and that a Christian is not necessarily a sectarian.

CORRESPONDENCE.

Elections at the Chemical Society.

To the Editor of the Chemical News.

Sir,—As a member of the Committee whose report to the Council of the Chemical Society you published last week (*American Reprint, January*, 1868, *page* 44), and which has not been signed nor agreed to by me, I feel myself called on to enter a protest against the concluding sentence of that report. I was present at earlier meetings of the Committee, but not at the last meeting, nor at the meeting of Council which received the report. I have privately expressed to members of the Committee my disapproval of the last sentence of the report.

Having voted against the candidates whose rejection raised the question of the abrogation of the by-law, and being still of opinion that I and those gentlemen who voted with me were in the right in the course we took, I cannot submit to having the occasion used as an opportunity for tendering advice as to the line of action to be followed by the Society in the event of members making an improper use of the ballot-box. More than that, I could not recommend such an alteration of the by-law as is mentioned in the report, even in the event of a section of the Fellows banding themselves together to exclude a desirable candidate.

It appears to me, moreover, that the Society is in far more danger of falling helplessly into the hands of an official clique than of being deprived of the fellowship of valuable candidates through the combined action of private members. For my own part, I look rather upon a radical change in the manner of appointing the Council of the Chemical Society as the desideratum.

It is a notorious fact that the annual meeting for the election of officers and Council is, as a rule, poorly attended, and that the new Council is, as a matter of fact, elected by the votes of the retiring Council, whose individual members vote for that purpose in the capacity of private Fellows.

This state of matters, which is partly indicative of apathy on the part of the Society at large, and partly a consequence of the scattered state of the 500 members, is styled in official language a proof of the great confidence of the Society in its officers.

I regard it as a sign that election by vote does not answer as a mode of appointing the Council. In truth, at the present moment, the Council is not really delegated by the Society at large, but owes its present composition to a kind of apostolical succession.

Two courses present themselves to my mind as calculated to remedy this evil. The one is to deprive the Council of its votes in the election of its successor. Relieved of this depressing circumstance, the Society at large might perhaps be induced to take part in the election of a Council. The other course is to have the Council chosen by lot out of the whole 500 Fellows of the Society. Unpromising though this latter plan appears at first sight, I am inclined to think it would not work badly, and it would certainly deliver the Society from the kind of apostolic rule which I regard as one of the worst evils with which a Society can be afflicted.

I am, etc.,
J. Alfred Wanklyn.

London Institution, December 2, 1867.

Lecture Experiments.

To the Editor of the Chemical News.

Sir,—I should be glad to know if you would spare a column occasionally in your journal for an exchange of notes concerning lecture experiments. I am sure that to a large majority of the now numerous body of science teachers such a column would be highly useful. As a rule the conditions of success are not stated in our text-books when an experiment is described, and much time is lost in seeking them. Thus I have spent a considerable time in finding a neat and ready method of demonstrating the combustion of oxygen in hydrogen. No doubt all who have made the experiment have met with similar difficulties; but if there were a column such as I venture to suggest, the labours of one person would serve for all. As a contribution to such a column I would ask you to insert—

FLOATING SOAP BUBBLES IN CARBONIC ACID.

A vessel in which to hold the carbonic acid may conveniently and cheaply be made by getting five square pieces of glass—they should be at least 30 or 40 c. square,—then joining their edges by bibulous paper soaked in glue, so as to form a cubic shaped vessel. When the glue is dry a strip of cloth about two centimètres wide should be glued in the inside of the vessel, and lap over the edges, not only for protecting the ragged edges of the glass, but to prevent the bubbles from bursting. The carbonic acid used should be passed through a wash-bottle containing potassic carbonate, so as to free it from the vapour of hydrochloric acid, and then conducted into the cubic vessel until it is quite full. If a bubble now be blown with the glycerine and soap solution it may be floated easily on the carbonic acid. Should a draught carry it to the side of the vessel, the strip of cloth will cause it to rebound again to the centre, and thus the bubble may float for many seconds, or even for a minute or two.

I am, etc., C. J. Woodward.

Middlesex Institute, Birmingham, Dec. 11, 1867.

The Decline of English Manufactures.

To the Editor of the Chemical News.

Sir,—The relative decline of the industrial arts in England has been ascribed, with more or less probability, to a variety of causes. As far as chemical manufactures are concerned, "strikes" and trades' unions can have had no injurious influence. Neither can excessive wages, since at a chemical works labour bears a lower proportion to the gross returns than probably in any other branch.

As far as opportunity has enabled me to judge, the main root of the evil is the defective education both of managers and foremen, and of common workmen. That the Englishman is naturally inferior in intellect to the German or the Frenchman I deny, but his faculties are not systematically developed, and the school system of this country leaves him in such a state of ignorance, that if you speak to him of "science," he actually thinks you mean pugilism. To expect men thus dragged up to compete with such as have been trained in the schools of the leading continental nations, is as rational as to pit a regiment armed with the old flint-lock musket against one equipped with the needle-gun or the Chassepot. A fact mentioned some time back in the Chemical News throws a strong light on the inferiority of this country. The inspectorship of high schools in France was held till lately by the celebrated chemist Dumas. Any similar office in England would be committed not to a man of science, but to a clergyman, a briefless barrister, or a half-pay officer.

Men whose powers of observation have never been cultivated cannot execute the most simple process without mistakes. In proof of this I cite two facts which have lately come under my own observation. A man employed at a large dye-works was sent to the warehouse for a bucket of extract of quercitron-bark. He returned with his pail full of methylated alcohol!—a clear, colourless, transparent fluid, in place of one deep, brown, turbid, and opaque. Another, sent for archil paste, brought a cargo of grease used along with soda for cleansing the goods previous to being dyed. Both the men had worked for years at the establishment, and had had every opportunity of becoming familiar with the articles in question. Such men necessarily occasion their employer waste and loss incalculable. They are a perpetual stumbling-block in the way of the inventor, who is frequently told by practical men: "The process is all right in your hands, but how am I to get my careless men to work it?"

We can maintain supremacy in manufactures only on condition of supremacy in science; and if we are unable or unwilling to arrange our national system of education accordingly, we must be content to fall into the rear.—
I am, &c., W.

A lecture Experiment.

To the Editor of the Chemical News.

The presence of earthy or alkaline carbonates in spring or river water, etc., may readily be shown by adding a small quantity of a freshly prepared decoction of logwood, when a more or less deep purplish-red colour is produced. With distilled water only a slight reddish-yellow colour is observed. The experiment may conveniently be performed by adding an equal quantity of the logwood solution to the water or waters to be tested, contained in glass jars or beakers, placed on white paper, a similar jar of distilled water being used for comparison. The solubility of carbonate of lime in pure water, may be shown by placing some pulverised calc-spar on a properly purified filter and allowing distilled water to pass through it; on adding a few drops of the logwood solution to the filtrate a deep purplish red colour is immediately produced. An alcoholic solution of alizarine may be used instead of the logwood decoction; with distilled water a yellowish colour is produced, but if any alkali or alkaline earth be present a violet colour is developed, especially on application of a gentle heat. Of course in using the above tests to show earthy carbonates in water, care must be taken that no alkali is present. J. W. Y.

MISCELLANEOUS.

Royal Institution of Great Britain.—At the General monthly meeting, Monday, December 2, 1867. Sir Henry Holland, Bart., M.D., D.C.L., F.R.S., President, in the chair. George Willoughby, Esq., M.I.C.E., F.R.O.S., William Daniel Mitchell, Esq., and Morgan Bromsby Williams, Esq., M.I.C.E., were elected members of the Royal Institution.

Obituary.—We have to record the death of Dr. Thomas Clark, late Professor of Chemistry in Marischal College, Aberdeen: his decease occurred on the 27th November, at Clydeview. Dr. Clark's method of testing and purifying water are well known. The soap test devised by him has been used by chemists for twenty-five years without receiving modification or improvement in their hands. This process for softening water on a large scale is also much used at the present time, and was indeed mentioned by several fellows in a discussion at the last meeting of the Chemical Society. The subject of water seems to have been the one to which Dr. Clark specially devoted himself, and to which almost all his papers in scientific journals refer: his writings have a peculiar charm in them from the modesty with which he expresses himself. Though not a Fellow of the Royal Society, Dr Clark, had he lived, probably would have been made one at the next election, since his friends were circulating a certificate for signatures.

Utilisation of the Grey Barks.—Mr. Broughton, the quinologist, or quinine chemist, employed by Government at the Neilgherry plantations, has produced pure sulphate of quinine from the *Crispa* variety of the *Cinchona Officinalis.* From his analysis it appears that, although the grey barks do not contain quinine, they are among the richest in their yield of alkaloids. Mr. Broughton is conducting experiments for the determination of the best season for cropping the bark on a large scale, the various methods of drying the bark for exportation, the influence of soil, the physiology of the alkaloids, and the effects of mossing the barks. The question of a ready means of utilising the barks in the form of some simple preparation for therapeutical purposes in India, is also receiving his best attention. —*British Medical Journal.*

The late Professor M'Gauley.—We are requested to draw attention to the M'Gauley memorial fund (offices, 21 Cockspur Street, Charing Cross), which is now being raised on behalf of the widow and family of the late Professor M'Gauley, Editor of the *Scientific Review*,—a man whose literary and scientific attainments made him much respected by all who knew him. As he had no opportunity of realising more than a bare sufficiency for his immediate wants, his wife and four children are unfortunately left utterly unprovided for, and a fund is now being raised for their relief.

Singular Explosion.—Under this head the *Times* gives the following account of the circumstances, as elicited at the inquest, under which a fatal accident took place during the preparation of hydrogen:—"Mr. Laurence, 9, Little George-street, Greenwich, said that he was a house painter and decorator, and that he was employed to make the lime-light to be used during the performance at the Greenwich theatre. On Saturday morning, the 16th ult., he went into the workshop at the back of his house, and while he was in the act of making it an explosion of hydrogen gas took place. He had placed some old nails and some scraps of iron in a large glass vessel, and he then poured three pints of water into the vessel. After that he poured some sulphuric acid into the jar. There were three pints of water in the vessel, and when he had poured three to six ounces of the acid into it an explosion took place, and he was covered from head to foot. He then heard loud shrieks, and upon rushing out of the workshop he found that two of his daughters had been injured. The deceased had her throat cut, and blood was rushing from the wound. He placed her in a cab, and she was taken to Guy's Hospital. Witness made the hydrogen gas every other day in a bag of three feet nine cubic inches. That lasted two nights. He had studied chemistry for twenty-five years, and he believed that the cause of the explosion was the coldness of the preceding night acting upon an old solution of water, iron, and sulphuric acid that was in the glass jar at the time that he poured the fresh solution into it through a funnel. The old solution was crystallised. He was not injured." The explosion is peculiarly "singular," since as far as we can understand the circumstances as described by our contemporary, the explosion took place in one room while its effects were chiefly felt in another; and, moreover, no mention is made of any flame by which ignition could have been caused. If the vessel burst simply from an enormous pressure of gas, this could only have happened by the apertures being purposely tightly closed. We can scarcely imagine a man having "studied chemistry for twenty-five years" making hydrogen without using an acid funnel. If anything were wanted to complete the mystification, the explanation of the cause of the explosion, given by the student of twenty-five years' standing, is sufficient. The jury returned a verdict of "Accidental death," recommending with naïveté that there should be more caution used in future while hydrogen gas was being made; and the coroner finished by saying that "he did not think such a dangerous *compound* (!) ought to be made except in places properly allotted to it." This remarkable commentary shows that "Crowner's" Chemistry is as profound as "Crowner's" Law.

Edwards v. Norris.—In Chancery.—This case relates to the chemical works of the defendants at Sowerby-bridge, in the West Riding of Yorkshire. The bill prays "That the defendants, their agents, and workmen, may be restrained from discharging, or causing, or permitting to be discharged from their works at Sowerby-bridge, any vapour, or sulphuric acid, or sulphurous acid, or any other noxious or offensive gas, vapour, or substance, so as to occasion any nuisance or injury to the plaintiff's property called Pye Nest, or the property belonging to the plaintiff and his brother Joseph Priestly Edwards as tenants in common, or to the timber, or other shrubs or herbage on the same respectively." The second clause asks for an inquiry as to damage. Lord Romilly, in delivering judgment in the Rolls Court on December 4th, said:—"A vast amount of evidence has been gone into on both sides to prove and disprove the case of the plaintiff. The burden of proof lies on the plaintiff, and after a very long and detailed examination of the evidence on both sides, I am of opinion that the plaintiff has failed in giving the proof which is necessary to induce me to give him a decree. The evidence is so voluminous and so varying, that I should only become obscure, and fail in showing the true grounds of my decision, if I were to attempt to state in detail what, in my opinion, each witness establishes, and in what parts his evidence falls short of the required proof. But I will state, generally, what I consider to be proved by the evidence, and the points in which I think the evidence is detective. I think it is clearly proved that vegetation and trees in the plaintiff's park are seriously injured by noxious vapours. It is also, in my opinion, proved that the presence of sulphuric acid, or sulphurous acid, is prevalent on the leaves and grass in various places in the plaintiff's park. It is also proved that a very considerable amount of sulphurous acid escapes from the works of the defendants—as much as 14 ounces per minute from the cupola chimney, and 11 ounces per minute from the lofty chimney, and that this occurs although they are so constructed as to consume their own smoke, and this on the most approved method, and although no dense vapour escapes from either chimney. This goes a great way in the plaintiff's favour, but I think the evidence fails in showing that the injury to the trees and vegetation is attributable exclusively, or indeed mainly, to the defendants' works. The escape of sulphur from the other chimneys at Sowerby-bridge, according to the evidence before me, amounts to six times as much as that which escapes from the defendants' works; besides which, there are bleaching works where sulphur is burnt, and where, consequently, a considerable escape of sulphurous acid must take place; and although the evidence does not enable me to estimate accurately the amount which does so escape, it must, I think, be considerable. The defendants' works are also close to the gas works, from whence it appears that some escape of sulphuretted hydrogen takes place, and although the amount of it is not proved, yet some of it must occur. This evidence might be neutralised if it were established that the injury to the trees and vegetation took place in the immediate neighborhood of the defendants' chemical works and where the vapour from these works alone could extend. But this is not so. The injury to the trees is very capricious, and some which stand very near the works are not affected at all, while others, more distant, are very seriously injured. The attempt to establish that this is occasioned by accidental shelter is very far from producing conviction in my mind as to the truth of the cause alleged. That the defendants' works do contribute their quota of injury to the plaintiff I do not doubt, but I am by no means clear that if their works were put an end to to-morrow the plaintiff's park would benefit by it, or that it would not appear to be in a position just as injurious the next year as if their works had been carried on. I have in vain endeavoured to reconcile the evidence with any particular theory as to the place from whence the noxious vapour flows, and I believe it to be impossible to do so. I think the most probable solution of the case is that the whole atmosphere around is impregnated with substances more or less injurious to vegetable life, and that these substances affect some trees, and some places, more than others—not merely because the wind is principally in that direction, as is the case on the west side of the trees, of which some striking instances are shown, but also arising, in some measure, from the accidents of the soil, and other natural accidents, which render some trees more susceptible of this peculiar injury than others. At all events, after trying very much, I have failed in being able to trace distinctly the injury proceeding from the defendants' works as to justify me in granting any injunction. In addition to this failure on the part of the plaintiff, there is another circumstance which, in my opinion, tells much in support of the view that I have taken of the case on the question of the relief sought by this bill. The works of the defendants were established upwards of forty years ago—I think in 1819. Since then they have much increased, and particularly since 1857. But simultaneously with the increase, or nearly so, improvements have been made which, according to all the evidence, which on this part of the subject is all one way, have materially diminished the escape of noxious vapours. And this was particularly accomplished by alterations and improvements made in the year 1859. I have found it impossible to arrive, on the evidence, even at a distant approximation as to the amount of noxious vapours which escape from the defendants' works now as compared with those which escaped from them prior to the year 1859. For anything that appears, it may have been worse then than it is now; and if so, the question arises—When did the vapours first begin to be injurious to the neighbouring vegetation? I do not believe that it has ever been decided that prescription to foul a running stream, or to impregnate the air with noisome or noxious vapours, can be established against the public; but, unquestionably, it can be established against a particular individual who has seen it going on for upwards of twenty years, and has taken no steps to prevent it. It is incumbent, in such a case, that the suffering proprietor should seek the assistance of this court as soon as the nuisance is really perceptible, according to the doctrine laid down in many cases, and which I followed lately in the case of Goldsmid *v.* the Tunbridge Wells Commissioners. On this subject, in the present case, I am left in the dark. My surmise, drawn from the evidence, is—that if the present injury proceed from these works, as is alleged by this bill, this injury must have been perceptible more than twenty years ago, and as it is from the cupola chimney, which is said to be the main cause of the injury, and as this has been in the same situation since 1859, when it was made much less hurtful than it was before, the present state of the vegetation is more attributable to the general increase of the neighbouring manufactures than to these works themselves; and in my opinion, I am unable, as I am invited to do, to draw any safe conclusion or analogy from the effects produced by London smoke, or by that of our other large towns, even if I had, which I have not, any trustworthy evidence on the subject as applied to the effects of a mass of manufactures with smoking chimneys, contracted in a narrow valley, and as to which the vapours must be for the most part confined. I have therefore come to the conclusion that the plaintiff has not made out his case; and as the burden of proof rests on him, his bill must be dismissed. I had thought at one time of offering to him an issue! but, having regard to the lapse of time, which cannot be tried by an issue, and also considering that according to my experience in this court an issue entails great expense, and ultimately, for the most part, leaves the parties very much as they were before, I have determined not to adopt this course, but to dismiss the bill *simpliciter;* and therefore I dismiss the bill with costs."

Obituary.—MR. WARINGTON, F.R.S., the distinguished chemist, died on November 12th, at Budleigh, Salterton, Devon. So many benefits have been conferred upon chemists by Mr. Warington's zeal, that his decease will be a matter of general regret in the profession. His life seems to have been pre-eminently a life of public activity. He may be said to have founded the Chemical Society; upon its establishment he became secretary, a post he retained for many years. He also obtained the same responsible position in the Society which has rendered so much service to chemists in publishing a translation of Gmelin's Handbook—the Cavendish Society. These are not the only public positions in which he has figured; he was a juror of the Chemical Section of the International Exhibition of 1862, co-editor (Dr. Redwood being the other) of the British Pharmacopœia, 1867, besides having a hand in the production of some other works on pharmacy and allied matters. Mr. Warington has undertaken many researches. A vast number of papers were published by him on various subjects in chemistry and pharmacy; some of these are to be found in the *Philosophical Magazine*, but the majority in the *Chemical Gazette*. We append a list of some of the more important. On improvements in the operation of tanning; on refining gold; on the formation of Prussian blue on the surface of gravel; on the action of weak acids on vessels plated by the electrotype process; on the action of alkalies on wax; on chemical symbols; on a curious change in the molecular structure of silver; on molecular changes in solid bodies; on the green glass of commerce; on some properties of animal charcoal; on the preservation of animal and vegetable substances; on chromic acid; a process for estimating the value of the materials used in tanning; on the production of coloured films by electro-chemical influence and by heat; on a new yellow dyeing agent; on the production of boracic acid and ammonia by volcanic action; on biniodide of mercury; on the determination of phosphoric acid by magnesia. Mr. Warington held the office of chemical operator at the Society of Apothecaries for upwards of twenty years. He retired from the profession last year.

DR. DAUBENY.—With deep regret we announce the death of Dr. Daubeny, Professor of Botany, Oxford, whose contributions to the cause of science have often appeared in our columns. He was a son of the Rev. James Daubeny, Rector of Stratton, and was born in 1795. He was educated at Magdalen College, Oxford, where, in 1814, he took the degree of B.A., when he was second class in classics. In the following year he gained the Chancellor's prize for an essay. He afterwards obtained a lay fellowship at Magdalen, and applied himself to the study of medicine, taking the degree of M.D. In 1822 he was elected to the Professorship of Chemistry, and in March of the same year he was elected a Fellow of the Royal Society. In 1829 he entirely relinquished the practice of his profession; and devoted himself to the study of the physical sciences. During the year 1834 he was elected to the Professorship of Botany, and he was also curator of the Botanical Gardens at Oxford. In 1841 Dr. Daubeny became a fellow of the Chemical Society, of which at the time of his death he was Vice-President, having previously filled the office of President. Dr. Daubeny has written several works, among them may be noticed "A Description of Active and Extinct Volcanoes;" "An Introduction to the Atomic Theory;" "Lectures on Roman Agriculture," etc. Amongst his contributions to the Chemical Society may be mentioned a paper on the power possessed by the roots of plants of rejecting poisons, and also one on "Ozone," which embodied the results of an extensive series of experiments and meteorological observations made at Torquay and Oxford. He died at the Botanical Gardens, Oxford, on December 12.

Paraffin Lamps.—We learn from the *Norfolk* (U.S.A.) *Journal* that it has been found that the light of coal-oil lamps is greatly improved by adding to the oil one-fourth its weight of common salt. It makes the light much more brilliant and clear, keeps the wick clean, and prevents smoking.

A Well-deserved Honour.—The "International Societies for Aid to the Wounded in Time of War" have awarded to Mr. Condy, of Battersea, their medal, in recognition of the importance to military surgery of his discovery of the disinfecting properties of the alkaline permanganates, and the great sanitary value of Condy's fluid, as proved by the experience of the Prussian army surgeons during the late Bohemian war.—*Lancet.*

Faraday as a Discoverer.—Under this title Messrs. Longman & Co. announce a memoir of the late Professor

Faraday, by Dr. Tyndall. It is to be in one volume, crown, 8vo, and will be published in January.

Storage of Nitroglycerine.—Another disastrous accident has happened with this recently introduced blasting oil. Unless means are taken by the manufacturers to prevent explosions causing such lamentable results as that which occurred at Newcastle on Tuesday, a valuable blasting agent will be lost to miners and quarriers. If this be the case, however, the manufacturers of it will have themselves to blame, for explosions of nitroglycerine during transport or storage ought to be unknown. Nitroglycerine dissolved in two or three times its bulk of methylated spirit is quite inexplosive, and when required for use, the addition of water will precipitate the oil, the layer of water and spirit merely requiring decanting off. The nitroglycerine separated in this way possesses explosive properties quite as active as the original oil, which indeed is frequently rather improved than otherwise by the treatment. The process we have described is sometimes used. At Newcastle a part of one canister was found after the explosion marked "safety solution of nitroglycerine in wood naphtha." It is, however, quite certain that there must have been a quantity of oil present either entirely untreated, or treated with an insufficient quantity of the protecting fluid. It should be remembered that nitroglycerine dissolved in a small quantity of methylated spirit or of wood naphtha in warm weather might crystallise out in winter when the temperature approaches the freezing point of water. Probably this is what occurred. Shipping agents and railway companies should refuse to receive nitroglycerine unless protected in the manner already indicated.

Dinner of the Scientific and Manufacturing Chemists of Glasgow.—On Thursday evening, about fifty gentlemen connected with this large and important branch of industry in Glasgow and neighbourhood, met at dinner in the Victoria Hotel. The meeting was intended as a preliminary to the formation of a Chemical Society in that city. Mr. E. C. C. Stanford, F.C.S., presided, and Mr. Whitelaw discharged the duties of croupier. Among the company we noticed Professor Anderson, F.R.S., Dr. Wallace, F.C.S., Mr. Leisler, Mr. Townsend, Mr. J. J. Turnbull, Mr. J. N. Cuthbertson, Mr. Galbraith, Mr. Middleton, and others. Mr. Stanford read a note from Professor Penny, stating that he had shortly before received a telegram calling him to Edinburgh, to visit a friend seriously ill, and expressing great regret at his unavoidable absence. After the usual loyal and patriotic toasts, the Chairman, after some introductory remarks, said—The tendency of chemistry appears to be synthetical. The chemist used to be described as a man who could pull anything to pieces and put nothing together—who was troubled with a large bump of destructiveness and a small one of constructiveness. Now the time appears less distant when we shall build up our organic compounds from their inorganic constituents; manufacturing chemistry is following—we are turning to the earth already for many materials which were formerly organic. Some of these changes will at once occur to you. Our sources of potash have been suddenly and enormously aided by the discovery of a large evaporated bed at Strasfurt; and that mine, so admirably worked by Mr. Townsend, has reduced the price of potash to a third its former value. Liebig accused us in passionate language of "turning up the battlefields of Leipsic, Waterloo, and the Crimea for bones; from the catacombs of Sicily we carried away the skeletons of many generations." Speaking of England in 1863, he says—"Annually she removes from the shores of other countries to her own the manurial equivalent of 3½ millions of men, which she squanders down her sewers to the sea. Like a vampire she hangs upon the neck of Europe, and sucks the heart blood from nations." Almost while he was writing this we discovered coprolites in England, and exported phosphates to Germany; and now most of our phosphates are mineral. Guano will doubtless also give way to salts of ammonia and nitrate of soda from the earth. The time is probably not far distant when our large Cinchona plantations will be rendered useless by the introduction of artificial quinine. And a change may come over our Turkey-red makers by the production of artificial guarancin. Does any one think this visionary? Let me remind him that not long ago we imported nearly all our dyes, and now we are the largest colour exporters. Our dyes are dug from the earth. Can anything be more wonderful than extracting from coal the sun's light of a bygone age, and splitting it up into all the colours of the rainbow? or, as in Mr. Young's discovory, using it in the form of paraffin once more to light our rooms? What would have been said of any one ten years ago who spoke of analysing the atmosphere of the sun? We must be prepared for startling discoveries in this glorious science. Whatever I have said generally of manufacturers applies more particularly to Glasgow. This city has great reason to be proud of its chemical factories—nearly every known branch is here represented. Long before the stranger who approaches Glasgow sees the flames of her forges, or hears the sound of her hammers, his attention must be arrested by her tall chimney shafts; the masts to which her chemical flag is nailed, and her manufacturers' challenge held high before the world. If a factory can be measured by its height, one of these stands distinguished and pre-eminent. Humboldt called chemistry "the Egyptian art," and unless it should return to that country, and one of the pyramids be converted so that it "draws" even better than at present our friend Mr. Townsend will still reign without a rival, and never be able to compete with any one his own size. Glasgow is no less distinguished for its scientific chemists—Thompson, Ure, and a long list of names form a brilliant scroll, Here, then, of all cities, the scientific stranger will expect to find one of the best chemical societies in the kingdom. Gentlemen, you and I know how much he would be disappointed. We are all to blame, but let us now make a move. This is an opportune time. The Government is at last awakened to the necessity of scientific education. The British Association, the Society of Arts, the Paris Exhibition, have all joined in one cry, which must be heard—scientific education for our people. Let us commence by forming a section of the Philosophical Society, where chemical subjects can be brought forward in a crude state and discussed. Scientific chemists and manufacturers are both required to work out some purely scientific subjects from which they would derive mutual profit. Let it not be said that the birth-place of Thompson, Graham, and Stenhouse cannot keep up a chemical society. If we do not make an effort now, the chemical world will move on, and we shall be left behind." The following toasts were also given:—"Scientific Chemists," by the croupier, responded to by Professor Anderson; "Manufacturing Chemists," by Dr. Wallace, responded to by Mr. Galbraith; "Foreign Chemists," by Mr. Sutherland, responded to by Dr. Mertz; "Chemical Brokers," by Mr. Turnbull, responded to by Mr. Leisler. Several other toasts followed, and a very agreeable evening was spent.

Science as a part of Education.—The following is the concluding passage of the last of a course of chemical lectures recently delivered at Eaton College by Mr. G. F. Rodwell:—"In conclusion, let us consider the nature of the various processes which we have studied. Let us enquire by what means we have been enabled to produce the different chemical changes which have been brought before us. These changes have been effected by inducing in matter unnatural and forced conditions; by influencing substances either externally or internally by superadded actions; or by the removal of a prevailing force which prevented the prominent assertion of chemical affinity. "*Occulta naturæ*," says Francis Bacon, "*magis se produnt per vexationes artium, quam cum cursu suo meant.*" From the beginning to the end of this course we have extracted our knowledge by thus harassing and vexing nature. The tearing asunder of combined atoms; the separation of atoms from one form of combination, and the compelling them to unite otherwise; the addition or subtraction of vibrating motion. These are the actions which have enabled us to gain some insight into the working of the force called chemical affinity. Of the actual nature of chemical affinity we know nothing; we can only regard it as an attractive force exerted between dissimilar atoms, acting through an insensible space, and varying in intensity as the atoms vary in composition. Chemical affinity can only be studied through its actions, and the greater number of the processes which we have employed have been for the purpose of eliminating the action of certain forces so as to cause chemical affinity to be the dominant force during the continuance of the experiment. These studies are to be viewed as a means to an end. The primary object of science teaching is not to make you acquainted with its applications to the useful purposes of life, but to induce in you that exact and discriminative mode of thought which is inseparable from the right study of physical phenomena. Why I say that to thoroughly master the vibratory motion theory—theory though it be—as applied to the explanation of chemical phenomena, is better as a mental exercise, than the knowledge of fifty of the applications of chemistry. Do not let your science be of too practical a character. Remember that science applied to the useful arts—to the making of dyes, the extraction of metals, the manufacture of coal-gas—has ceased to be pure science. It is not such science as this that I would have you study. True science must have as its primary objects the search for truth, the investigation of causes, the enlargement of our knowledge of the various agencies which are at work around us. It is to be built up mainly of well-verified experimental facts and of theories readily deducible from them. The former are to be received as absolute; the latter as liable to change. That such change is inevitable will be obvious when we remember that in the present state of science we are all—both those who theorise and those who work out the facts upon which theory is supported—all alike are boys and listeners, and learners in the school of nature.

Counterfeit Creosote.—A large proportion of ordinary creosote is simply carbolic acid. But the pure creosote, which constitutes the lachrymosal property and peculiar smell of smoke, is quite a different substance, and may be distinguished from the false, as shown by Rust, by its behaviour with collodion. A mixture with this latter and carbolic acid gives a gelatinous precipitate, while with true creosote the collodion remains clear. Dr. Hager gives another test. To a weak solution of iron, a few drops of ammonia are added until the precipitate, which originally forms, is dissolved. Carbolic acid communicates a blue or violet tinge to the solution, while genuine creosote gives a green colour, afterward turning to brown.—*Scientific American.*

Desiccated Egg.—Mr. Charles Lamont has discovered and patented a very ingenious process for preparing eggs so that they may be kept for years without change or decay. The process consists in emptying the fresh eggs from the shell into a long covered trough; a shaft, armed with a series of metallic discs about 15 or 20 inches in diameter, is made to descend into this trough while revolving, which beats the eggs into homogeneousness, and covers the surfaces of the discs with a thin covering of egg. The discs still revolving are elevated from the trough, and a current of hot air passed through the covered box, which quickly dries the egg, when a series of scrapers are brought into action so as to scrape off the egg in the form of fine thin scales or granules, which have the appearance of being crystallised. This process may be repeated *ad libitum*. The preparation thus obtained retains, perfectly, all the properties and flavour of the fresh egg, and may be used for the various purposes where broken egg is needed, and for cooking, by dissolving a little in water and beating it as usual. One pound is equal to 44 eggs; 100 doz. eggs, when crystallised or desiccated, occupy one cubic foot. We are glad that this very useful article of diet has been added to the now long list of preserved articles of food. An enterprising company in New York have, we understand, purchased the invention, and it is now being successfully introduced into the market.

Iodine and Carbolic Acid.—Dr. Percy Boulton, to remedy the inconvenience attending the external application of iodine and its preparations, has adopted the method of adding a few drops of carbolic acid to the iodine solution to be employed. The formula is as follows: Compound tincture of iodine, 3 grms.; pure liquid carbolic acid, 6 drops; glycerine, 30 grms.; distilled water, 150 grms. This carbolate of iodine is not perfectly colourless, so that it may be applied with impunity; and it is not only one of the most powerful antiseptics we possess, but is intrinsically a more efficacious agent than iodine alone. In the form of injections, gargles, and lotions, for sore-throat, ozœna, abscess in the ear, etc., this preparation is a sovereign remedy.—*Extract from a letter in the Journal des Connaissance Médicales.*

Royal Polytechnic Institution.—The Christmas entertainments at this Institute were inaugurated on Saturday evening last with a grand dinner, followed by a conversazione. The company at dinner, included the following noblemen and gentlemen:—His Highness Prince the Maharajah Duleep Singh; his Grace the Duke of Wellington, K.G.; Viscount Strangford; the Rt. Hon. Lord Ernest Bruce; Capt. the Hon. F. Maude; Hon. A. Kinnaird, M.P.; Professor Wheatstone, F.R.S.; Professor Abel, F.R.S.; Erasmus Wilson, F.R.S.; Professor Pepper; Dr. Letheby; Wm. Crookes, F.R.S.; Robert Hunt, F.R.S., Rev.J.B. Owen,M.A.; Professor Graham,F.R.S.; J.Glaisher,Esq., J.Spiller,Esq.,etc. The conversazione was very numerously attended, and the varied entertainments for the holiday season were rehearsed. Professor Pepper delivered a lecture on "Faraday's discoveries and their results, being real science as contrasted with unreal science called spiritual manifestations." Commencing with the discovery of bicarbide of hydrogen, the lecturer spoke of Dr. Hofmann's researches on benzol, which had been the means of producing the colours mauve and magenta since manufactured on a very large scale by the firm of Nicholson and Maule. He then dwelt upon Faraday's discoveries in electricity, explaining the construction of Wheatstone's bridge, etc. Mr. Apps also exhibited his new induction coil, with which a spark about four inches in length was obtained and other brilliant experiments performed. The lecture was closed with some extracts and observations on spiritualism, when Professor Pepper stated that Dr. Bence Jones had lent him a manuscript, which he had received from some one who professed to have held communication with Faraday's spirit, and that the latter was now a firm believer in spiritual manifestations, and regretted he had not taught the truth when he was alive. During the evening the Polytechnic was placed in telegraphic communication with the United States, Paris, Manchester, etc., and the visitors were amused by receiving the following message from Newfoundland two or three minutes after its transmission:—"Temperature 19° Fahr., wind, N.E., snow drifts in some places nearly 20 ft. deep." Mr. Ladd's dynamo-electric machine, machine-made jewellery, and various other ingenious novelties are introduced into the programme. We have no doubt the Institution will be well attended during the holidays; the programme is a very long one, and many other novelties are promised.

To Cement Brass on Glass.—Puscher uses a cement particularly adapted for fastening brass on glass lamps, which consists of a resin soap—made by boiling three parts of resin with one part of caustic soda and five parts of water—which is mixed with one-half its weight of plaster-of-Paris. This cement has great adhesive power and is not permeable by petroleum; it sets firmly in less than an hour, and is a very slow conductor of heat. Zinc-white, white-lead, or precipitated chalk may be substituted for plaster-of-Paris, but the material will be longer in hardening.—*American Artisan.*

Microscopic Crystallography.—Mr. H. S. Waddington, in a paper read before the Pharmaceutical Society, and published in the *Journal*, says that the formation of perfect crystals depends upon the rapidity with which they are deposited. He has obtained better results by allowing the crystals to deposit from a hot and concentrated solution, than placing a few drops of a cold saturated solution on a clean slide, and allowing it to evaporate spontaneously. When crystals are pretty soluble in water, the way of procedure is as follows:—A solution is made in hot distilled water, the liquid filtered, and a few drops poured on to a clean slide, just before the crystals begin to form in the solution itself, and immediately poured off, sufficient will remain behind for the production of crystals, which will form at once. When of a sufficient size, the remaining liquor, if any, should be drained from them and the slide allowed to dry. The result will generally be a slide, evenly covered with crystals, having well-defined edges, and but few of which are agglomerated. This process answers well for alum, chlorate of potassium, nitrates of barium and strontium, potassio-tartrate of antimony, sulphate of copper, sulphate, acid tartrate, binoxalate, and quadroxalate of potassium, the strength being regulated by experience. If crystals are not very soluble in cold water, they may be allowed to separate in the bulk of the solution itself as it cools, then remove a small quantity of liquid and crystals to a slide by means of a glass tube. The slide must be kept moving to prevent the aggregation of the crystals, and the superfluous liquid removed by applying blotting paper to the edges of the slide. To obtain perfect crystals from substances generally met with in long prisms, Mr. Waddington finds the best method is to make a hot solution, containing rather more of the salt than would saturate it at ordinary temperatures. Having filtered and allowed it to become nearly cold, place a few drops on a slide and draw a very fine glass rod across it. This method overcomes the difficulty of producing typical crystals. For hippuric acid, the solution, when on the point of crystallising, should be poured on to a cold slide, and when the crystals have formed, the remaining liquid should be poured off and the slide allowed to dry. Sugar, citric and tartaric acids, and all substances very soluble in water, may be obtained in crystals by making a concentrated solution, filtering it, and then pouring it on to a slide, taking care that only a thin layer of liquid remains, which should be allowed to dry in the air. To obtain crystals from sulphate of iodo-quinine or "Herapathite," the author mixes 3 drachms of spirit of wine and 1 drachm of acetic acid in which he dissolves 10 grains of bisulphate of quinine. He then pours 10 or 15 drops on to a slide and adds a drop of tincture of iodine. When clear he pours it from slide to slide as long as the liquid holds out. The best method of obtaining uric acid in crystals is to allow 8 or 10 oz. of urine to stand some hours after the addition of 2 or 3 drachms of acetic acid. In a day or two the crystals will have grown larger, when the bottle should be shaken to detach them from the sides, and then wash them with distilled water, acidulated with acetic acid. To obtain the rarer forms it is requisite to allow the crystals to deposit quickly, which may be done by making a solution of urate of sodium by boiling uric acid with solution of caustic soda until no more is taken up. If 1 or 2 drachms of this is put into 8 oz. of urine and a small quantity of acetic acid added, not more than sufficient to neutralize the soda, very perfect crystals will be obtained. Another deposit found in urine is the phosphate of ammonium and magnesium, or triple-phosphate, which may be prepared in prisms by dropping about 25 or 30 grs. of carbonate of ammonium into 8 or 10 oz. of urine, and allowing it to remain quiet for some hours. When the crystals are of sufficient size the bottle may be gently shaken and the urine poured off. This deposit may also be obtained in stellate crystals by adding 1½ to 2 drachms of carbonate of ammonium to urine, and allowing it to stand. The crystals should be washed with distilled water, to which a little liquor ammoniæ has been added. Calcic oxalate may be obtained by dropping a single small crystal of oxalic acid into 8 or 10 oz. of urine, and leaving it at perfect rest for some hours. Mr. Waddington has also obtained good results from salicin by pouring a saturated solution in cold water on to a slide, holding it over a flame until it is at the boiling point; then pouring off the slide, when only a viscid film will remain. This must become quite cold, and the under surface held close to the flame of a lamp or gas jet. The moment it begins to crystallise it must be removed a few inches from the flame, or else it will fuse.

Crystallisations Produced by Means of the Blowpipe.—It sometimes happens in experiments with the blowpipe, when borax, phosphorus, salt or soda is used, that the bead, at first limpid, becomes suddenly opaque. M. G. Rose finds that this is due to the development of crystallised bodies in the interior of the mass. The crystallisation is often confused, although sometimes it is very regular, and on operating with titanium under sufficiently varying circumstances, M. Rose has been able to obtain anatase, and to effect the crystallisation of the two allotropic states of the titanic acid. With felspar and phosphorus salt (by the aid of which, as is well known, silicates are reduced to silica and phosphates) he obtained crystallised quartz, confused, but insoluble in alkalies. In order to recognise the crystals obtained under these conditions, flatten the yet warm bead and observe it under a microscope, or it may be attacked by water or an acid, in which case the residual crystals may be collected on a glass plate.

New Test for Molybdenum.—In a former paper I stated that under certain circumstances sulphocyanide of potassium produced a magnificent red colour with molybdic acid. I have since thought to use this reaction as a test for the presence of molybdenum, and I find it extremely delicate, far more so than any other test yet employed.

Thus, molybdic acid dissolved in hydrochloric acid, and treated with sulphocyanide of potassium, gave the following results, when progressively diluted and shaken up with scraps of zinc.

Degree of Dilution.	Degree of Coloration.
One part molybdic acid in .. 50,000	Dark.
" " " " .. 150,000	Pale.
" " " " .. 300,000	Very pale.
" " " " .. 600,000	Faint.
" " " " .. 1,000,000	Colour perceptible in small bulk.
" " " " .. 2,000,000	Colour perceptible only in large bulk.

The presence of sesquioxide of iron does not interfere with this test if time is allowed for its deoxidation.

Tungstic acid similarly treated also gave a magnificent red or yellow colour, which however is not superior as a colour test to that at present used.—*William Skey, New Zealand.*

Sulphocyanide of Chromium.—When a mixture of bichromate of potash and sulphocyanide of potassium is treated with hydrochloric acid, the liquid acquires a reddish purple colour, and on agitating it with ether, a red sulphocyanide of chromium is dissolved thereby, while the aqueous solution acquires a pure green colour. If the action of the sulphocyanide is further prolonged, the original purple red colour passes into green, and ether refuses to extract any of the chromium. It has since been found that by using a limited quantity of the sulphocyanide only the red colour is produced, and ether then extracts the whole of the chromium salt. From these circumstances it appears likely that the pleochroism exhibited by the solutions of various chromium salts is due to the presence of two different oxides of chromium, the green sesquioxide and a higher oxide. A double sulphocyanide of potassium and chromium appears to form when a solution of bichromate of potash is evaporated at a very gentle heat, with a small quantity of a sulphocyanide in hydrochloric acid. It falls in granular ruby coloured crystals.—*William Skey, New Zealand.*

NOTICE.

With this number the present volume closes, and we have pleasure in informing our readers that in our next number, commencing volume XVII., *will be published a verbatim report of the first of Professor Tyndall's Christmas Lectures at the Royal Institution, "On Heat and Cold." Other new features will also be gradually introduced into our pages during the ensuing year.*

CONTEMPORARY SCIENTIFIC PRESS.

[Under this heading it is intended to give the titles of all the chemical papers which are published in the principal scientific periodicals of the Continent. Articles which are merely reprints or abstracts of papers already noticed will be omitted. Abstracts of the more important papers here announced will appear in future numbers of the Chemical News.]

Comptes Rendus. September 2, 1867.

Dumas: "*Obituary Notice of Professor Faraday.*"—Chevreul: "*Obituary Notice of Professor Faraday.*"—Chasles: "*Answer to Faugere's Remarks on the Authenticity of the Pascal Correspondance.*"—Secchi: "*On a Self-recording Meteorological Apparatus in the Champ Mars.*"—"*On the Shooting Stars observed on the 10th of August, 1867.*"—"*On a Stellar Spectroscope.*"—A. W. Hofmann: "*On a new Class of Acids homologous with Hydrocyanic Acid.*"—Burdin: "*On the Use of Hot Air instead of Steam as a Motive Power.*"—P. Desains: "*Researches on the Absorption of Obscure Heat.*"—A. Oppenheim: "*New Researches on the Isomerism of Protochloride of Allyl and Monochlorinated Propylene.*"—A. Gautier: "*On Chloride of Hydrocyanic Acid.*"—M. Simpson and A. Gautier: "*On the direct Combination of Aldehyde and Hydrocyanic Acid.*"—J. Y. Buchanan: "*On some Derivatives of Isethionic Acid.*"—Phipson: "*On the Presence of Columbite in Wolfram.*"—A. Baudrimont: "*On the Chemical Composition of the various Kinds of Guano imported to Bordeaux during the last Twelve Years.*"

September 9.

Chasles: "*Answer to some further remarks by Faugere on the Authenticity of the Pascal Correspondence.*"—A. Secchi: "*On the History of the Static Barometer.*"—A. W. Hofmann: "*On a new Series of Homologues of Hydrocyanic Acid.*"—Faugere: "*Some further Remarks on the Authenticity of the Pascal Correspondence.*"—J. M. Gaugain: "*On the Polarisation of the Electrodes of the Voltaic Battery.*"—Berthelot: "*On some Hydrocarbons contained in Coal Tar, Styrolene, Cymene, Hydride of Naphthaline, etc.*"—A. Gautier: "*On a new Series of Isomers of the Fatty Hydrocyanic Ethers.*"—"*On a new Base derived from Hydrocyanic Acid.*"

Monatsbericht der Königlich-Preussischen Akademie der Wissenschaften zu Berlin. June, 1867.

Poggendorff: "*On the motion of Quicksilver in Glass Tubes under the Influence of the Electric Current.*"

Sitzungsberichte der Kaiserlichen Akademie der Wissenschaften zu Wien. (*Mathematische-naturwissenschaftliche Classe.*) March, 1867.

G. Tschermak: "*On Glaucodote and Danaite.*"

Bulletin de la Société Chimique de Paris. May, 1867.

J. Klob: "*On the Theory of the Manufacture of Soda by Leblanc's Process.*"—Louguinine and Lippmann: "*On the Preparation of Cymene by treating Camphor with Perchloride of Phosphorus.*"—P. Alexeyeff: "*On Crystallised Nitrotoluol.*"—E. Grimaux: "*On the Constitution of Benzoine, Hydrobenzoine, and some Allied Substances.*"

June.

E. Monier: "*An Improved Hair Hygroscope.*"—Phipson: "*On a Method of Detecting the Presence of Iodine and Bromine in the same Solution.*"—Lecoq de Boisbaudran: "*A new Method of Estimating Copper.*"—C. Friedel and A. Ladenburg: "*On a Silicic Mercaptan.*"—E. Grimaux: "*On the Brominated Derivatives of Gallic Acid.*"—J. J. Chydenius: "*On Hexylene-Pseudo-Urea.*"—F. Sestini: "*On Wax of coccus carica.*"—Mallet: "*On the Manufacture of Chlorine and Oxygen from Subchloride of Copper.*"—Anthoine and Genoud: "*An improved Iridescent Glass for Porcelain.*"—"*A method of Producing Designs on Agate.*"—Paraf-Javal: "*On the Transformation of Liquid into Solid Fatty Acids.*"—Fernod: "*A Method of treating Garancin.*"

Journal des Fabricants de Papier. August 1, 1867.

F. Bourdilliat: "*On Testing the Chemical Products used in Paper Making.* (*Continuation.*) *Prussian Blue.*" Z. Orioli and Henry: "*An Agitator for Preparing Solutions of Chloride of Lime.* C. Orts-Vandenberghen: "*A Process for Rendering Canvas, Tissues, Paper, Pasteboard, etc., waterproof.*"—A Swan: "*An Improved Apparatus for Evaporating and Recovering Spent Lyes.*"—B. C. Tilghman: "*An Improved Method of Treating Vegetable Substances for the Manufacture of Paper Pulp.*"

August 15.

E. Bourdilliat: "*On Testing the Chemical Products used in Paper Making.* (*Continuation.*) *Acetate of Iron, Sulphate of Copper, Acetate of Copper.*"—J. L. Brebant: "*On the Use of Waste Fabrics for the Manufacture of Pasteboard.*"

Archives des Sciences. August 25, 1867.

C. Marignac: "*On the Separation of Niobic Acid from Titanic Acid, and on an Analysis of Æschynite.*"

Kunst und Gewerbeblatt. July, 1867.

F. Moigno: "*On an Animal Charcoal Filter on H. Danchell's principle, manufactured by the London Water Purifying Company.*"—T. Wegler: "*On Disinfection.*"—"*On the Manufacture of Plastic Charcoal.*"—Hailer: "*On the Impurities of the Coals of Upper Bavaria.*"—R. Wagner: "*On the Detection of Cotton in Silk Yarn and Fabrics.*"—"*On Mandarin Yellow, a new Colouring Matter prepared from the Refuse of Cider Making.*"—E. Dietrich: "*Note on the Preparation of Indigo Carmine.*"

L'Invention. September, 1867.

J. Green: "*On the Disinfection of Petroleum.*"—Salvet: "*On Brianchon's Pearl Glass for Glass and Porcelain.*"

Journal für Praktische Chemie. August, 1867.

C. Marignac: "*On R. Hermann's Researches on the Atomic Weight of Tantalium, Niobium, and Ilmenium.*"—A. Kenngott: "*On the Alkaline Reaction of some Minerals.*"

Bulletin de la Société d'Encouragement. July, 1867.

F. C. Calvert: "*On Phenic Acid and its Properties.*"—Tessie Du Mothay: "*A Method of Preparing Oxygen and Ozone from the Alkaline Permanganates.*"—"*On the Preparation of Oxygenised Water.*"—S. Hunt: "*On some new methods of Smelting Ores of Use in New England.*"—Ballard: "*On the Use of Silicic Acid for the Manufacture of Soap, and for other Purposes.*"—Payen: "*On the Manufacture of German Yeast.*"—Dumas: "*On the Silkworm Disease.*"—Pasteur: "*On the Silkworm Disease.*"—Lenoir: "*An Improved Writing Telegraph.*"—Tresca: "*On the Advantages of Armstrong's Accumulator for Hydraulic Apparatus.*"

Comptes Rendus. September 23, 1867.

F. Lucas: "*On the Limits of Visibility of the Electric and other Light.*"—J. L. Soret: "*On the Intensity of Solar Radiation.*"—J. Kolb: "*Researches on Chloride of Lime.*"

Bulletin de la Société Chimique de Paris. July, 1867.

Lecoq de Boisbaudran: "*Some Experiments on Supersaturation.*" C. Lauth and A. Oppenheim. "*On the Action of the Chlorides of Terebenthine on Aniline and Rosaniline.*"—P. P. Dehérain: "*On the Use of Potash Salts as Manure.*"

August.

Lecoq de Boisbaudran: "*On the Supersaturation of Saline Solutions.*"—G. Lemoine: "*On the Transformation of Red Phosphorus into Common Phosphorus.*"—Bourgoin: "*On Organic Radicals.*"—P. P. Dehérain: "*On the Use of Potash Salts as Manure.*"

PATENTS.

Communicated by Mr. Vaughan, F.C.S., Patent Agent, 54, Chancery Lane, W. C.

GRANTS OF PROVISIONAL PROTECTION FOR SIX MONTHS.

3129. H. A. Bonneville, Rue du Mont Thabor, Paris, "Improvements in the method or means for preserving pasty matters and substances, and apparatus therefor."—A communication from E. Charton, Rue Lepic, Paris.—Petition recorded November 6, 1567.

3208. A. F. Galdan, Nimes, Gard, France, "Improvements in compressed or artificial fuel."—Partly a communication from L. Treagot, Nimes, Gard, France.

3219. E. Madge, Swansea, "Improvements in the mode of and apparatus for the reduction of sulphate of iron crystals."—Partly a communication from C. Madge, Carrizal Bajo, Chili.—November 13, 1867.

3226. W. H. Richardson, Glasgow, N.B., "Certain improvements in the manufacture of iron and steel, and in the means or apparatus for effecting the same."

3232. J. Clark, Ph.D., and A. Esilman, Glasgow, N.B., "Improvements in decomposing the sulphides of copper, silver, nickel, cobalt, lead, barium, strontium, and calcium, and in obtaining copper, sulphur, and other products."—November 14, 1867.

3233. R. G. Harcourt, Birmingham, "Improvements in the manufacture and composition of fire lighting material."

3243. A. M. Clark, Chancery Lane, "Improvements in refining copper."—A communication from F. Le Clerc, M.D., Boulevart, St. Martin, Paris.

3244. J. Templeman, Glasgow, N.B., "Improvements in the manufacture of fire-lighters, applicable also for temporary fires or heaters, and in the means or apparatus employed therein."—November 18, 1867.

3248. J. Swindells, Kegworth, Leicestershire, "Improvements in the process of, and in apparatus employed in treating and separating minerals, earths, and other substances when ground or pulverized."

3269. J. G. Tongue, Southampton Buildings, Chancery Lane, "Improvements in the process and apparatus employed for ageing and refining wines, alcohols, spirits, and other liquors."—A communication from R. D. Turner, New York, U.S.A.—November 16, 1867.

3264. C. E. Brooman, Fleet Street, London, "A new or improved process of, and apparatus or furnaces for the manufacture of metal direct from the ore."—A communication from P. E. Martin, Paris.

3270. G. Fitt, Limehouse, Middlesex, "Improvements in the manufacture of artificial manure."—November 18, 1867.

3282. W. H. Richardson, Glasgow, N.B., "Certain improvements in the manufacture of iron and steel."—November 20, 1867.

3191. F. L. de Gerbeth, Haggerstone, Middlesex, "Improvements in treating oils and spirits, and in apparatus to be used for this purpose."—Petition recorded November 11, 1867.

3275. W. J. Coleman, Bury St. Edmonds, Suffolk, and A Coleman, Lombard Street, London, "Improvements in the combination and mode

[English Edition, Vol. XVI., No. 418, page 294; No. 420, pages 316, 317; No. 418, page 293; No. 420, page 318.]

of treating and employing certain preparations from various articles of food."—November 19, 1867.

3288. Baroness C. de Lavenant, Brixton Road, Surrey, "Improvements in coating metals and metallic articles for the purpose of protecting or preserving the same from oxidation and decay; also in the materials, machinery, and apparatus to be employed therein."—November 20, 1867.

3295. J. Townsend, Glasgow, N.B., "Improvements in the manufacture of soda and potash"—November 21, 1867.

3307. T. Burchell, Charlmonte Avenue, Kingston, County of Dublin, Ireland, "A new and improved process and machinery for the manufacture of soda-water, and other aërated liquids."—November 22, 1867.

3340. J. P. Smith, Glasgow, N.B., "An improved mode of coating and uniting metals with metals."—November 26, 1867.

3360. H. F. Gardner, Boston, U.S.A., "Improvements in the means of, and apparatus for, treating metals and minerals, in order to produce their oxides or other chemical or mechanical combinations, and to separate metals from their ores, or from their alloys."—A communication from G. A. Willard, Boston, U.S.A., and W. G. Adams, Franklin, Mass., U.S.A.—November 27, 1867.

NOTICES TO PROCEED.

2114. J. Hargreaves, Appleton-within-Widnes, Lancashire, "Improvements in utilizing certain materials or products obtained during the manufacture of steel and iron."—Petition recorded July 19, 1867.

2132. T. A. Breithaupt, Passage des Petites Ecuries, Paris, "Certain processes of manufacturing extract and essence of hop to be substituted for the plant itself in the making of beer."—July 22, 1867.

2166. C. E. Brooman, Fleet Street, London, "Improvements in the manufacture of cast-steel and its derivatives."—A communication from E. Martin, and P. E. Martin, Paris, July 25, 1867.

2553. J. Eichhorn, Delahay Street, Westminster, "Improvements in furnaces, for melting iron and other metals, and for smelting ores."—Partly a communication from H. Krigar, Hanover, Prussia.—September 9, 1867.

2618. T. Bell, Hampstead, Middlesex, "Improvements in treating the oxide of iron residues of gas purifying in order principally to extract sulphur therefrom."—September 17, 1867.

2142. H. A. Dufrené, Rue de la Fidélité, Paris, "Improvements in preserving iron from oxidation."—A communication from A. Delcelier, Eu, France.

2146. S. Bonsall, Philadelphia, U.S.A., "Improvements in tanning, and in the machinery and apparatus to be employed therein."—Petitions recorded July 23, 1867.

3208. A. F. Gaidan, Nimes, Gard, France, "Improvements in compressed or artificial fuel."—Partly a communication from L. Tresgot, Nimes, Gard, France.—November 13, 1867.

3232. J. Clark, Ph.D., and A. Esilman, Glasgow, N.B., "Improvements in decomposing the sulphides of copper, silver, nickel, cobalt, lead, barium, strontium and calcium, and obtaining copper, sulphur, and other products."—November 14, 1867.

3275. W. J. Coleman, Bury St. Edmonds, Suffolk, and A. Coleman, of Lombard Street, London, "Improvements in the combination and mode of treating and employing certain preparations for various articles of food."—November 19, 1867.

3295. J. Townsend, Glasgow, N.B., "Improvements in the manufacture of soda and potash."—November 21, 1867.

2180. P. A. Rohart, Rue du Canteleux, Douai, France, "Improvements in the manufacture of gases for the production of light, heat, and motive power, and in apparatus for that purpose."—July 27, 1867.

2198. A. Watt, Putney, Surrey, "An improved fertilizing compost."

2200. J. Jones, Little Bolton, Lancashire, "An improved chemical mixture or compound for extinguishing fires and destroying explosive fire-damp in coal mines."—July 30, 1867.

2213. G. Gordon, San Francisco, California, U.S.A., "Improved processes and apparatus to be used in the manufacture of sugar, and in sawing, cutting, or forming the same into cubes for use."—July 31, 1867.

2239. E. A. Kirby, Gordon Square, Middlesex, "An improved system of dispensing medicines and preparing drugs therefor, together with an improved portable miniature dispensary and instrument case applicable to such system."—August 2, 1867.

2264. J. Heaton, Langley Mill, Derbyshire, "Improvements in blast furnaces."—August 5, 1867.

2270. T. Luthringer, Lyons, France, "A new red colouring matter."—August 6, 1867.

2288. A. M. Clark, Chancery Lane, "An improved metallic alloy and in the applications of the same."—A communication from G. A. Schmitte, and H. A. Levallois, Boulevart St. Martin, Paris.—August 8. 1867.

2294. H. A. Avery, and G. Penabert, Paris, "The application of a certain vegetable powder for removing and preventing incrustations in boilers."—August 9, 1867.

2337. J. A. Jones, R. Howson, and J. Gjers, "Improvements in puddling and other furnaces employed for melting, boiling, or heating iron."—August 14, 1867.

2685. A. Giegele, Mincing Lane, London, "Improvements in the manufacture of Epsom salts."—A communication from Messrs. Vorster and Gruneberg, Cologne, Prussia.—September 23, 1867.

NOTES AND QUERIES.

It has been represented to us that our column of Notes and Queries has occasionally been made the vehicle for the surreptitious disposal of trade secrets by subordinates in chemical works, unknown to their principals. This column has proved to be sufficiently useful to a large class of our readers for us to be reluctant to discontinue it for the sake of a few who abuse its privileges. Probably a more rigid supervision will enable us to obviate the difficulty. There will be no objection to a correspondent asking for information on trade subjects; but the answer must likewise be made public, and in such cases no name or address can be given, no private communications forwarded through us, and no offer of payment for information can be published.

Bichloride of Methylene.—Will you be good enough to let me know the best process for making bichloride of methylene, also any tests for its purity?—J. Miller. [Some commercial samples of bichloride of methylene have proved on analysis to be little more than a mixture of chloroform and ether.—Ed. C. N.]

Water for Steam Boilers.—Can any one refer me to a work which gives reliable information upon "Waters" with respect to their suitability for supplying steam boilers, or can you tell me what is the physical character of the deposits when the water contains much sulphate of lime or carbonate of lime?—E. S. T.

Watts's Dictionary of Chemistry.—In reply to a query addressed to us by a Subscriber, we are informed by the Publishers of Watts's Dictionary of Chemistry that there is only one sheet now due to the subscribers to that work, as the thirteenth part contained two sheets more than the proper quantity. The missing sheet will be given in a future part.

The Bichromate Battery.—Can any of your readers refer me to a description of the mode of using the bichromate battery? Is it as well suited as Grove's, for use with Ruhmkoff's induction coil? Is it as intense and as constant as that battery? Should the zinc plate of it be amalgamated? In what proportion should the saturated solution of bichromate of potash be mixed with strong sulphuric acid?—Clericus.

Atomic Weight Queries.—(1.) In p. 2 of Frankland's "Lecture Notes" we read that the atomic weight of an element is made to represent as far as possible "the weight of the element in the solid condition, which, at any given temperature, contains the same amount of heat as seven parts by weight of solid lithium, at the same temperature." A reference to Kopp's researches is, I presume, here made. Where can I find information elucidating the above statement? (2.) On comparing the titles of atomic weights in Watts's "Dictionary of Chemistry," vol. i., p. 465, published in 1863, and in Frankland's "Lecture Notes," page 6, published in 1866, I find a large number of the atomic weights doubled in the latter. The doubling is I believe due to the researches of Cannizzaro. Where can I find an account of these researches? Dr. Odling, in his article on "Atomic Weights," just referred to, speaks of the objections to Cannizzaro's proposal of doubling the atomic weights as being "too great to admit of its adoption?" How then has it become so rapidly adopted?—Rusticus.

Lime Soap.—Can any of your friends tell me how to decompose lime soap, completely and quickly? The hydrochloric and sulphuric acid methods require too much boiling to pay.—A. B. E.

Bone Boiling.—Your correspondent, "Oss," making inquiry as to means to abate the nuisance or smell of bone boiling, or rendering fats, as well as other kindred points, can receive the desired information by addressing H. S. B., Box 4968, P. O., New York City.—H. S. Bradford.

The Bichromate Battery.—In reply to "Clericus," I frequently use the bichromate battery with a Ruhmkoff's coil. Its action is considered as "intense" as a "Grove's," but it is less constant. This defect is to a great extent remedied by the usual form in which it is constructed, viz., with a means of raising the zinc out of the solution whenever it is required to stop the current, thus economizing its power. The zinc should be amalgamated, and the cold saturated bichromate solution mixed with a twelfth part by measure of sulphuric acid. One great advantage of this battery is that it gives off no fumes.—F. Baden Benger.

Extraction of Oil.—Dyeing Turkey Red.—Can any of your readers give me information relating to the use of bisulphide of carbon in extracting oil from liquors with which it is mixed, such as a solution of olive oil, pearl-ash ley, and water, and if an apparatus for this purpose would be expensive? Also, I should like to know if hyposulphite of alumina is at present in use as a mordant for Turkey-red dyeing, and if any of your numerous correspondents can supply me with a good practical process for dyeing Turkey-reds, both in cloths and yarns, similar to what is at present pursued in some of the large establishments in Lancashire.—E.

China Clay.—I deal largely in China clay, and find curious differences in the purity of certain lots—for instance, three months ago I delivered a maker of sulphate of alumina fifty tons, about which no fault was found; but six weeks after, when the remainder of the cargo came to be delivered, it was found to give out sulphurous acid largely during calcination, and to yield, moreover, a bad colour in the manufactured article. Now the question arises, can this clay, lying as it did in sheds at Runcorn, absorb sulphurous acid from the atmosphere, which may escape from some of the large chemical works in immediate neighbourhood? I can give no other reason for the impurity.—Enquirer.

Receipt for Preparing Blue-black Writing Ink, which also serves well for Copying Ink.—Take of blue Aleppo galls five ounces and a half; powdered cloves, quarter of an ounce; purified sulphate of iron, an ounce and a half; sulphate of indigo (in the form of a thin paste), an ounce and a half; pure sulphuric acid, thirty-five minims; cold rain water forty ounces. Digest the galls when bruised with the cloves in twenty ounces of the water for one week, then pour off the liquor into another bottle and cork it. Then pour ten ounces more of the water on the galls and digest four days. Then pour liquor as before into bottle.

Pour then the remaining ten ounces of the water on the galls, and digest four days. Then pour off liquor into bottle, and filter through French filtering paper, wringing out hard the refuse of the galls in a strong clean linen or cloth into the filter, so that nothing be lost. Add now the iron, and dissolve and filter through paper. Then the acid, and shake. Then the indigo, and thoroughly mix it, and pass the whole through filtering-paper. Care must be observed that the indigo be mild, and not contain too much free acid.—J. H.

Bleaching Palm Oil.—Your correspondent, Mr. George Johnson, has failed to bleach palm oil from three causes. He has, first, the water condensed from the steam. 2. The dreg is left in the oil; and 3. He is too sparing of his materials. Let him try the following: "Boil the palm oil, either with steam, or if in a pan with a fire under it, add half a hundred-weight of water to the oil in the pan before the fire is put to it, allow the whole to settle till next day, draw off the pure clear oil only into a clean dry palm-oil cask standing on end with the head out. For ten hundred-weight, fourteen pounds bichrome, to which add as much boiling hot water as will dissolve two-thirds of the chrome; after a little stirring add to this thirty-five pounds of muriatic acid which will dissolve the rest, making a strong solution; have ready fifteen or twenty pounds of concentrated sulphuric acid, add the acid chrome solution to the oil, and stir for a few seconds; while stirring begin pouring the sulphuric acid, not too slow, until a strong dark green appears, then stop pouring the acid, stir for a second or two, and add 20 gallons boiling water, a little more stirring and the process is finished. About 7 or 8 lbs. of sulphuric acid should bring out the green. With all the materials ready at hand, the process should be finished in 3 minutes. These proportions, especially the acids, are in excess, but a little practice will enable G. J. to reduce the quantities. The agitation should be vigorous, not round about but from the bottom to the top, so as to always bring up the chemicals through the oil. P.S.—Don't bother much about the temperature.—G. H. W.

Water for Steam-boilers.—In reply to E. S. I., I beg to give the following information:—Your correspondent will hardly meet with a book on the subject alluded to, since it would be next to impossible to meet every special case. There exists a German work, the title of which is "Der Fuhrer des Machinisten," bei C. F. Scholl, Civil Ingenieur, published at Brunswick by Vieweg and Sons, 6th edition, 1864. This thoroughly practical book contains a large amount of information which would be vainly looked for in books on steam engines. As regards the incrustations in boilers, it is not only the greater or less amount of mineral matters kept in solution by the water supplied to the steam-boilers which is to be considered, but also the presence of the steam, consequently the temperature to which the water is heated, the mode of supplying water to the boilers, and the place at which the feed pipes enter the same. As a general rule, a hard strongly adhesive incrustation is more due to the sulphate of lime than to the carbonate of lime. The latter, when no sulphate at all is also at the same time met with in the feed water, leaves a muddy slime rather than incrustation, provided, however, no alkaline salts be present at the same time, as, for instance, common salt; and provided also the pressure of the steam be kept comparatively low, at any rate below 45 pounds per square inch. There have been various means proposed to obviate the incrustation in steam-boilers. Some of these are really useless, or even injurious in many ways. A very useful mixture, which effectually answers the purpose, is the following:—1 hundred-weight of catechu; ½ ditto of common salt, are dissolved in 226 gallons of water (best rain water if it is to be had, or soft clear river water). 10 lbs. of this mixture are daily sufficient to keep a boiler, which has to convert into steam, in 12 hours' time, 400 cubic feet of hard water, free from incrustation. The mixture is best pumped in along with the feed water. If E. J. T. will send his address I will give him a diagram and description of an apparatus never met with in connection with steam-boilers in this country, but very frequently found abroad, where no steam-boilers can be used, without being under the supervision of properly qualified officers, and where no water is allowed to be used for steam-boilers but after taking every care with it so as to prevent explosions and accidents.—Dr. A. A.

ANSWERS TO CORRESPONDENTS.

NOTICE.—The American Publishers of The Chemical News *give notice that in accordance with a suggestion of* Mr. Crookes, *the Editor and Proprietor of the English publication, they will be pleased to receive and forward to him in London any scientific publications issued in America, for review—and also any Notes and Queries, Articles, Correspondence, etc., for publication or reply. Their facilities of communication with* Mr. Crookes *render this very desirable to all persons in the United States who wish to confer with him. Address,*

W. A. TOWNSEND & ADAMS,
434 *Broome Street, New York.*

S. E. Philips.—We are by no means so materialistic as our correspondent supposes, and shall be very glad to peruse the article named.

D. H.—"Santonine" was a misprint for "Saponine."

J. D. Barry.—Thompson's Dictionary of Chemistry is in one volume. Watts's Dictionary is the most suitable, but it is in four or five volumes.

Rusticus.—1 and 2. See "Notes and Queries." 3. The best elucidation will be found in Wurtz's "Introduction to Chemical Philosophy," published at our office. 4. Not generally adopted. 5. There is no *best* method, strictly speaking. Either mode of formulation meets with supporters, and at present there are not sufficient data to enable one to decide between them.

An Old Subscriber.—Is thanked for his communication: we will endeavour to attend to his wishes. There is sometimes difficulty in getting the requisite permission; we think however, that we may promise the lectures this year.

H. Boyes.—The cause of the effervescence of aërated liquids when a foreign body is introduced into them (*i.e.*, bread into champagne) was fully explained in a paper by Mr. Tomlinson, F.R.S., which appeared a few months ago in the Chemical News.

E. Kernan.—The *Moniteur Scientifique* is a very good scientific journal. The publisher can supply you from our office. *Les Mondes* can also be sent from our office. The subscription, post free, is £1.12s. per annum, in advance.

W. Schofield.—The matter is under consideration. We will communicate again on the subject.

Professor Weltsein.—Your letter arrived some time ago, but not the book.

J. Bournes.—Our publisher has answered your first question. 2. Galloway's chemical tables are sufficiently large for class teaching, but we believe they have not yet been printed with new atomic weights and most recent formulæ.

X. Y. Z.—In examining white lead mixed with oil, it is best to extract the oil with ether or chloroform, and then examine the residue with acids, etc., in the ordinary way. If you ignite to drive off the oil you will in all probability alter the composition of the lead compound.

Hg.—There is no particular physical reason why the other planets should not be inhabited. Venus would, however, be preferable to Mercury to human beings constituted like ourselves, for on the former planet a body weighing 1lb. on the earth would weigh 0·98 lb., and the light and heat received would be 1·91 times that received by the earth. On Mercury a pound weight would weigh 1·106 lbs., whilst the proportion of light and heat would be 6·68 times that received by the earth. This excessive radiation may, however, be in a great measure intercepted by a dense atmosphere.

James Y.—The copies cannot be sent, as you neglected to give an address.

J. S. P.—O'Neill's "Dictionary of Dyeing" will probably give you all you want to know. *L'Invention* is a French periodical; it can be ordered through any foreign bookseller.

J. G. Tatters.—Many organic ethereal bodies which contain sulphur, and some which are free from that element, have an offensive odour resembling garlic.

A. B.—We can give no idea of the prices. Your best plan will be to advertise for what you want.

Communications have been received from Dr. Rohrig; Dr. Attfield; Dr. Odling, F.R.S.; W. Spalding; Dr. R. Angus Smith, F.R.S. (with enclosure); F. O. Ward; I. Baggs; H. K. York; Herr Sensolmer (with enclosure); W. White; H. Morris; T. Reader; S. Muspratt; M. Murphy; C. David; J. McDougall; J. Slater; C. Tomlinson, F.R.S.; Herr H. Kunde; J. Grieveson; J. Walker (with enclosure); J. Malcome; H. Johnson; J. Williams; J. O. M. Wallace; E. Riley; J. D. B. Fraser; Rev. Edwin Smith; A. Warner; T. Loveridge; Dr. Davy; Dr. S. Muspratt; (with enclosure); Carrell and Co.; W. Molyneux; T. Smith (with enclosure); C. Dussace; G. Maurice; H. Hayman; J. Thompson; W. Ladd; Professor Morton; Dr. Quesneville; Professor Heaton; C. Greville Williams, F.R.S.; W. Jameson (with enclosure); Rev. C. W. Kett; Johnson and Sons; Dr. Pattison; Dr. Wilhelms; J. Imray, C.E.; G. H. Jennings (with enclosure); J. Gra. Tatters; W. A. Jackson; S. Scott (with enclosure); H. E. Roscoe, F.R.S. (with enclosure); J. Baxendell, F.R.S., (with enclosure); H. Simkinson; Dr. F. C. Calvert, F.R.S. (with enclosure); Dr. Sansom (with enclosure); J. Masterman; Professor Tyndall, F.R.S.; J. Ellison (with enclosure); H. Spence; M. Williamson; Jabez Hughes; W. Scott; G. F. Rodwell; J. A. Wanklyn (with enclosure); Rev. F. Sonley Johnstone; T. W. Cowburn; G. F. Barker, M.D. (with enclosure); Dr. A. W. Hofmann, F.R.S; Youngman, Brothers; Professor J. H. Pepper; H. K. York; H. S. Bradford (New York); G. Maurice; R. Reading; Young's Paraffin Light and Mineral Oil Company; Nicholson, Taylor and Co.; H. R. Marsden; M'Dougall Bros.; D. Knight; W. Smith and Co.; H. Baillière; E. Riley; R. Le Neve Foster; G. W. Eccles; Dr. Quesneville (with enclosure); J. Lonsdale; W. E. Brennon; D. Forbes, F.R.S.; W. Dennison (with enclosure); F. C. Samuels; Dr. H. Dobel; R. Boyd; G. J. Symons; S. Merrill; H. K. Bamber; W. Whitmore (with enclosure); Dr. Rohrig; J. Ford (with enclosure); D. Wilhelm; R. Oately; F. Rose; C. P. Bahin; F. Cooke (with enclosure); H. Jackson; Gustav Trouvé; F. Schutzenberger; T. Jones; J. Pearson; M. Morris; J. Grimwood; A. E. Sansom; S. J. Mackie; W. Adams; F. Sutton; F. B. Benger; T. Blair; J. Sutherland; W. A. Ross; F. C. Calvert and Co.; E. Meldrum; Piesse and Lubin; F. Hodgson; D. Knight; W. Paterson; J. Walker; H. B. Condy; P. Squire; J. Frankau and Co.; G. W. Eccles; J. Slater; D. Swan, junior; J. Henderson; R. Warington; J. Young; Dr. Attfield; Liebig Extract Meat Company; E. Wood (with enclosure).

Books Received.—"Cassell's Popular Educator." "The Reducer's Manual and Gold and Silver Worker's Guide," by Victor G. Bluede. London: Trübner and Co. "American Artisan." "Journal of Gas Lighting." "Bulletin Mensuel de la Société Chimique de Paris." "Scientific American." "Proceedings of the British Pharmaceutical Conference," Dundee Meeting, 1867. "A Catalogue of Photographic Apparatus, Chemicals, &c.," Murray and Heath, Jermyn Street. "The Pharmaceutical Journal" for December. "Mining and Scientific Press." "American Gaslight Journal." "Hardwicke's Science Gossip." "Philosophical Magazine" for December. "American Journal of Mining." "A Manual of Pharmacy, for the Student of Veterinary Medicine." By W. J. T. Morton. London: Longmans. "American Artisan." Bulletin de l'Encouragement." Journal of Gas Lighting." "Chemist and Druggist." "Scientific American." "American Journal of Mining." "American Gas-Light Journal."

THE CHEMICAL NEWS.
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ON THE MANUFACTURE OF GLASS FOR VESSELS EMPLOYED IN CHEMICAL RESEARCHES.

BY PROFESSOR J. S. STAS.

In my researches "*on the Reciprocal Relations of Atomic Weights*" I have stated that the ordinary glass of which retorts, flasks, etc., are made, gives up to nitric and hydrochloric acids at the ordinary temperature traces of the metals it contains. In such vessels it is impossible to evaporate the purest acid to dryness without leaving a saline residue.. Hard Bohemian glass, known as *refractory*, and in general all glasses having no alumina, and containing an excess of silica, resist for an almost indefinite time the action of hot concentrated acids; but the manufacture of balloons, flasks, and retorts in refractory glass presents great difficulties, the most skilful workmen not being always able to work in it when articles are required of an extra large size. I have had this difficulty frequently brought before me.

Wishing to ascertain what should be the composition of glass which would be at the same time unaffected by acids and sufficiently fusible to be manipulated with no great difficulty, I decided to carry out some experiments on this manufacture in an actual glass-house. These researches showed me that a glass having for bases sodium and calcium, if it contains a sufficient excess of silica, resists acids almost as well as refractory Bohemian glass, having for bases potassium and calcium. But it is known that a mixture of equal molecular weights of the carbonates of sodium and potassium is much more fusible than the most fusible of either carbonate by itself; starting from this fact, I have been led to the endeavour to replace, in the composition of refractory glass unattacked by acids, a portion of the potassium by an equivalent quantity of sodium. The result has completely verified my anticipations.

I started from this fact, that to obtain a glass very refractory and unattacked by acids, having for bases potassium and calcium, it should contain about—

Silica	75·00
Oxide of potassium	15·00
Oxide of calcium	10·00
	100·00

Upon replacing in such a glass half of the potassium by its equivalent of sodium, we have—

Silica	77·00
Oxide of potassium	7·70
Oxide of sodium	5·00
Oxide of calcium	10·30
	100·00

In this glass the bases are in the proportion of one atom of calcium (Ca″=40) to one atom of potassium and one atom of sodium.

With these data I made some trials on a manufacturing scale; using for this purpose fine, pure sand employed in the manufacture of crystal glass, monocarbonate of potassium as pure as it comes from the English works, purified bicarbonate of sodium, and carbonate of calcium in the form of white marble, finely pulverised and passed through a silk sieve. These materials, in appropriate quantities, were intimately mixed with ten or twelve per cent. of their weight of arsenious anhydride, and were then submitted in very refractory crucibles to a heat strong enough to bring them to a sufficient state of fusion to enable the glass to be worked. The addition of this enormous quantity of arsenious anhydride was made by the superintendent of the glass works, with the object of more readily determining the liquefaction of the mass. I confess I cannot understand the action of this; it produced, however, no other inconvenience than filling the air with torrents of poisonous matter, and analysis has satisfied me that no trace of the arsenic employed remains in the glass produced.

Operating with the proper proportions, two meltings were effected upon tolerably large quantities. With the glass obtained I had balloons with long necks, matrasses, small flasks, cylinders, etc., blown. The largest balloon which an excellent workman succeeded in making, held about four litres; the capacity of the other balloons varied from one to three litres. The sides of the sphere were kept thick enough to be able to resist the traction to which the glass would be exposed by the shrinking experienced by nitrates when solidifying after fusion.

This glass had a yellowish reflection; it was excessively hard, but little elastic, and as free from hygrometric properties as the best refractory glass of Bohemia.

I took the trouble to submit to analysis some fragments of two balloons from different batches; these balloons were broken after having been used for my experiments. They contained—

Silica	76·4	77·3
Oxide of potassium	7·1	6·2
Oxide of sodium	5·9	6·5
Oxide of calcium	10·6	10·0
	100·0	100·0

In these analyses I determined directly the silica, and the oxides of potassium and calcium. The oxide of sodium was estimated by difference. The glass also contained a little alumina derived from the crucible; I did not weigh it; the numbers for the sodium are therefore that much in excess.

ON THE ESTIMATION OF CARBONIC ACID IN MINERAL WATERS.

BY PROFESSOR FRESENIUS.

When ammoniacal solution of chloride of calcium or barium is mixed with carbonic acid, the metals are not precipitated immediately in the state of carbonates. The author formerly stated that this was due to the previous formation of carbamate of ammonia, whilst M. Carius attributed it to the solubility of the precipitated carbonates in sal-ammoniac. The author has repeated several experiments in support of his view of the case; when chloride of calcium is added to a freshly prepared solution of carbonate of ammonia, the liquid is at first clear, but gradually becomes turbid. If the solution has been made about half an hour, the precipitate forms immediately.

To an ammoniacal solution of chloride of calcium, add twice its volume of water charged with carbonic acid, and it remains clear for a quarter of an hour; after twenty hours the precipitation was not complete and

[English Edition, Vol. XVII, No. 422, pages 1, 2.]

the filtered fluid immediately precipitated with a diluted solution of carbonate of ammonia. A diluted ammoniacal solution of chloride of calcium was treated by a current of carbonic acid for five minutes; it did not become turbid at once. When two drops of a solution of carbonate of ammonia, in 20 parts of water, were added, the first precipitate dissolved when agitated, but a third drop rendered it permanent.

Dr. Fresenius concludes from these experiments that there is at first a formation of ammoniacal carbonate, which only changes slowly into carbonate; this transformation is rendered very rapid by heat. The presence of chloride of ammonium has no influence in these phenomena.

ON A NEW PROCESS OF

ELEMENTARY ORGANIC ANALYSIS,

FOUNDED ON THE ANALYSIS OF THE GASEOUS PRODUCTS.

BY M. F. SCHULZE.*

Burn the substance to be analysed with chlorate of potash in a tube, having previously sealed and exhausted it; then submit to analysis the gaseous mixture produced. The advantage of this method is the small amount of material necessary; the analyses cited as examples were performed with from 5 to 13 milligr. of matter. M. Schulze introduces the mixture, together with rather more than enough chlorate for complete combustion into a combustion tube, sealing it at one end, and drawing it out of the other; after having exhausted and measured the pressure of the remaining air, he seals the tube, shuts it up in a gun barrel, and heats it to a dull red heat for twenty minutes. When cold, he breaks the point of the tube under mercury and collects the gas in a eudiometer. By measuring the quantity of carbonic acid formed, and absorbing it by potash, are to be found all the elements necessary for the calculation of the composition of an organic matter containing only carbon, hydrogen, and oxygen. If the carbon absorbs its proper amount of oxygen, and the compound is a body corresponding to a hydrate of carbon, such as starch, the gaseous material obtained is exactly equal to the amount of oxygen supplied by the chlorate of potash used. If more gas be found, it is because the body contains more oxygen than is needed to burn all its hydrogen; if, on the contrary, there is less gas, it is because the body contained an excess of hydrogen with respect to its oxygen in the constitution of water. 5.5 milligr. of cholesterine, burned with 60 milligr. of chlorate, gave (after allowing for the air remaining in the tube) 9·6667 c.c. of gas (at 0° under one metre pressure). The 60 milligr. of chlorate furnished 12·483 c.c. of oxygen; the difference, 2·8177 c.c., is the quantity of oxygen corresponding to the quantity of hydrogen which was not burnt by the oxygen belonging to the cholesterine. Again, 2·8177 c.c. of oxygen burns 0·6664 milligr. of hydrogen (say 12·1 per cent.). The amount of gas absorbable by potash is 16·566 milligr. of carbonic acid, corresponding to 4·518 milligr. of carbon, say 82·145 per cent. The complement, that is to say

$$100-(12{\cdot}1 + 82{\cdot}145) = 5{\cdot}775,$$

corresponds to the water formed by the oxygen in the cholesterine, and a part of its hydrogen, say 5·133 per cent. of oxygen and 0·639 of hydrogen; on adding this last number to 12·1, there remains 12·74 per cent. of hydrogen. [These numbers, compared with those calculated from the composition of cholesterine,

$$(C=83{\cdot}8;\ H=11{\cdot}8;\ O=4{\cdot}3)$$

do not appear satisfactory.] In the combustion of azotised matters, nitrogen may be obtained (after an estimation of the carbonic acid) by absorbing the oxygen with a stick of phosphorus; but the results are not very correct. As to chlorinated matters, the author effects their combustion with oxide of mercury; the chlorine remains in the condition of a mercurial chloride, in which it may be estimated by decomposing the chloride by potash.

ON THE ESTIMATION OF LEAD BY PRECIPITATION IN A METALLIC STATE.

BY M. F. STOLBA.

To estimate lead by this method, the author treats both soluble and insoluble lead combinations with zinc in the presence of water acidulated from time to time with hydrochloric acid; the reduction is effected at the temperature of the water-bath in a platinum capsule; the lead is deposited partly on the sides of the capsule and partly on the zinc, whence it is easily dislodged. When the reduction is complete, which is easily discerned by a clean surface of the zinc remaining brilliant in the liquid, decant and wash the spongy deposit of lead with water. As pure water might dissolve small quantities of lead, the author recommends an addition of a drop of sulphuric acid. After washing, dry the lead first in a water-bath, then at about 200 C. Even then its exact weight cannot be ascertained, because it has undergone a partial oxidation. After weighing it, the oxygen absorbed must be ascertained, which may be done by Mohr's volumetric method—namely, by treating the lead with a weak standard solution of nitric acid. Wash the dissolved oxide of lead, and add a standard alkaline solution until it begins to produce turbidity. The quantity of oxide of lead is given by the difference in the standard of the nitric acid before and after its action on the lead.

NITROGLYCERINE OR GLONOINE.

The awful accident which lately took place at Newcastle-on-Tyne, caused by the sudden explosion of nitroglycerine, or Nobel's patent blasting oil, has induced me to collect together, from various sources, chiefly published abroad, the following particulars in respect of this substance, and as many of the leading daily London papers have in various ways given accounts about nitroglycerine which are incorrect, I venture to hope this paper will not be found by yourself, Mr. Editor, and your many readers quite uncalled for.

Nitroglycerine was discovered by the well-known Dr. Sobrero, now Professor of the Technical Institute at Turin, somewhere about 20 years ago. The substance was studied simply in a scientific interest by Dr. J. E. de Vrij, the chemist of the Netherlands Indian Government, well known for the analysis of this and testing of the Cinchona bark, and also by Dr. Gladstone, and of late by Dr. Kopp. Up to the end of 1864 nitroglycerine was not only not familiarly known, nor to be had in quantity in commerce, but continued to belong entirely to the domain of science. This may easily be accounted for by the fact that

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* Zeitschrift für Analytische Chemie, t. v., p. 239.—Zeitschrift für Chemie, nouv. ser., t. iii., p. 391.

glycerine itself is only in use and to be had on the large scale since the last 8 or 10 years. When pure, nitroglycerine is a liquid of from 1·525 to 1·6 specific gravity; it has no odour, is often colourless or yellowish, has a sweet, pungent, aromatic taste, and is powerfully poisonous. It is only very slightly soluble in water, readily so in ether, alcohol, and methylated spirits; it does not inflame when touched with the light, nor does it explode by being so touched, but concussion, touching with a red-hot iron, or the concussion due to the explosion of gunpowder, and, better yet, detonating mixtures, and fulminates, sets off the nitroglycerine. According to Dr. Johann Rudolf Wagner, the well-known technologist to the Bavarian Government, nitroglycerine may be cooled down to 4° Fahr. without becoming solid; but it appears after all that the nitroglycerine of commerce, if exposed for a continued period to 46·4° Fahr., becomes solid, crystallising in long needles, which are most dangerous to handle, since they explode, even on being gently broken, with a frightful violence. At 320° Fahr. the nitroglycerine begins to decompose, giving off red vapours, and if the heat be suddenly applied, or slightly raised above this point, the substance explodes instantaneously and with great violence, shattering even open vessels to atoms. Nitroglycerine may be assumed to consist of anhydrous glycerine, in which 3 atoms of hydrogen have been replaced by 3 atoms of NO_4. The products of the complete combustion of 100 parts of pure nitroglycerine are the following:—

Water	20
Carbonic acid	58
Oxygen	3·5
Nitrogen	18·5
	100·0

Since the specific gravity of nitroglycerine is 1·6, one volume, say 1 cubic inch of the material, yields on combustion or explosion—

Aqueous vapour	554	volumes, or bulk.
Carbonic acid	469	"
Oxygen	39	"
Nitrogen	236	"
	1298	"

According to Nobel, these gases expand on explosion to 8 times their bulk. 1 cubic measure volume of nitroglycerine will, therefore, give 10,384 cubic measures of gases; while 1 cubic measure of gunpowder will only yield 800 cubic measures of gases. Hence it follows that for equal bulks nitroglycerine is 13 times stronger than gunpowder, while by equal weights the former is 8 times stronger than the latter.

The danger of the use of nitroglycerine is greatly enhanced by the instability of this compound; even when pure it is affected by increase of temperature, and at from 68° to 75° Fahr. it is prone to incipient spontaneous decomposition, accompanied by a slow but sufficiently strong escape of gaseous compounds, which while exerting a slight pressure on the vessels the liquid is contained in, also can cause the fluid to explode on the slightest concussion. During the slow and spontaneous decomposition of the glonoine there are formed divers products, among these glyceric, oxalic and hydrocyanic acids, and ammonia, and others unknown. Nobel's patent nitroglycerine, or blasting oil, is made in the following manner:—To 13·5 parts by weight of strong sulphuric acid is added 1 part by weight of nitrate of potash of best quality, and this mixture cooled down to 32° Fahr., the result of which is the crystallising out of a salt which contains 1 equivalent of potash, 4 equivalents of sulphuric acid, and 6 equivalents of water; the strongly acid liquid is decanted from the crystals, and to the liquid, commercial glycerine is added, taking care to keep the liquid cold; the ensuing nitroglycerine is separated from the acid by water, once washed with fresh water, and is fit and ready for use.

I may here observe that the manufacture of the substance which has already given rise to so much mischief is carried on in the free City of Hamburgh, which is not subject to any of the laws which in other closely adjacent countries would render the manufacture of the nitroglycerine, if not entirely illegal, at least subject to very stringent but equally justifiable police supervision. In France, Switzerland, Belgium, and the Netherlands, where the French law of 1810, *régulant les métiers insalubres et dangereux* is yet in force, the manufacture of this article can be prohibited. The best mode of manufacturing nitroglycerine where it is desirable to use it, and that is the case in open quarries where one has to deal with tough hard rock, is to make it extempore on the spot where it is to be applied. Take a sufficient quantity of strong nitric acid, density from 1·4758 to 1·4902, mix therewith the double of its weight of strong sulphuric acid, weigh off 3300 grammes of the acid mixture when quite cool; take 500 grammes of glycerine which must be free from either lime or lead salts, and mix the same cautiously with the acid while keeping the mixture very cool by constantly stirring. Let the mixture stand quietly for about 10 minutes, and then pour it out in from 5 to 6 times its bulk of cold water, taking care well to stir the same all the while. The nitroglycerine will sink to the bottom; the dilute acid is removed by decantation, the nitroglycerine once more washed with water, when it would be fit for use after removal of the latter. The glycerine to be used should have a specific gravity of from 1·2459 to 1·2562, *i.e.*, contains from 94 to 96 per cent. real glycerine. Dr. Gladstone has found while engaged with his researches on nitroglycerine, that the perfectly anhydrous glycerine did *not* yield, when treated with the mixture of nitric and sulphuric acids, an explosive compound, but, on the contrary, one which when touched with a flame, or red hot metal, burns off pretty quietly. Impure nitroglycerine is dangerously self-explosive, even while standing quietly.

From the evidence brought forward at the coroner's inquest at Newcastle-upon-Tyne, it appears that the nitroglycerine which had been brought to that town certainly contained some methyl-alcohol, or methylated spirits of wine, not in sufficient quantity to render the nitroglycerine safe; in fact it is not in the interest of the makers of the article to render it so, as this would evidently occasion the trouble of having to separate the nitroglycerine from its solution by the addition of water, previous to being fit for use, and this trouble would be undoubtedly disliked at the mines and quarries, and the use of the article therefore discarded.

From the evidence, given at the inquest just alluded to by I. L. Bell, Esq., the well-known proprietor of large chemical works, collieries, and iron mines, in the county of Durham, it appears that the printed instructions issued by M. Nobel, of Hamburgh, the manufacturer of the nitroglycerine, contain statements which are very contrary to fact, and apt to mislead, and hence give rise to serious mischief and grave danger, while the instructions omit to mention how to deal safely with the article when congealed. The quantity of nitroglycerine, 30 canisters, originally brought to Newcastle in May last, and stored in a public-house under the semblance of dirty grease, was equal to 4·5 tons of powder, and would, according to the pamphlet of M. Nobel, have sufficed to blow down 115,000 tons of solid rock. Mr. Bell stated that he had caused the use of nitroglycerine to be discarded in his mines on account of the injury to the health of the workmen. The inducement to the use of this substance is that, as compared with gunpowder, its effect in blasting is far greater, with less previous labour, and that its use loosens large blocks of rock or stone without crumbling, thus enabling miners and quarrymen to extract large masses of stone easily and without damage. The Messrs. Webb and Co., of Carnarvon, Wales, are the sole agents and consignees of Nobel's patent blasting oil in this country. They also have on hand an article made by Nobel and sold under the name of dynamite, or patent safety blasting powder, 7 times stronger than ordinary gunpowder. It is stated that dynamite will not explode either from a spark or concussion alone, but requires the combined effect of both. In a compressed state it may be fired under water equally well as nitroglycerine. What that dynamite is made or composed of I have not been able to find out; I never saw a sample of it, nor find it mentioned otherwise than in advertisements. A. Adriani.

Mr. Nobel has writen to defend his blasting oil against many of the accidents which have been laid to its charge, he also denies that it possesses some of the properties that have been ascribed to it.

The following almost ludicrous examples, tending to show that the accidents have chiefly arisen from ignorance, are detailed with several others in a list of minor disasters which have resulted from its use:—

"In five cases congealed nitroglycerine has been melted purposely over fire.

"In three cases a red-hot poker has been inserted into the oil in order to melt it.

"In one case a man took to greasing the wheels of his waggon with nitroglycerine, knowing what it was, and it went all right until it struck hard against something, and the wheels went to pieces.

"In one case it was burnt in a lamp as an improvement on petroleum.

"In these days, every mischief is charged to nitroglycerine. Thus, we read in the *Northern Evening Express* that recently a box with nitroglycerine exploded at a railway station in the city of Berlin, 'and that the simple act of placing it in the van caused it to explode.' It is a proved and confirmed fact, that it was fulminate of mercury that exploded."

Nitroglycerine, Mr. Nobel says, has been accused of spontaneous combustion, but the truth is that unless properly purified, it emits a nitrous odour and will gradually decompose during some years. The nitroglycerine, however, now made by him is always pure, he writes "chemically pure;" it is obtained by crystallisation from wood naphtha.

"Nitroglycerine is also charged, and all the world believes it, with being extremely dangerous, even from the scratch of a needle, when congealed. It is a mere fable. It is the nature of every explosive to be more sensible to concussion in its liquid than solid state, since bodies, as a rule, are possessed of greater stability at a lower temperature. As regards nitroglycerine, the congealed crystals, to be exploded, require a far more potent blow than the liquid oil, and it was probably owing to adhering drops of the latter that the Newcastle explosion took place. A crystal thrown with great violence against a stone or iron plate is crushed without exploding, and a strong percussion cap, when inserted into it, produces the same effect. In the mines of Königsgrube (Silesia) a large lump of congealed oil was hurled by an explosion against the rock, and dropped harmlessly to the ground."

Another accusation cited, and refuted, is that of nitroglycerine exploding by contact with oil of turpentine. Mr. Nobel maintains that nitroglycerine is a substance of uniform composition and very reliable.

There is unquestionably much truth in many of the remarks made in this letter, and we thoroughly acquiesce in the following statement.

"Whenever an article can be regularly manufactured it may be regularly used, and accidents are only the result of inexperience—the want or neglect of instruction."

ON THE CHEMICAL CONSTITUTION OF FLUORINE COMPOUNDS, AND ON THE ISOLATION OF FLUORINE.

BY M. PRAT.

The following is a full abstract of M. Prat's memoir on this subject, recently communicated to the French Academy. The complete paper will not be published until the chemical referees of the Academy have reported on it.

M. Prat considers that chemists have hitherto been mistaken as to the composition of fluorides and the theory of fluorine. He regards the fluorides as in reality oxy-fluorides, and the equivalent of fluorine as consequently much higher than is usually supposed. He represents fluoride of calcium by—

2 equivalents of	calcium	40·0
1 "	oxygen	8·0
1 "	the new fluorine	29·6
		77·6

This accords with the known analyses of fluor spar, since it contains 51·5 per cent. of calcium.

By doubling the old equivalent of fluorine (19), we get 38, that is to say nearly the sum of the equivalents of oxygen (8), and of the new fluorine (29·6) = 37·6.

According to M. Prat, in order to obtain true fluorine it suffices to heat fluoride of calcium with chlorate, or rather with perchlorate of potash, since it is only after the formation of this latter salt that the reaction takes place. Oxygen is disengaged, and also a product which silver absorbs. The compound so formed is fluoride of silver, insoluble in water, soluble in ammonia, from which it is precipitated by nitric acid, and more rapidly altered in the light than chloride of silver. Neither chlorine nor oxygen attack it even at the fusing point of the fluoride. It is, however, decomposed by potash at a dull red heat, and this reaction permits its analysis: it contains—

Silver	0·785	108·0	= 1 equivalent.
Fluorine	0·215	29·6	" "
Fluoride of silver	1·000	137·6	

This fluoride of silver, insoluble and very stable, and having great analogy with the chloride and the other compounds of this family, differs essentially from the soluble fluoride of silver of chemists, which according to M. Prat is a compound of—

$AgFl$, AgO, HO, in the hydrated state;
$AgFl$, AgO, in the anhydrous state.

Fluorine combines with chlorine. To obtain this compound it is sufficient to pour a weak solution of the hydrofluoric acid of the chemists into a solution of hypochlorous acid: there form

$$FlH, HO + ClO = 2HO + FlCl.$$

Fluoride of chlorine is gaseous, of a more intense colour than chlorine. It converts silver into a mixture of chloride and fluoride.

Fluorine may be isolated, according to M. Prat, by heating fluoride of lead of chemists (1 part) either with nitre (5 parts) or with binoxide of manganese (2 parts); oxygen and fluorine are evolved. A platinum alembic must be used. The oxygen is removed from the mixture by passing over fragments of heated baryta.

Fluorine is gaseous, almost colourless, of a chlorous odour, visibly fuming in the air, incombustible, and heavier than air. It bleaches indigo, and reddens and bleaches litmus. Ammonia produces fumes with fluorine, and will thus detect traces of it. It immediately decomposes water at the ordinary temperature. It combines with hydrogen in diffused light. Fluorine decomposes hydrochloric acid gas, and eliminates bromine and iodine from their compounds. It unites with boron and silicium, and with all metals of the first five groups.

ON THE MANUFACTURE OF STEEL FROM CAST IRON, BY THE USE OF NITRATES AND OTHER OXIDISING SALTS.*

BY J. HARGREAVES.

The object of this invention is to effect the acieration of cast iron by a direct process, and thus dispense with the many permutations which it is at present made to undergo before the condition of steel is attained. I effect this by the agency of oxidising salts and oxides of iron and manganese. The oxidising salts which are most suitable for the purpose are the nitrates, and especially the nitrate of soda, on account of its lower cost, higher percentage of oxygen, and the highly electro-positive character of its base, which renders it a most effective agent in removing the metalloids, silicium, sulphur, and phosphorus, and the semi-metal arsenic from iron, by forming with them compounds of sodium, thus enabling inferior qualities of cast iron to be used in the manufacture of steel, and also to improve the qualities of malleable iron by depriving it of those objectionable substances. This is effected by placing the converting materials below the fused cast iron, and allowing the products of their decomposition to rise through the fluid metal, exerting while passing through it their chemical energies in separating from the cast iron the excess of carbon above that which is required to constitute steel, and in removing the metalloids which, even in the smallest proportions, deteriorate the value of the product.

The necessary use of fossil coal in Great Britain, in consequence of the scarcity of wood from which charcoal can be obtained, tends, while lowering the cost of production, also to deteriorate the value of the iron produced, by communicating to it some of its own impurities, which, added to those existing in the ore, render the iron very impure.

A great proportion of silicium and sulphur are separated by the lime used as a flux in the form of slag. But the last traces of these elements are very difficult to remove under the circumstances; while, with the exception of a slight trace, the whole of the phosphorus originally present in the ore remains in combination with the metal.

In 1861 my attention was first seriously attracted to the subject of the acieration of iron by the communications of the researches of MM. Caron and Fremy to the French Academy of Sciences, and which were published in the *Comptes Rendus*, an English translation of which first appeared in the Chemical News. These communications show that, to form steel with such qualities as will compete with that produced from Swedish and Russian iron, by cementation; it is necessary that the iron should be pure or approximately so; and that to effect cementation, nitrogen must be present in combination with carbon, or in contact with some gaseous compound of carbon. M. Caron disputes the hypothesis that nitrogen is an essential element in steel; but he admits that it acts as an interporter or carrier of carbon between the charcoal in the cementing pots and the iron, acting in fact as a kind of chemical shuttle, carrying carbon to the iron, and depositing it within the substance of the metal; and after delivering the carbon to the iron, it returns, and again picks up another charge or load of carbon, which it again delivers to the iron. A very small quantity of nitrogen is by this means made the agent for carrying a comparatively large quantity of carbon; and he proposes cyanide of ammonium as a gaseous cementing agent. M. Fremy, on the contrary, asserts that nitrogen is absolutely essential to acieration, and that in the absence of this element cast iron and not steel is the result of operating upon iron with pure carbon, unless the iron itself contains nitrogen. That iron can be made to combine with nitrogen has been placed beyond doubt by M. Despretz, whom M. Fremy quotes and whose experiments he repeats, a ferammonium (NFe_4) having in fact been produced; *i.e.*, a compound analogous to the quasi-metal ammonium in which the hydrogen is replaced by iron. M. Fremy proposes to effect the cementation of iron by the use of ammoniacal and hydrocarbon gases, to supply nitrogen and carbon. I was then, and am still, disposed to take the view of M. Fremy as the correct one. But the thought suggested to me by the discussion between these eminent savants, was—"Why not obtain and use some material which shall effect the removal of the excess of carbon, with all other objectionable elements, and at the same time add nitrogen, which being chemically very inert, must be in the nascent state to effect its combination with iron, instead of removing the whole of the carbon, and then by a very expensive, laborious, and inconstant process, restore a portion of the carbon extracted and a very small portion of nitrogen?"

The great difficulty was to find an agent capable of fulfilling the requisite conditions, which are:—

1. That it shall remove any desired quantity of carbon, this being varied with the varying proportion of carbon contained in the cast iron, and leave in it just sufficient carbon to effect its acieration.
2. That it shall remove all the silicium, sulphur, and

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* Abstract of a paper read before the Liverpool Polytechnic Society.

phosphorus, or at least leave only traces of these elements.

3. That it shall supply nitrogen in the nascent state.

I occasionally took up and studied the subject, but with no satisfactory result—as I could not see clearly how to have a practical process, which could compete with those then in operation—till in 1864, when considering the theory of the action of the alkaline nitrates upon the elements composing cast iron, I found that these salts possessed all the properties necessary to accomplish the object I then had in view.

1. The quantity of carbon to be removed could be regulated at will by the quantity of nitrate used.

2. The alkali would in all cases be in excess of the quantity required to form chemical compounds with silicium, sulphur, and phosphorus, and give rise to the formation of silicate of soda, sulphide of sodium, and phosphide of sodium.

3. Nascent nitrogen would be formed in the presence of iron, in consequence of the formation of cyanides, by the reaction of the sodium or potassium and nitrogen of the nitrate upon the carbon in the cast iron, and would also be liberated upon the decomposition of the oxides of nitrogen.

4. The reaction of the nitrate upon fused iron could be easily effected by placing the nitrate at the bottom of a suitable vessel, and allowing the products of its decomposition to rise through the metal.

Before describing the details involved in treating iron by this process, I beg to point out the general principles upon which it is based. You will have before observed, that to effect the removal of a given quantity of carbon from cast iron, a given quantity of oxygen must be supplied, to convert this carbon into carbonic acid, or carbonic oxide. To convert six parts of carbon into carbonic acid, sixteen parts of oxygen are required, and to form carbonic oxide, eight parts of oxygen combine with six parts of carbon, and in one or both of these forms the carbon is eliminated from the iron. The weight of oxygen contained in the acid of the nitrate of soda, and which will be eliminated from it, is equal to 47 per cent.; binoxide of manganese yields about 36¼ per cent., and sesquioxide of iron 30 per cent. It thus becomes tolerably easy to ascertain, especially after a few practical trials, the proportion of oxidising materials necessary to remove a given quantity of carbon. This is, however, complicated by some conditions, each of which requires a trial or two to obtain exact results. For instance, when the operation is carried on in a deep vessel, the oxidising materials act more effectively than when a shallow vessel is used, because the products of their decomposition have more time to become saturated with the impurities which it is desirable to remove from the iron. The rate of evolution of the gases from the oxidising material is regulated by the proportion of oxide of iron, of oxide of manganese, mixed with the nitrates. These oxides, though themselves evolving oxygen, especially in the presence of carbon, do so tardily: and they restrain the otherwise uncontrollable rapid action of the nitrates alone. By this means the action of the nitrate can be so retarded as to cause only a comparatively slight ebullition. The nitrate of soda is mixed with a proportion of oxide of iron, the most suitable form of which is hæmatite, and when in a slightly moist condition, passed or tamped into the bottom of a vessel lined with fire-brick; the whole is then dried into a solid block. If the vessel has been used immediately before, its heat will be sufficient to dry the mass, but if not heated by a previous operation, it is heated by other suitable means. The nitrate of soda of commerce is generally sufficiently moist, without the addition of more water. When the mass is dry, the fused iron is poured upon it; successive layers being then removed, the materials by their levity are carried through the body of the metal, and the reactions occur, to which I have before referred; and as each layer is removed, the part immediately below is exposed to the heat of the melted metal. A boiling appearance accompanies this action, and a frothy slag containing some oxide of iron, and compounds of soda, with the impurities extracted from the iron, rises to the top. After the action has ceased, in consequence of the converting materials being expended, the steel is tapped out, and made use of in any suitable way.

Refined iron, for the manufacture of malleable iron in the puddling furnace, may be made by the use of about 3 per cent. of nitrate and 6 per cent. of peroxide of iron. Steel, by 8 to 10 per cent. of nitrate and equal weight of binoxide of manganese. Malleable iron by 8 per cent. of nitrate and 20 per cent. of peroxide of iron. In each case iron with 5 per cent. of carbon being used.

But it was often suggested to me that the use of separate and special apparatus is objectionable, on account of its expense, as manufacturers are generally averse to any large outlay upon new processes; and that some mode of applying it to the ordinary puddling furnace would be very useful. But there was this difficulty to contend with, the puddling furnace is too hot for the introduction of the converting materials, and fixing them at the bottom, and could this be done they would be decomposed before the fusion of the iron could be commenced, to say nothing of their remaining till it could be melted, so as to allow the gases evolved to rise and act through the fluid metal. I get over this difficulty as follows:—I make the converting materials into blocks or balls, and fix them on the ends of iron rods. These balls being made hard by drying, are ready for use. When the iron is fused in the puddling furnace, and the boil has commenced, one of these balls is pushed to the bottom of the metal in the furnace—the products of its decomposition rise through the metal, causing rapid agitation, which is much more effectual than that produced by the puddler with his tools. After the ebullition has ceased, the rod is withdrawn and another put in its place. The time occupied in puddling is thus very much shortened, the labour very much reduced, and fuel saved, and a better yield of metal obtained, in consequence of the soda forming a base which readily combines with the silicic and phosphoric acids eliminated from the iron. In the ordinary puddling operations the silicium and phosphorus are extracted by the previous formation of oxide of iron, with which those acids, which are also products of oxidation, combine. But when silicium and phosphorus are reduced to a somewhat small proportion of the whole, the last traces of them are removed with difficulty, still the powerfully basic character of the soda intensifies the disposition of these substances to separate from the iron, and to enter into combination with itself.

The malleable iron produced from cast iron which has been treated with nitrates is of a very superior quality, having great range of temper. The same metal which by gradual cooling is fit for purposes requiring great toughness and powers of endurance of bending and torsion, may by rapid cooling be made sufficiently hard for wood-cutting tools; and its freedom from impurities is shown by the remarkable thin scale formed when the iron is worked by the smith, and the consequently small amount of loss in working. In this respect it very much resembles the best charcoal iron, and contrasts very remarkably with the iron made from the same "pig," but which has not been previously treated with nitrates. The presence of silicium causes a large amount of waste when malleable iron is exposed to the atmosphere at high temperatures, causing a thick, heavy scale, which must contain at least 70 per cent. of iron.

ON THE
CHEMICAL GEOLOGY OF MR. DAVID FORBES.

BY T. STERRY HUNT, F.R.S.

In the Chemical News of October 4th, 1867 (*Am. Reprint*, Dec. 1867, page 281), there appears a paper purporting to be a criticism of some views on the chemistry of the primeval earth, put forward by me in a lecture delivered before the Royal Institution of Great Britain, on the 31st of May last, and published in the Proceedings of that Institution, as well as in the Chemical News of June 21st (*Am. Reprint*, Aug. 1867, page 82), and the *Revue des Cours Scientifiques*, of October 19th, and also in *Les Mondes* and *Cosmos*. The object of my present communication will be to notice briefly some of the criticisms of Mr. Forbes. The readers of my lecture are aware that I assumed as my starting-point the hypothesis, now generally accepted, of the origin of our earth, and of all planetary and stellar worlds, by a process of condensation and cooling from a nebulous or a gaseous matter, so intensely heated as to be luminous, and to contain, at the same time, in a free or dissociated condition, the various chemical elements. The first objection of Mr. Forbes, is that I do not explain the origin of this intensely heated condition: a consideration entirely beyond the scope of my lecture, but established by the spectroscope, and to be accepted as an ultimate fact, the secret of which, like that of the origin of matter itself, rests with the Great First Cause.

In discussing the laws which presided over the cooling of our own globe, I gave several reasons which have led modern investigators to reject the old theory of a liquid centre covered by a thin crust of congealed rock. I alluded briefly to the mathematical deductions of the late Wm. Hopkins from the phenomena of precession and nutation,—those of Archdeacon Pratt on the feeble resistance which would be offered by a crust of the thickness generally admitted by the old school, to the crushing weight of masses like the Himmalayah Mountains,—and the conclusions of Thompson as to the rigidity of the earth, deduced from the theory of the tides, as so many concurrent arguments in favour of a crust at least many hundred miles in thickness, if not of a globe entirely solid. Proceeding, thence, to consider the conditions of cooling presented by the fused and oxidised mass of the globe, I asserted that the analogies offered by most of the bodies forming the earth's crust, which yield compounds considerably denser when solidified than when in their fused condition, lead us to conclude that the solidification of the globe must have begun from the centre. In fact, the numerous and detailed experiments of Charles Deville (*Comptes Rendus*, xx., 1453), and those of Delesse (*Bull. Soc. Geol. de Fr.* [2] iv., 1380), not to mention the earlier ones of Bischof, unite to show that the density of fused rocks is much less than that of the crystalline products resulting from their slow cooling, so that, as Saemann has justly observed, we are forced to conclude that the crystalline stony masses formed at the surface of a liquid globe must sink towards the centre (*Ibid.*, Feb. 4, 1861). To this conclusion Mr. Forbes objects that, in the cooling of sulphur or metals from fusion, a crust forms at the surface before the interior is solidified; he should consider that the conditions in a small crucible, placed in a cold atmosphere, where cooling is rapid, and the crust is supported by adhesion at the edges, are vastly different from what would obtain in a world-wide bath, cooling with great slowness beneath an intensely heated atmosphere. In such a case, as the crystalline silicates known to us, are, according to numerous experiments, from one-seventh to one-sixteenth denser than the same materials in a fused condition, it would require a suspension of the laws of gravity to counteract the inevitable tendency of the heavier solids formed at the surface to sink in the fused mass, in which they would subside as naturally as the crystals which form at the surface of an evaporating basin of brine. The analogy holds good since the crystals formed at the surface, whether by evaporation or by cooling, obey the laws of gravity. The freezing over of the surface of such a mass would be as unnatural as the freezing of a lake of water from the bottom.

Mr. Forbes next comments upon my allusion to the experiments of Hopkins on the effect of pressure in elevating the melting points of such bodies as contract in cooling, and says that I appeal to these as conclusive proof that the melting points of bodies do become (*ad infinitum*) elevated in proportion to the pressure. In fact I said nothing of the sort, but insisted that the researches of Hopkins "are to be considered in this connection." If Mr. Forbes had taken some pains to inquire into the question, he would learn that these experiments of Hopkins, and others (by W. Thompson on the effect of pressure in reducing the melting point of ice) were suggested by a remarkable essay by James Thompson (Trans. Roy. Soc. Edin. xvi., part 5). In this it was shown that the fusing point of ice, which contracts in melting, must necessarily be reduced by pressure; while, as Sir Wm. Thompson showed, the reverse effect was to be expected for all solids which expand in melting (L. E. and D. Phil. Mag. [3] xxxvii. 125). The results of Hopkins thus come under a general physical law. Mr. Forbes will find a simple and intelligible statement of the principle laid down by Thompson, and Hopkins's argument therefrom, for the solidity of the interior of the globe, in the fourth of Dr. Tyndall's admirable lectures on Heat, delivered before the Royal Institution. See also Mr. Sorby's Bakerian Lecture for 1863. As to Mr. Forbes's suggestion of denser matters towards the earth's centre, I have said the same thing in my lecture.

Mr. Forbes next proceeds, in his own words, to submit my views of the chemical changes which took place at the surface of the globe, to "careful scrutiny," in order to determine whether "they are sound and likely to meet with acceptance in the chemical world." Of the critic's fitness for his self-imposed task the reader shall judge. The first thing to be determined in the cooling of an intensely heated vaporous mass is the nature of the chemical compounds which would be formed among the dissociated elements. As I have stated in my lecture, the combinations stable at the elevated temperature then prevailing, would be first formed. The affinities of oxygen are such, that under such conditions, an excess of this element being present, instead of sulphides of the heavy metals, as imagined by Mr. Forbes, oxides and sulphurous acid would be produced in virtue of affinities known to every chemist and metallurgist. So with regard to chlorine, the production of alkaline chlorides in such conditions is inconceivable, since in the conjoined presence of oxygen, hydrogen, and silica, an alkaline silicate and hydrochloric acid would necessarily result. Even if, as Mr. Forbes supposes, chloride of sodium were to be formed in the heated atmosphere, it would be precipitated into an intensely heated bath of fused silicates, covered by an atmosphere charged with aqueous vapour, or with mingled hydrogen and oxygen, and would immediately undergo the same decomposition that takes place when the vapours of common salt are diffused through the heated atmosphere of a potter's kiln, or, as in Mr. Gossage's new soda-process, are passed with steam over red-hot flints. In both cases silicates of soda are formed, with separation of hydrochloric acid. These considerations lead to the conclusion that, after all the more fixed elements were precipitated, the whole of the chlorine would remain as hydrochloric acid, and the whole of the sulphur as sulphurous acid, together with a large proportion of oxygen, since we find sulphates and not sulphites in the sea-waters. To this constitution of the still intensely heated atmosphere, Mr. Forbes objects, and inquires whether it is "at all probable, even if possible, that an excess of oxygen could exist along with the vast amount of sulphurous acid." He farther adds that "the improbability of such an atmosphere containing a mixture of hydrochloric and sulphurous acids, may be inferred from Dumas's researches; that chemist having long ago shown that these two gases, when mixed together, react and mutually decompose each other, with the formation of water, chlorine, and sulphur." Mr. Forbes thinks he has hit upon two objections to the existence of a heated atmosphere holding, as I have endeavoured to show, besides nitrogen, oxygen and watery vapour, sulphurous and hydrochloric acids. He has evidently a vague notion that sulphurous acid and oxygen have an affinity for each other, and ought to form together sulphuric acid. So they do unite slowly at proper temperatures, in the presence of water, being converted into oil of vitriol, and it was doubtless in this way, as I have elsewhere shown, that the sulphur was eventually brought down from the atmosphere, and formed the sulphates of the sea. But every chemist is aware that at higher temperatures oil of vitriol is resolved into water, sulphurous acid and oxygen gases, and that this reaction is made use of as an economical process for the preparation of oxygen on a large scale, the sulphurous acid being removed by absorption from the cooled gases. As regards his second point, Mr. Forbes, who cites Dumas (Traité, i. 146) has been misled by quoting at second-hand, apparently from the English edition of Gmelin (ii. 321). Dumas' states that in solution sulphurous acid and hydrochloric acid undergo no change; but, "in a dry state, on the contrary, they are rapidly decomposed, at least in operating over mercury." It may be true that as Gmelin states, water, chlorine, and sulphur result, but such is not the assertion of Dumas. The point, however, is immaterial, since as Dumas and Gmelin state, and as every chemist knows, the two gases remain unaltered in the presence of water, even if in the form of vapour. Indeed, it happens, unfortunately for both of Mr. Forbes's objections, that large quantities of precisely such an atmosphere as he supposes to be impossible, are disengaged from numerous volcanic vents, as he will find by referring to the researches of Charles Deville and Leblanc. (Ann. de Ch. et Phys. [3] lii. pp. 5—63). Among other examples described by these chemists, a *fumerolle* of Vesuvius yielded in June, 1856, a mixture of highly heated steam, hydrochloric acid and air, the latter containing in 100 parts, oxygen 18·7, sulphurous acid 2·6, the remainder being nitrogen; while the acids of the steam and air together yielded, for one part of sulphurous acid, about five parts of hydrochloric acid. Traces of sulphuric acid, due to the slow union of the sulphurous acid and oxygen, were found in the water condensed from this fumerolle. Volcanoes, as I have elsewhere stated, reproduce, on a limited scale, the conditions of the primeval earth, not only in their solid but in their gaseous products.

Mr. Forbes proceeds to comment upon my illustration of the condition of the primitive globe from a supposed reaction of the present air, sea and land under the influence of intense heat. He suggests that the carbonaceous matters would convert into sulphides the mineral sulphates. Here, as before, he ignores the intervention of water and siliceous matters, which would cause the sulphur to escape in the form of sulphuretted hydrogen (which is doubtless evolved from modern volcanoes by a similar reaction), and this at an elevated temperature, would at once be burned to sulphurous acid and water. He descends to trifling when he objects that by the effect of heat upon the present surface of the globe, the water of the sea would be first evaporated, and then the chloride of sodium sublimed in its turn. It was made clear to every reader, that I never intended by this illustration to represent the process of nature; moreover, I said, "*if the elements were made to react upon each other*," which would not be the case if they were successively removed by evaporation beyond the sphere of reactions.

Here I cannot resist the temptation of giving my readers a choice specimen of Mr. Forbes's chemistry, which he has embodied, with many other surprising things, in a further criticism of my lecture, which appears in the *Geological Magazine* for October, but has been, for some unknown reason, withheld from the readers of the Chemical News. Proceeding to give his own notions of the chemistry of the primitive globe, Mr. F. supposes that immediately above the "solidified crust," there existed a zone composed chiefly of chloride of sodium; "above this, a stratum of carbonic acid, and then of water in the form of steam, whilst the oxygen and nitrogen would be elevated still higher;" and, probably, also, in Mr. F.'s imagination, separated according to their densities. In explanation of this order, he tells us, in a note, that the zone of carbonic acid gas would be heavier than that of steam, and should therefore come below it; he even gives their respective weights, but he forgets that oxygen and nitrogen are also both heavier than steam, and should be found below, and not above, this zone of watery vapour. In fact, as is well known, the specific gravity of oxygen being 1·109, and nitrogen 0·970, that of atmospheric air is 1·000; while carbonic acid gas is 1·525, and that of watery vapour 0·624. But, apart from this absurd mistake, what shall we say of the man who displays an utter ignorance of the laws which govern the diffusion of gases and vapours? Will Mr. Forbes explain why it happens that in our present atmosphere, these same elements, namely, oxygen, nitrogen, carbonic acid gas, and watery vapour, are commingled, instead of being, as he would have them, arranged in separate zones?

Mr. F.'s mode of explaining the saltness of the sea must fall to the ground, unless he succeeds in showing how, despite well known chemical affinities, the requisite amount of chloride of sodium could be formed and preserved under the conditions which I have discussed above, so that, as he supposes, it was ready to be dissolved by the first waters precipitated on the surface. When he has satisfactorily established this part of his theory, he will, perhaps, tell us how sulphates found their way into the sea, if, as he asserts, all the sulphur was at first separated in the form of dense metallic sulphides, which sank at once, "and remained in the interior of the earth, protected from oxidising action?" Mr. F may have data unknown to the world, for estimating the total amount of sulphur in the globe; but when he tells us that it would be sufficient to convert all the soda of the sea to sulphate, he reasons as if the amount of bases in nature were limited, forgetting that the earth's crust contained more than enough of alkalies, lime, and magnesia, to saturate the acids of the primeval atmosphere, and, moreover, that the whole of the sulphur, sulphates, and sulphides of the earth's crust, have, to judge from all analogy, been derived from the soluble sulphates of the ocean.

Mr. F. next proceeds to enquire why the sea contains so much sodium, and so little potassium? If he will study the question, as he may do in my *Contributions to the Chemistry of Natural Waters* (Amer. Jour. Sci. [2] xxxix., 176; xl. 43, 193), he will learn that at an early period the salts of calcium and magnesium greatly predominated over those of the alkalies in the ocean waters, precisely as they must have done in the crust of the primitive earth. It is by subsequent subaerial decomposition that have been liberated the alkalies, which, in the form of carbonates, have decomposed the salts of the primitive sea, and substituted sodium for calcium, for it is well known that natural alkaline waters convey to the sea chiefly soda, and comparatively little potash, which is retained by argillaceous sediments. Moreover, the potash which does find its way to the sea, is constantly withdrawn in the form of glauconite, and also by the agency of fucoids, which as Forchammer has shown, fix great amounts of potash, and, subsequently, by their decay in the ooze. restore it to the earth.

Mr. Forbes next expresses surprise that I find the origin of all carbonate of lime (except that from the subaerial decomposition of primitive calcareous silicates) in the reaction of carbonate of soda on the lime-salts of sea-water, since, according to him, the results of the careful study of limestone rocks by geologists, palæontologists, and microscopists have shown these rocks to be "*the result of organic action.*" And, moreover, that neither chemists nor zoologists will accept my assertion that *animals can only appropriate the carbonate of lime which they find ready formed*, but "will consider these animals capable of utilising the other lime-salts in the sea." If we admit the power of the lower animals to decompose chloride of calcium or sulphate of lime, as would appear from the acid liquid said to be found in some of them, will Mr. Forbes tell us what becomes of this at the death of these animals, and how the acid is to be disposed of? If the thousands of feet of limestone strata, consisting in large part of organic remains, have been derived from the decomposition of the sulphate or chloride of calcium of the sea by any other process than by that which I have indicated, namely, the intervention of alkaline carbonates, will Mr. Forbes kindly inform us what has become of the vast amount of hydrochloric acid equivalent to all this carbonate of lime?

As to the origin of dolomites, Mr. Forbes will do well to read my paper in the *Amer. Jour. Science*, for July, 1866 ([2] xlii. 49). In this, at § 112, he will see that apart from the formation of stratified sedimentary dolomites, I insist upon the frequent occurrence of dolomite as a mineral of secondary deposition, lining drusy cavities, filling veins, and even the moulds of fossil shells. To such cases, the observations of Sorby and of Harkness may probably be referred; the microscopical investigations of the former, as given by him in the British Association Report for 1856, are, like all the other works of that excellent observer, doubtless entitled to the highest credit. No one, however, who has carefully studied, as I have done, the distribution and association of the great beds of dolomite which occur in the Lower Silurian rocks of Canada and New England, can for a moment admit that they are the products of subsequent alteration. Repeated alternations of pure blue lime-stones with reddish ferruginous dolomites, interrupted beds and patches of these enclosed in the former, the line of demarcation sharply drawn, and finally conglomerates in which pure limestone pebbles are enclosed in beds of dolomite, all of which may be studied near Quebec, are evidences incontrovertible against the theory of dolomitization of pure lime-stones, and in favour of the deposition of dolomites as magnesian sediments.

Mr. Forbes insinuates that I am unaware of the various speculations and theories which have been put forward to explain the supposed origin of dolomites by alteration. Although the stratigraphical relations of dolomites, as described above, set aside entirely this hypothesis of its formation, at least in the great majority of cases, Mr. Forbes will find that the observations and speculations of Haidinger, Von Morlot, Marignac, and others on this subject, have been discussed and made the subject of multiplied experiments by me in a memoir published in 1859 (*Amer. Jour. Sci.* [2] xxviii. 170, 365), and farther in the paper quoted above; and that I have shown that the reaction of the sulphate of magnesia on carbonate of lime, to which he refers, does not give rise to dolomite, but to an admixture of the carbonates of lime and magnesia.

Some of the results of my prolonged study of certain salts of lime and magnesia, which are for the most part set forth in the papers just referred to, were, says Mr. Forbes, by me considered worthy of being presented to the French Academy of Sciences (*Comptes Rendus*, April 22, 1867), although he declares the reactions there described to have been for twenty-five years in general application on a large scale in Great Britain, for the manufacture of magnesian salts. Here it becomes difficult to admit the plea of ignorance which suggests itself for most of Mr. Forbes's previous statements. I have, in the note to the French Academy above referred to, pointed out the following as facts discovered by my investigations of the salts of lime and magnesia: 1st. That bi-carbonate of lime, at ordinary temperatures, decomposes solutions of sulphate of soda and sulphate of magnesia, with formation of sulphate of lime and bi-carbonates. 2nd. That from mingled solutions of sulphate of magnesia and bi-carbonate of lime, there separates, by evaporation, crystalline gypsum, and subsequently a hydrous carbonate of magnesia; the bi-carbonate of this base being, as is well known, very much more soluble than the sulphate or the bi-carbonate of lime. 3rd. That this separation of gypsum is favoured and rendered more complete by an atmosphere impregnated with carbonic acid gas; and 4th. That mixtures, in due proportions, of precipitated carbonate of lime and hydrous carbonate of magnesia, when gently heated under pressure, and in the presence of water, unite to form the anhydrous double carbonate, dolomite. These are the reactions which I described to the French Academy as *new*, and I demand Mr. Forbes to make good his assertion to the contrary, or to show that any one of them has been employed for the last twenty-five years in the manufacture of magnesian salts.

Montreal, December, 1867.

ON THE IDENTITY OF THE

BODY IN THE ATMOSPHERE WHICH DECOMPOSES IODIDE OF POTASSIUM, WITH OZONE.

BY THOMAS ANDREWS, M.D., F.R.S.

It was assumed for many years, chiefly on the authority of Schœnbein, that the body in the atmosphere which colours iodide of potassium paper is identical with ozone; but this identity has of late been called in question, and as the subject is one of considerable importance, I submitted it lately to a careful investigation, the results of which I beg to lay briefly before the Society. The only property of ozone, hitherto recognised as belonging to the body in the atmosphere, is that of setting free the iodine in iodide of potassium; but as other substances, such as nitric acid and chlorine, which may possibly exist in the atmosphere, have the same property. no certain conclusion could be drawn from this fact alone

One of the most striking properties of ozone is its power of oxidising mercury, and few experiments are more striking than that of allowing some bubbles of electrolytic oxygen to play over the surface of one or two pounds of mercury. The metal instantly loses its lustre, its mobility, and its convexity of surface, and when moved about it adheres in thin mirror-like films to the sides of the containing glass vessel. The body in the atmosphere acts in the same way upon pure mercury; but from the very minute quantity of it which is at any time present, the experiment requires some care in order that the effect may be observed. On passing a stream of atmospheric air, which gave the usual reaction with test-paper, for some hours over the surface of mercury in a U-tube, the metal was distinctly oxidised at the end at which the air first came into contact with it.

This experiment, however, cannot be considered conclusive, as mercury will tarnish and lose its mobility under the influence of many bodies besides ozone.

It is well known that all ozone reactions disappear when ozone is passed through a tube containing pellets of dry peroxide of manganese, or other body of the same class. The same thing occurs with the substance supposed to be ozone in the atmosphere. About eighty litres of atmospheric air were drawn, at a uniform rate, through a tube containing peroxide of manganese, and afterwards made to play upon very delicate test-paper. Not the slightest coloration occurred, although the same paper was distinctly affected when ten litres of the same air, without the interposition of the manganese tube, were passed over it.

But the action of heat furnishes the most unequivocal proof of the identity of the body in the atmosphere with ozone. In a former communication (*Phil. Trans.* for 1856, p. 12), I showed that ozone, whether obtained by electrolysis or by the action of the electrical brush upon oxygen, is quickly destroyed at the temperature of 237° C. An apparatus was fitted up, by means of which a stream of atmospheric air could be heated to 260° C. in a globular glass vessel of the capacity of five litres. On leaving this vessel, the air was passed through a U-tube, one metre in length, whose sides were moistened internally with water, while the tube itself was cooled by being immersed in a vessel of cold water. On passing atmospheric air in a favourable state through this apparatus, at the rate of three litres per minute, the test-paper was distinctly tinged in two or three minutes, provided no heat was applied to the glass globe. But when the temperature of the air, as it passed through the globe, was maintained at 260° C., not the slightest action occurred upon the test-paper, however long the current continued to pass. Similar experiments, with an artificial atmosphere of ozone, that is, with the air of a large chamber containing a small quantity of electrolytic ozone, gave precisely the same results. On the other hand, when small quantities of chlorine or nitric acid vapour, largely diluted with air, were drawn through the same apparatus, the test-paper was equally affected, whether the glass globe was heated or not.

From these experiments I consider myself justified in concluding that the body in the atmosphere, which decomposes iodide of potassium, is identical with ozone.—*Proceedings of the Royal Society.*

ON SOME POINTS IN CHEMICAL GEOLOGY.

BY DAVID FORBES, F.R.S., ETC.

No. II.—Dr. Sterry Hunt's Geological Chemistry.

In the Chemical News of October 4, 1867 (*Amer. Repr.*, Dec., 1867, p. 281), I commenced some remarks under this title, for the express purpose of exciting more interest in the application of chemistry to geology, and with the hope of starting a discussion, which might at the same time enliven as well as elucidate the subject. Accepting Dr Hunt's invitation, his views, being the most recent, were first selected for consideration; and, although that gentleman now appears greatly astounded at my presuming to differ from his opinions, it is still highly gratifying to find that he has at last condescended to reply.

As this reply, however, contains absolutely nothing which can in any way affect or modify the opinions which I have already expressed on the views of Dr. Hunt, or even require a reconsideration of the arguments upon which those opinions were based, I am enabled to reply *tout de suite*.

Dr. Hunt adopts a line of argument which is an elaborate attempt to convince his readers of the utter incompetency and ignorance of his reviewer; yet at the same time, it is amusing to observe that the character and tone of his remarks, in conjunction with his studious avoidance of some of the knotty points, and more important arguments brought forward in opposition to his views, are strikingly suggestive of his being in reality ill at ease, and possibly afflicted with a presentiment that there may, after all, be some rickety points in his theoretical views.

Men who live in glass houses should not throw stones; Dr. Hunt's accusations of ignorance will appear strange to those who have paid attention to some of his sweeping assertions; amongst others, for example, when he emphatically declares that quartz "can only be generated by aqueous agencies," geologists will infer that Dr. Hunt must be ignorant of the most important fact that quartz is found in abundance in volcanic lavas in many parts of the world, although not in Canada.

Had Dr. Hunt remained content with his Canadian laurels, he would probably have enjoyed them in peace without having his opinions disputed, but when he now aspires to be recognised in Europe, he cannot complain if his views be criticised by any, or all, of those interested in the subject; an ordeal which must be undergone before he can expect them to receive general acceptance, for surely he does not issue them as axioms or oracles.

Europe differs greatly from Canada, and, amongst other things, in close competition being the order of the day. No man in Europe can expect to retain any portion of the field of science exclusively for himself, or to travel alone on any of the many different roads which lead to one and the same scientific truth.

If real progress is to be made in science, the student must reason for himself and not be content with accepting, merely on authority, opinions which are inconsistent with his own deductions or experiments; nor should he be deterred by the opposition to be expected from those already in office or authority, who are sure to be jealous of intruders on what they imagine to be their own domain, and, doubtless, also dislike having their peace of mind disturbed by innovations.

A discussion of this nature may be carried on in two ways; either by considering the main points of the argument first, before engaging the minor details, or the reverse. Dr. Hunt prefers the latter course, which no doubt is best suited to the defence of a weak cause, but which, as his rather rambling remarks in last week's Chemical News [*] (*Amer. Repr.*, March, 1868, page 107) will show, is not calculated to convey to his readers any very clear idea of the exact points at issue, and likely to confuse by the number of minor details having little or no bearing on the main questions.

It is therefore most important to me that no misunderstanding should arise as to the exact points on which I have presumed to differ from the principles of chemical geology which Dr. Hunt has recently brought before the scientific public in Europe.

Expressed in as few words as possible, I object to the following of Dr. Hunt's assumptions or assertions:

1. That the earth is solid to the core.
2. That the surface of the earth immediately previous to its entire solidification was "a liquid bath of no great depth surrounding the solid nucleus."
3. That the original atmosphere contained "the whole of the chlorine in the form of hydrochloric acid —the sulphur as sulphurous acid."
4. That the saltness of the sea is due to a rain of hydrochloric acid "flooding the half-cooled crust" with a highly heated acid deluge.
5. That the whole of "the calcareous strata—the marbles and various limestones which we find on the earth's surface"—have been precipitated from the sea by carbonate of soda.
6. That all the magnesian limestones and gypseous beds were formed in a dense atmosphere of carbonic acid.
7. That quartz "can only be generated by aqueous agencies."
8. "That granite is in every case a rock of sedimentary origin."
9. That volcanic rocks are merely ordinary sedimentary beds melted by being "depressed so that they come within the action of the earth's central heat."

Any minor differences fall naturally under these heads, and I may add that the perusal of Dr. Hunt's defence has confirmed me more than ever in the belief that the above premises are unsound; and I shall now endeavour as concisely as possible to examine the arguments *pro et contra*.

1. That the earth is solid to the core.

Dr. Hunt seems to imagine that if the earth is not solid to the core, it can only consist of an immense central sphere of molten matter covered by a thin external crust or shell: for he wastes all his arguments in attempting to upset this theory, to which I had never given my adhesion. I have preferred adopting in the main the hypothesis of Bunsen, no mean authority, and when opposing Dr. Hunt's view, simply asserted my opinion, that the earth still encloses "a vast reservoir or reservoirs of still fluid igneous matter in its interior," and the main argument with which I support this opinion is, that I consider that the molten lava ejected from volcanoes must be derived from some such source. This is a very simple but common sense view of the case, which I imagine Dr. Hunt will find some difficulty in refuting.

2. That the earth's surface immediately previous to its entire solidification was "a liquid bath of no great depth surrounding the solid nucleus."

Hopkins has taken into favourable consideration, the supposition that the earth actually was solid, both in its centre and crust, and yet might retain fluid igneous matter in the intermediate space, and taking a somewhat similar view of the case, I believe that, even allowing that the solidification actually did commence at the centre, that it still could not have reached the exterior before, on the other hand, the surface itself had also solidified and formed a crust commencing from the exterior, due to the external cooling action. In opposition to this, Dr. Hunt states that silicates when cold are from one-seventh to one-sixteenth part more dense than when molten, and would at once sink down into the fluid mass below, and further adds that no crust could be formed unless the laws of gravity were suspended. I do not know what Dr. Hunt's ideas of the laws of gravity may be: but would again ask how far he imagines a crust of sp. gr. 2·6 could sink down into a molten sphere of a mean sp. gr. 5·3?

I will not, however, repeat the other arguments which I have used in the *Geological Magazine*, but content myself by bringing forward one not before employed by me in support of my opinion.

Some experiments which I am now engaged in, on the effect of heat upon bodies which contract in cooling, *i. e.*, which are more dense when cold than when molten, show in the cases tried, that a body upon the first application of heat expands and continues to do so up to near its melting point, when it contracts at the instant of fusion; in other words, although the substance when cold was heavier than when molten, yet the same

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[*] It is necessary to explain here that many of Dr. Hunt's observations refer to a previous communication in the October number of the *Geological Magazine*, and not to the subsequent one in the Chemical News of October 4th (*Amer. Repr.*, Dec., 1867, p. 281), which, as is distinctly stated therein, is only supplementary to the former, and to be read in conjunction with the same. Yet Dr. Hunt indulges in the absurd accusation that the contents of that communication "have for some unknown reason been withheld from the readers of the Chemical News."

substance expanded by heat was lighter than when molten. Thus some metals were found to float about, (like ice upon water) upon the surface of a molten bath of the same metal, into which they were placed in a heated condition.* It appears probable that the same phenomena would account for such a crust as Dr. Hunt disputes, not sinking, but floating on the molten bath below.

That the earth may possibly have solidified at the centre first, is not disputed by me, nor does its so doing in any way affect my theoretical views. The object of my observations on this head were to show that we are altogether too ignorant of the character of the central mass of the earth, and of the effect likely to be produced by such enormous pressures, to be enabled to reason on such insufficient data with any confidence in the result.

3. That the original atmosphere contained "the whole of the chlorine in the form of hydrochloric acid—the sulphur as sulphuric acid."

The perusal of Dr. Hunt's remarks does not in any way tend to modify the conclusions I had previously arrived at on this head; I still believe that chemists will not be disposed to regard an atmosphere containing enormous volumes of sulphurous acid, steam, and oxygen in excess, or in other words, which resembles a great sulphuric acid chamber, as *probable*, and as Dr. Hunt does admit that they would slowly unite to form sulphuric acid, it merely becomes a question of time as to whether they united slowly or quickly.

The arguments I advance against supposing that such an atmosphere ever did exist, are that I consider that the sulphur would unite mainly with the heavier metals, and the chlorine mainly with the alkaline metals, and I consequently infer, that these elements never went into the atmosphere in any such quantity as Dr. Hunt imagines.

Dr. Hunt in opposition states that sulphides could not be formed, since oxygen was in excess. Metallurgists know that sulphides are far less easily oxidisable than are generally imagined, and that they are produced in both blast and air furnaces, where the waste gases still contain unconsumed oxygen; and that time is an important element in this consideration.

But we have no proof whatever of any great excess of oxygen in the primeval atmosphere; on the contrary, we know that a vast amount of the oxygen now present in the air, must have been derived from the decomposition of the carbonic acid, when the immense supplies of carbon, afterwards buried in the various sedimentary formations, were extracted from the atmosphere by the action of vegetable life. The slight excess of oxygen which no doubt was present would further be so diffused through the enormous volume of carbonic acid, nitrogen, and aqueous vapour, that it cannot be imagined to have exercised other than a most feeble oxidising action.

The carbonic acid, also, being so infinitely more dense, and present in so overwhelming quantity, would further act as a powerful shield against the very oxidising action which Dr. Hunt lays so much stress upon.

That the chlorine, also, did not go into the atmosphere as Dr. Hunt imagines (combined with hydrogen as hydrochloric acid), I infer, from the well known greater affinity which it has for sodium than for hydrogen, and the volatility of sodium would be far more likely to bring it in contact with the chlorine than with the silica.

The idea that the action of the feeble excess of oxygen above alluded to, in connection with silica and steam, would prevent the formation of chloride of sodium, is not of much weight; since the chloride of sodium would be formed as a vapour in the atmosphere, whilst the silica remained below in the earth's mass, in the solid form.

But Dr. Hunt next writes, "Even if, as Mr. Forbes supposes, the chloride of sodium were to be formed in the heated athmosphere, it would be precipitated into the intensely heated bath," &c.; *precipitated!* when it would be in the state of vapour at this temperature.

Metallurgists know how indifferent chloride of sodium is when fused with silicates, and to this property is due the employment of what is termed a salt cover in assays; however well the salt may be intermixed, once the mass is fused, it rises and swims on the top, and (if the heat be not too elevated, or protracted, as to volatilize it entirely) presents, upon cooling, a well-defined crystalline crust of salt, below which is found the unaltered silicate slag, and below this again the button of metal, pure, or more or less in combination with sulphur, arsenic, antimony, &c., as the case may be; thus presenting, on the small scale, an illustration of what I have supposed may have occurred in nature, in which case, also, the cover, or crust of salt, would act as a shield against oxidation.

In a potter's kiln, the vapour of salt under confinement, merely glazes the surface of the ware to a minute depth, and this very glaze protects the silicates from further action; but both the potter's kiln and Gossage's soda process are worked under forced circumstances, not applicable in this argument; and when Dr. Hunt explains that in his illustration of this subject, he merely used the words "*if the elements were made to react upon one another*," is it not rather he who is trifling with the subject when he supposes conditions which never could occur in nature in the case referred to.

4. That the saltness of the sea is due to a rain of hydrochloric acid, "flooding the half-cooled crust" with a highly heated acid deluge.

This assumption requires no further comments than those included under the preceding head, where I have endeavoured to show that the whole of the chlorine did not ascend into the atmosphere as hydrochloric acid, and, consequently, could not flood the earth with the hot acid deluge insisted on by Dr. Hunt.

5. That the whole of "the calcareous strata, the marbles, and various limestones which we find on the earth's surface" have been precipitated from the sea by carbonate of soda.

Geologists have long agreed that sedimentary limestones are the products of the action of organic life, and microscopists, in confirming this, have further proved that they do not possess the character of precipitates. Dr. Hunt evades any reply to these objections, but asks a question in return; requesting to know what becomes of the acid in case, as I contend, animals can utilise the salts of lime contained in the sea. As is well known, sulphur plays a very important part in vital economy, entering both into the composition of organism, and being also given off as sulphuretted hydrogen in the gaseous form. I see, therefore, many reasons for believing that animals do assimilate the sul-

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* As a metallurgist I have frequently observed such cases, but for a long time did not understand the explanation; I have to thank my friend Mr. Hackney for directing my attention to the behaviour of Bessemer steel under these circumstances, as it gives much trouble to the workmen by persistingly floating high on the surface of the melted steel (even when in pieces of 40 pounds and more) as long as its temperature is below its fusing point.

phate of lime, which we know is contained in such an enormous quantity in the ocean.

6. That all the magnesian limestone and gypseous strata were formed in a dense atmosphere of carbonic acid.

In 1846, when in Birmingham, I was informed that for some years the manufacture of magnesian preparations was based upon the reactions of the compounds of magnesia with carbonic acid in a compressed atmosphere of carbonic acid. In 1849, Mr. Osborne, a gentleman connected with a similar manufactory in Ireland, fully confirmed these statements, and shortly after the publication of Dr. Hunt's paper in the *Comptes Rendus*, Dr. Lawson, in the course of conversation, expressed his surprise at Dr. Hunt being unaware of this, since he knew that the principle had long been in use in a manufactory at Cork.

Dr. Hunt has further applied this principle,* and obtained very interesting results, which he considered to be the counterparts of nature's operations; and remembering that there are dolomite beds in the lower silurian strata of Canada, at once asks geologists to believe the rather hasty generalisation that all the magnesian limestones and gypseous beds were formed in a dense atmosphere of carbonic acid.

Geologists, however, well knowing that the grand development of magnesian limestones and gypseous strata occurred in periods when air-breathing animals existed on the surface of the globe, could not believe that these animals actually lived in a dense atmosphere of carbonic acid; and had some of the more modern great gypseous formations occurred in Canada, Dr. Hunt would probably not have brought forward this theory.

7. That quartz "can only be generated by aqueous agencies."

Dr. Hunt wisely, no doubt, does not take any notice of my arguments against this assertion, since they are facts, not opinions; and consist merely in pointing out that the volcanic lavas of Italy, Hungary, Peru, Bolivia, Chili, &c., contain abundance of quartz, often in well-defined crystals. In connection with this, I may here extract a passage from a letter received from Mr. Sorby, who writes: "I have splendid cases of recent lavas with quartz, both in the shape of small crystals and as rounded masses, like those seen in some older rocks, and this quartz, in both cases (crystals and rounded masses), contains splendid glass cavities just like those in the felspars, the Arran pitchstone, and the various lavas; thus we have complete proof, according to my views, that quartz both can and has crystallised out from a melted mass of rock."

Now, in face of such facts, what importance, may I ask, can be attached to such of Dr. Hunt's dogmatic assertions as "that the composition of the primitive crust would have excluded free silica;" that quartz is "only the result of a secondary process," &c.?

8. "That granite is in every case a rock of sedimentary origin."

Dr. Hunt makes this assertion in opposition to the opinion of many able men who have well studied the subject. If he, however, only founds this opinion on the presence of quartz in granite, the value to be attached to it may be inferred from the remarks contained in the preceding paragraph.

If he speaks as a geologist, it may fairly be inquired whether he considers his Canadian experience sufficient to enable him to arrive at such sweeping generalisations.

Sir Charles Lyell has stated that three things were essential to a geologist, namely, "to travel, to travel, and to travel;" and such advice may be recommended to Dr. Sterry Hunt before he ventures again to generalise for the world on the strength of a local knowledge of a very minute part of the same.

9. That volcanic rocks are ordinary sedimentary beds melted by being "depressed so that they come within the action of the earth's central heat."

In the *Geological Magazine* I ventured to inquire of "the author of this ingenious theory, by what mechanical arrangement he supposes strata on the surface of the earth to be lowered down into a globe solid to the core;" and again, "How are we, according to this theory, to account for the fact, that volcanic rocks, taken from any quarter of the world, no matter how far distant from one another—from Iceland or Terra del Fuego, from the islands of the West Indies, or from those of Polynesia,—that in all cases such rocks possess an absolute identity in chemical and mineralogical composition, in physical and optical properties? Can any geologist be expected to believe that such rocks have been formed by the melting up of a mere mechanical aggregate of rock débris, possessing no analogy whatsoever, and whose chemical composition, &c., is known to vary to the widest imaginable extremes?" Questions as yet unanswered.

Before concluding these remarks, I would here acknowledge that Dr. Hunt has discovered an inaccuracy which occurs in my communication to the *Geological Magazine*, where the position of steam in the imaginary original atmosphere is by accident placed below that of air, although steam is in reality lighter, as a moment's reflection would have shown. This error has not the most minute influence on any of my generalisations, as it is perfectly immaterial whether this stratum be above or below that of air.

I shall always be ready to admit at once any error which may be found in my communications; still Dr. Hunt is quite entitled to make the most of such a blunder if he thinks it will support his views; at the same time I trust that he will also be equally candid in cases where he may be found tripping.

Dr. Hunt alludes to a rough sketch of some of my views contained in the *Geological Magazine*; but, as I have already accepted the invitation of the Council of the Chemical Society to give a lecture on chemical geology (20th February next), Dr. Hunt will thus be enabled to take my views into full consideration, and after comparing them with his own, I trust will give us the benefit of his scrutiny; for, as I regard the ultimate object of all my labours as being the attainment of scientific truth, I am as fully prepared to be corrected in points where I may be proved to be wrong, as to defend those which I hold to be right.

London, January 20, 1868.

ON THE INFLUENCE OF

APERTURE IN DIMINISHING THE INTENSITY OF THE COLOUR OF STARS.*

BY JOHN BROWNING, ESQ., F.R.A.S.

At the last meeting of the Society some remarks were made on the subject of the amount of colour visible on the moon during the late lunar eclipse.

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* *Vide* Chemical News, Sept. 13, 1867, p. 148 (*Amer. Repr.*, Nov., 1867, p. 276).

* Proceedings of the Royal Astronomical Society.

I had previously stated that I had failed to detect either the coppery or the blue tints generally seen during the occurrence of this phenomenon. As my observation did not agree with those of several well-known observers, I have given the matter some attention, and endeavoured to ascertain from what cause the discrepancy proceeded.

Mr. Slack suggested that probably my having used a telescope of larger diameter than those employed by most of the observers would prove the explanation desired, and since then I have heard that our able Secretary, Mr. Huggins, is of the same opinion. The result of my inquiries completely confirms this suggestion. I find that while most observers who use telescopes of only three or four inches aperture speak of the colour as being less than usual, yet very noticeable, observers who use telescopes of seven or eight inches aperture saw very little colour. Neither Mr. Barnes nor myself, observing with a 10½-aperture, nor Mr. With or his nephew, employing a 12¼-inch silvered-glass speculum, could detect any colour at all.

It is true that I failed equally in detecting colour with a 4-inch object-glass, but I account for this by supposing that the sensitiveness of my eye to faint-coloured light had been injured by the glare of the moon in the large aperture. Experimenting in connection with this subject, I have noticed that the chocolate colour of the so-called belts of *Jupiter* is much more perceptible with 6-inches aperture than with 12 inches. Again, a small star in the cluster in *Perseus* appears of an indigo-blue with 8½ inches, Prussian-blue with 10½ inches, and royal-blue with 12¼ inches of aperture. It follows from this that colours estimated by comparison with the ingenious chromatic scale of Admiral Smyth, in which each colour is represented of four different degrees of intensity, will not possess any relative value unless taken in connection with the aperture employed when the colour was estimated. Were due allowance made for this disturbing influence of variation of aperture, I think many discrepancies between the colours attributed to double stars by different observers might probably be reconciled.

Note.—An enlarged diagram of Smyth's chromatic scale, and another showing the apparent difference in the colour of a star when seen with apertures of 4 inches and 12 inches, was exhibited and described at the time the paper was read.

OBSERVATIONS ON THE

NATIVE HYDRATES OF IRON,

BY GEORGE J. BRUSH.

WITH ANALYSIS OF TURGITE,

BY CHARLES S. RODMAN.

The well known iron mines of Salisbury, Conn., have long enjoyed a reputation among mineralogists as furnishing superior specimens of *limonite*, and hitherto this has been thought to be the only ferric hydrate occurring in quantity at this locality. Minute crystals of supposed Göthite have occasionally been found, but not in quantity sufficient to render certain their mineralogical determination.

On a recent visit to these mines Mr. Rodman obtained a considerable number of specimens, lining pockets in the ore, which had the usual brilliant metallic lustre on the interior surface, and showed on the fracture a fibrous structure, but differed from brown hæmatite in having a decidedly red colour, and in affording when pulverised a red powder, closely resembling that of ordinary red hæmatite. This red layer was in some cases an inch or more in thickness, and was deposited on a bed of limonite (brown hæmatite);—the line of demarcation between the brown and the red ore was so perfect, in most instances, as to readily admit of a complete separation of the two minerals.

An examination of this red ore showed it to be an oxide of iron, containing not far from 5 per cent. of water, a number of specimens yielding very uniform results; and a complete analysis proved the mineral to be a ferric hydrate with the formula $Fe_2O_3 2HO$, identical with the *Turgite* of Hermann,* and with Breithaupt's *hydro-hæmatite*, as analysed by Fritzsche.† The physical characters are so nearly those of ordinary anhydrous hæmatite that it is difficult to distinguish the species without having recourse to an estimation of the loss on ignition. The turgite yields an abundance of water when heated in the closed tube, and it decrepitates in a remarkable manner. Hardness, about 5·5. G.=4·14. For analysis the mineral was carefully dried over sulphuric acid until of constant weight, and this desiccated mineral was then heated for several hours in an air bath at 100° C. without showing any further diminution of weight. The amount of hygroscopic moisture abstracted from the air-dried mineral by treatment in the desiccator was 1·40 per cent. The iron in one instance was determined by titration with permanganate of potash; in the second case it was thrown down by ammonia, the precipitate washed, dried and weighed, and then the iron was separated from the silica and alumina by Deville's method by first reducing with hydrogen, and subsequently volatilising the iron by heating in a current of dry hydrochloric acid gas. The analytical results were all obtained by Mr. Rodman. Composition:

	1.	2.	Mean.
Ferric oxide............	91·45	91·29	91·36
Manganic oxide.........	0·67	0·55	·61
Alumina................	0·75	....	·75
Silica..................	0·22	0·24	·23
Phosphoric acid, sulphuric acid, and cobaltic oxide	traces	....	traces.
Insoluble in acid........	1·83	....	1·83
Water................	5·20	5·21	5·20
	100·12		99·98

Other determinations of water on different specimens gave 5·02 and 5·09 per cent.

Five grammes of the mineral yielded only minute traces of sulphuric acid, and three grammes showed but an unweighable trace of phosphoric acid. A very perceptible trace of cobalt was found even on examination of one gramme of the mineral. The portion insoluble in acid proved on analysis to consist entirely of silica, and excluding this, with the small amount of silica and alumina found in the soluble portion, the result of the analysis is

$$\begin{aligned} &\mathrm{Fe_2O_3}\ 94{\cdot}00 \qquad\qquad \mathrm{Mn_2O_3}\ 0{\cdot}63 \qquad\qquad \mathrm{HO}\ 5{\cdot}35 = 99{\cdot}98 \\ \text{Oxygen, } &\underbrace{28{\cdot}20 \qquad\qquad\qquad\qquad 0{\cdot}19}_{28{\cdot}39} \qquad\qquad 4{\cdot}75 \end{aligned}$$

giving the oxygen ratio 6 : 1 or $Fe_2O_3 2HO$.

This result confirms the conclusions of Hermann and Breithaupt, that there is a native ferric-hydrate with one-half an equivalent of water. Several years since the attention of the writer was called to this subject

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* Journal für praktische Chemie, xxxiii. 97.

† Breithaupt, Vollständiges Handbuch der Mineralogie, iii., 840.

by Prof. W. T. Roepper of Bethlehem, who stated that he had found Breithaupt's hydrohæmatite to be of frequent occurrence with the limonite ores of the Lehigh Valley. A water determination on the Lehigh mineral made by Prof. Roepper, and kindly communicated for this article, gave 5·34 per cent., and Prof. Roepper calls especial attention to the characteristic decrepitation of this mineral when heated. On examination of the specimens of limonite in the Yale College collection, a fine specimen of the red hydrate was found occurring with the limonite of Düsseldorf in Prussia. This yielded on examination by Mr. Rodman 4·75 per cent. water. Another specimen was found from Ioditz in Bavaria, besides numerous specimens from Salisbury in Connecticut. A mineral of like composition has also been found by Bergemann * at the Louisa Mine near Horhausen in Prussia. From these numerous localities it would appear that the mineral is of common occurrence. It has heretofore been confounded by most mineralogists with hæmatite, which it so strongly resembles in physical characters. It may be readily distinguished from hæmatite by simply heating a fragment in the closed tube, when it decrepitates violently and gives off a large amount of water.

Hermann does not give the pyrognostic characters of *turgite*, but Breithaupt, in his description of *hydrohæmatite*, makes particular mention of its characteristic decrepitation when heated. The turgite is described by Hermann as being associated with copper ores; its chemical composition is, however, identical with hydrohæmatite, and as it has priority of publication, the species must bear the name of turgite, and hydrohæmatite be used only as a synonym.

We have, therefore, three well-defined hydrates of iron occurring native and forming three distinct and well-established mineral species, differing from each other in physical characters and in their relative content of water.

Turgite	$Fe_2O_3 + \frac{1}{2}HO$
Göthite	$Fe_2O_3 + HO$
Limonite	$Fe_2O_3 + 1\frac{1}{2}HO$

Two other hydrates have been described containing respectively two and three atoms of water. Murray * found in a brown iron ore from Huttenrode in the Hartz—

Fe_2O_3 81·41, HO 17·96, SiO_3, 0·17, Carbon 0·46=100, giving the formula $Fe_2O_3 + 2HO$

A compound of similar composition from Kilbride in Ireland, having a pitchy colour, analysed by Haughton, gave $Fe_2O_3$77·15, HO 20·43, $SiO_3$0·30, Al_2O_3 tr., PO_5 1·60=99·48.

Xanthosiderite also appears to be a mineral of like composition, but its mixture with a silicate of unknown composition renders it difficult to conclude positively that it belongs here.

A. H. Church † has analysed a stalactite of a rust-coloured ferric-hydrate from Botallack mine in Cornwall, which gave:—

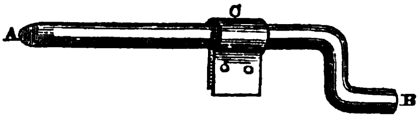
$Fe_2O_3$73·70, HO 24·40, loss, PO_5, and organic matter 1·76=100, giving the formula $Fe_2O_3 + 3HO = Fe_2O_3$ 74·77, HO 25·33.

Other analyses of ferric-hydrates by many different analysts, and from a great range of localities, give an amount of water which corresponds to one or the other of these last two hydrates; but as these contain also either organic matter, phosphoric acid, or silica in the combined state, it is impossible, without further investigation, to know to what hydrate to refer them.

The artificial ferric-hydrate precipitated by ammonia from ferric-chloride varies in composition according to the method of treatment. Schaffner obtained a hydrate with one atom, Gmelin with two atoms, and Wittstein with three atoms of water; this last kept for some time under water, became crystalline, and was converted into a hydrate with one and a half atoms of water. Recent investigations by E. Davies * show that the ordinary precipitated ferric-hydrate loses water on being boiled in water; in one case the amount of water was reduced to 3·52 per cent. Similar experiments conducted in this laboratory by Mr. Rodman showed that by continued boiling in water the amount of water remaining in the hydrate could be reduced even to two per cent. These facts, as Mr. Davies suggests, explain in a very satisfactory manner the association of the different ferric-hydrates in nature, and do not necessarily demand the supposition of great heat to account for the large beds of anhydrous hæmatite found in different parts of the world.

NOTES ON LECTURE EXPERIMENTS.

Preparing Coils of Wire.—I have found the following simple apparatus extremely serviceable in preparing coils of steel wire for combustion in oxygen, and for coiling wires for battery connections. The apparatus was devised by Mr. Waite, of this town.



A B is a rod of iron of the diameter it is desired the coils shall be made. The extremity A is flattened, and a hole drilled through, while the other end is bent twice at right angles to form a winch. C is a piece of sheet iron bent so as to fit loosely round the bar, and when the lower part is fastened in a vice, serves as a support for the rod A B. To prepare a coil of wire, the lower part of the support C is clamped in a vice; a long piece of this wire is then threaded through the hole at the extremity A, and the two ends held while an assistant turns the winch. The wire when twisted is held against the bar with the left hand, while the winch is turned with the right, and the wire is thus wound on the rod. On cutting the wire at A, the coil may be removed, and is ready for use.

C. J. Woodward.

Midland Institute, Birmingham, January 6, 1868.

Succinic Acid from Ethylidenic Chloride.—M. Simpson. The action of potassic hydrate upon ethylic cyanide resulting in the formation of succinic acid led the author to expect that an isomeric acid would be obtained if instead of ethylenic, ethylidenic cyanide were taken. No isomer, however, but ordinary succinic acid is formed, and this apparent anomaly is explained by the supposition that during the process of heating ethylidenic chloride with potassic cyanide (180° C. being the temperature required) the former is partially converted into ethylenic chloride by an exchange of places between hydrogen and chlorine:—

$$\left\{\begin{matrix}\Theta H_2H \\ \Theta HCl_2\end{matrix}\right. = \left\{\begin{matrix}\Theta H_2Cl \\ \Theta H_2Cl\end{matrix}\right.$$

—(*Comptes R.* lxv. 351.)

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* Rammelsberg, Handbuch für Mineralchemie, 983.

† Journ. Chem. Society, II. iii. 214.

* Journ. Chem. Society, II. iv. 69.

LECTURES.

ON HEAT AND COLD; A COURSE OF SIX LECTURES* (ADAPTED TO A JUVENILE AUDITORY), DELIVERED AT THE ROYAL INSTITUTION OF GREAT BRITAIN (CHRISTMAS, 1867-8).

BY JOHN TYNDALL, ESQ., LL.D., F.R.S.

Lecture I.

The nature of heat, and the various modes of generating it.—Friction and combustion.—Changes of volume produced by heat.

I wished very much indeed to be able to write out notes of these lectures, in order that you might take those notes home with you, and that they might help you to remember what I spake here. But I have been so very, very busy with other matters—so very hard at work—that I have found it perfectly impossible to write out and to get printed those notes which I now refer to. In fact, I wished very much indeed to avoid giving this course of lectures altogether, in consequence of the heavy labours of another kind that I have been engaged in; but, however, some friends of mine said that the boys and girls here present would take it very unkindly of me, and would think that I was neglecting them if I did not come forward and give this course; and inasmuch as this was a thing I did not wish you to think of me, I thought it best to come forward and to do the best that I can under the circumstances. Of course your not having those notes to bring things back to your minds will render it all the more necessary on your part to give me the utmost possible attention, to endeavour to understand all I say,—and indeed at the very starting I shall have to bring some very difficult matters before you that will require a concentration of attention on your part. But I calculate—and I know I can calculate with confidence—upon your attention; and if you give me that attention, as I am sure you will, I have no doubt we shall get on, on the whole, exceedingly well together. (Applause.)

Now, I suppose all of us twenty times a day—perhaps more—make use of the word "*I*." Every boy here present says, "I eat," "I drink," "I sleep," "I feel;" but perhaps very few boys or girls either ever ask themselves, "Who is this *I* that does all these things?" and if you went to the biggest man in the world, or the greatest philosopher, you would puzzle him exceedingly if you asked him, "Who is this *I* that sleeps, and drinks, and eats, and feels?" In fact, philosophers, great as they may be—and great they are—find that there are things altogether beyond their knowledge and beyond their power to understand, and this wonderful human *I* is one of those things. Hence, I do not want you to be able to answer me if I ask, Who is this *I*—what is this *I*—that sleeps, and drinks, and eats, and feels, and makes use of its senses? In fact, as I have said, the best of us know very little about it; but we know a great deal of that peculiar instrument by which the *I* operates upon the world, and by which it understands the things that are going on in the world, and that instrument is the wonderful human body. When we examine that body, looking into its interior parts, we find bones and blood and muscles and tissues of various kinds; and passing through these muscles we find strings of whitish matter—strings going from the spinal marrow, and going from a mass of matter that rests in this wonderful cavity called the head. I say those strings of white matter go through the body, and they are called the *nerves;* and it is by the intervention of these nerves and this wonderful brain that we human beings are able, so to say, to hold converse with the world round about us. Now, these nerves transmit the impressions from without. If I prick my finger a nerve is affected: it is lacerated by the pricking of the pin or the penknife, and that nerve thus lacerated sends intelligence through itself up along the arm to the brain; and until it arrives at the brain you do not feel anything. It travels up to the brain at the rate of about 180 feet in a second. This is one of these wonderful things that have been measured by able men. You do not feel the exact moment your finger is pricked.

Now, what the nerves in all these cases convey to the brain is something in the nature of motion; and in order to enable you to form an idea of this motion I have arranged a little experiment. And here I must call upon that power which every boy and girl here possesses—that wonderful power which is sometimes called "imagination"—the power of picturing things before the mind. I would ask you to picture one of these nerves going through the body to the brain; and I would ask you to figure that nerve burned, we will say. Now, how are you to conceive of this nerve? The nerve is made up of very minute particles to which we give the name of "molecules" or "atoms." They are sometimes called atoms. In fact a molecule is an aggregate of atoms. But what I want you to clearly realize, and which is perfectly in your power to realize, is that these nerves are composed of little particles—(I do not care about the name, whether "atoms" or "molecules"); and if you disturb the end of any nerve—if you burn it—if you prick it—what you do there is that you impart *motion* to the body. This motion runs along the nerve, and when it reaches the brain it declares itself in some form—of pain, or, it may be, of pleasure. Now, how is this done? You may, in fact, consider those nerves to be like the telegraphic wires that go through the streets. You have seen them passing through the air of London; and these telegraphic wires carry messages to and fro through various parts of London. I say, you may consider the nerves as being represented by those telegraphic wires, and you may consider the brain a great central station, so to say, with which the nerves communicate—to which they communicate their messages, and from which they receive their messages. In order to make this plain I have here arranged a little experiment—very simple indeed. You can make it yourselves with the glass balls used in the game of solitaire. You see I have there a series of these balls, and I want to enable you by these balls to conceive how motion is propagated through the nerves. There is nothing shot through the nerves: the motion is communicated from particle to particle. Observe, here. If I take hold of this ball and strike it against the first ball of this series, you will observe what occurs. The motion will be transmitted through all the series of balls. Each ball will take up the motion given to it by the preceding one, and pass it on to its neighbour, and thus the motion will go through the entire series, so that the last ball of the series will be the only one affected. Observe how the last ball is detached. There it goes away. The moment I hit this first ball the terminal ball flies off. Now, in some such way—in a way somewhat analogous to this—is motion propagated to the brain. Allow this bell to represent the brain. Now, if we take our series of balls thus, and strike, as I have said, the first ball, the blow will be communicated to the terminal ball, and that, liberated, will strike against the bell. The sound of that bell is something like a signal given in the brain. [The bell was sounded in the manner indicated.] Here you have the motion transmitted from the first ball, and finally the bell is thus affected. In the way somewhat rudely and roughly represented by this experiment the motion is transmitted to the brain, and when it reaches the brain it evidences itself, as I have said, as pleasure or pain, as the case may be.

Now, having exercised your imagination upon those particles which I have called atoms or molecules, I think we may go on to consider the character of this power that we have to deal with in this course of lectures; that is, this thing that we call "heat." Long reflection and many experiments on this important subject have caused men of science—learned men who investigate such things—to the notion that this thing that we call heat is a kind of motion. And now I should like every, even my youngest hearer—(and that is a large

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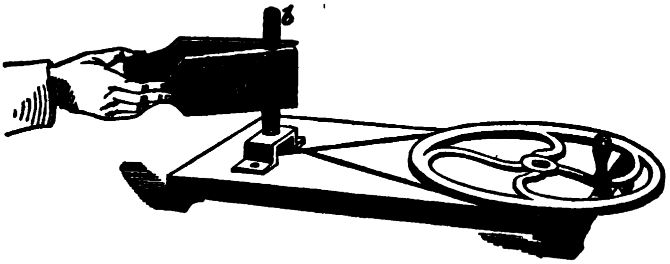
* Reported verbatim, by permission of the Author, for this Journal.

demand)—to figure, by this power of imagination, what I describe. Take any substance,—for instance, this body which I hold in my hand. This, like our nerves, is composed of little particles or atoms. It is not absolutely cold at the present time. Of course it may feel cold to my hand, but it is really not cold. Those particles that I have been speaking of are in a state of motion. Although they are too small to be seen, and although the motion is entirely too small to be seen even by our best microscopes, still we have every reason to believe—the very strongest reason to believe—that the particles of that body at the present time are vibrating. The little particles, remember—(picture them to your mind)—are vibrating to and fro; and the warmer the body is, the more intense is this motion; and, in point of fact, it is this motion of the smallest particles of the body to which, when communicated to the nerves, and through the nerves to the brain, we give the name of heat. Now although I am dealing with some of the deepest things in science, still I expect all the boys and girls here to clearly figure to their own minds this substance as an assemblage of small particles, and those particles oscillating—vibrating; and the warmth that I feel when I take this in my hand is due to the multitude of these small motions that are going on within the body.

Well, now, this motion of the particles of a body can be excited in various ways, and one of the most ordinary ways of exciting it is by friction. If you take, say a flat brass button, in your hand, and if you rub this button upon a surface of wood, as I am doing this which I hold in my hand, very soon by rubbing this body [a short rod of metal] I make it so hot that I don't like to bear it against the skin of my face. In point of fact, the friction exerted against this substance produces the motion we call heat, and I very nearly burn myself. The rubbing throws the particles into this furious motion. If I place this body, before rubbing it, upon a flat piece of white wax, there it stands; but let me rub the end of the piece of metal for a time, thus, and then place it upon the wax, you observe it runs away; it melts the wax underneath it, and slides down in this way. This body [a similar short rod of metal] which has not been rubbed, will never melt the wax, and there it rests. The sliding of the other piece of metal is due to the heat produced by the friction. And in various other ways heat is produced by friction. For instance, if you take a saw, and pass that saw through wood, if you are careless and do not put grease upon the saw, then there is so much friction that the amount of heat developed in the saw becomes very great indeed. The saw becomes quite hot. And that is the theory and that is the reason why carpenters grease their saws when they use them. They do not want to make heat, for when this friction is overcome you actually *create* heat. Now, the carpenter is not anxious to make heat; he wants to get through the wood, and he wants to get through it with the least possible trouble; and, in consequence, he lessens the heat by putting grease upon the saw; he makes it as smooth as possible.

In this way, then, that is, by means of friction, we can actually generate, produce, create, this thing we call heat—this motion; and that is a very important point. It was thought for a long time impossible that heat could be generated. It was supposed that there was a certain quantity of heat in the universe, and that this was perfectly constant, no change occurring in it; but you see we have simply to produce this motion of the particles, and then that motion we call heat is set up. I have here an experiment that will still further illustrate this. When I was a boy—and I suppose I was like the average of boys—I was very fond of savages, and people of that kind. Now, I should like immensely to be able to transform myself into a New Zealand savage for the next five minutes. If I could do so I should be able to make a very beautiful experiment which it is not now in my power to do, for I am not so clever as those savages. My friend, Sir John Lubbock, who is a very great man on savages, has given me these two sticks. These are the genuine articles, brought from Australia. This stick is made of a particular kind of wood, pithy, and rather soft; and you see there are holes in one of the pieces of wood. This second stick is made of a harder material. Now, one of these native savages takes one stick and places the end of it in one of the holes of the other stick. He then clasps it, thus, and by the friction he uses he causes a little dust, first of all at the end. He works on until that dust takes fire; and then he manages by blowing, and by operating with far more skill than I can bring to bear upon the experiment, to actually produce flame. These are the very articles used by these New Zealand savages when they wish to produce fire by friction.

FIG. 1.



Well, I can illustrate still farther this mode of producing heat. I have here, you see, a hollow tube, *b*, and I will place in this tube a quantity of a certain liquid which boils a little more readily than water. I might take water, but I will make use of ether for the purpose of making the experiment more rapidly. Now I will try whether I can not boil that liquid by friction. You see after putting the ether into the tube I cork it up thus, and then fix the tube on this instrument which is called a whirling table, and by means of which I can cause the tube of liquid to spin round with great rapidity. The tube is now fixed firmly upon the whirling table, and we will there spin it rapidly round and round. I could boil that ether by simply clasping the tube in my naked hand. I have done so over and over again. The friction of my hand against this tube has been sufficient to boil this ether, but I have found it very hot and very unpleasant; and in order to protect my hand I will take a piece of flannel, and grasp the tube tightly with the flannel round it. Now, I want you to observe that if the experiment succeeds—(and experiments are always liable to fail)—the friction of the flannel against the tube which goes round and round will cause the ether to boil, and when that happens the steam of the ether underneath the cork will project the cork into the air. I want you now to observe the cork while I clasp the tube in the flannel. [In the course of a few seconds the cork flew from the mouth of the tube.] There it is, you see. Look at that!—boiled in half a minute,—boiled by the friction of that piece of flannel against the tube. Well now, I have here another tube, and I have here a quantity of metal. Look at it,—hard metal. There it is. Now, I break that metal into bits thus; and I purposely avoided putting it into this tube until now so that you might actually see the metal going in, and see that there is no delusion or mistake about the matter. Now, I will place some of this broken metal in this tube. We can put a little more in afterwards. I have put in as much as will go in now. I expect to be able to melt that metal by friction. I will cork the tube up tightly as in the former case, and when the metal is melted I will pour it out on this plate. [The rotation was commenced.] I am beginning to feel the heat now, and I have no doubt that very soon we shall have the metal in the tube molten. [Examines the contents of the tube.] Yes. I will put in more, so as to get a greater quantity melted. I will pour it out presently, but you must first exercise your patience until we get it all melted. I put in as much as the cavity would hold in the first instance. Now, we will work the whirling table once more, and I will clasp it as before. [After a further interval]—Now the tube is so hot that I have no doubt the metal inside is melted. Yes, it is melted. Let us put in a last bit, and thus we shall get back the whole of that cake after it has been liquefied by the friction. I cork up the tube in order to keep the molten metal from splashing about. [The tube was caused to revolve again for a short time, and then detached from the whirling table. The metal was poured out, and found to be completely fused.]

Well, there are various other ways by which this motion that we call heat can be generated. It can be generated by percussion—by hitting with anything hard. For instance, I have here a piece of lead—a lead bullet: if I place this bullet upon an anvil, and strike it in this way, when I take it up afterwards it is too hot to hold, and burns me. I have actually created that heat. I have called that heat into existence. By hitting this bullet I have thrown its particles into this peculiar vibratory motion to which we give the name of heat.

Now, how do we know the precise amount of heat produced by a stroke of this kind? I had intended to make an experiment before you in connection with this point; but you will understand the experiment without my taking up your time to perform it in your presence. Here is a piece of lead, and there I have upon the floor a thick plate of iron. I intended to send one of my assistants to the top of the house, and I intended him to drop this piece of lead down, and let it fall upon this plate of iron. Now, it so happens that the height of this room is such that this piece of lead, having a certain amount of temperature on leaving the hand, would have that warmth augmented by one degree of temperature. I must here make use of the term "degree," although I cannot explain it till the second lecture; but you will remember that by the falling of this piece of lead from the ceiling, upon this plate of metal, we should raise the temperature of the lead one degree Fahrenheit. In like manner, if I sent up this liquid metal, which is called mercury, and had it poured out from the ceiling, and let it come down upon this plate, the mercury in falling from the top of the house to the bottom would have its temperature raised one degree. But if I took water it would be totally different. In this case I should have to go not to a height of 30 feet, but to a height of 770 feet and a little more, in order that the water should have its temperature raised one degree. You will understand this difference between water and mercury and between water and lead, by-and-by. I now wish you to understand that we can tell the exact amount of heat which a shot falling from a certain height can generate or produce; and we should find an increase of heat produced in all such cases, if we had instruments of sufficient delicacy. No doubt many of you will see when you grow up that fine waterfall in Switzerland where the river Aar jumps or tumbles down a perpendicular precipice. I suppose it jumps from a vertical height of 400 feet. Well, if you could place a thermometer at the top of that fall and another at the bottom, the water at the bottom, if the thermometer were delicate enough, would be found warmer than the water at the top; and knowing the height from which the cataract plunges, we can tell the exact amount of heat generated by its fall downwards, through its power of percussion in developing heat.

When I was a boy instead of using percussion caps, which are now so common for firing guns, they used to employ an instrument of this kind in guns—[exhibiting an old-fashioned gun-lock.] Here is a piece of steel, and this other substance is a piece of ordinary flint which you see moves forward in this way. Now I can cock that gun-lock, and then by pressing on the trigger I release the hold, and the flint falls against the steel, and you notice the sparks produced. This is a very old lock, and a very bad one; but still you see there are sparks produced when I liberate the flint and it strikes against this steel. If we put a little powder in the pan beneath the flint, we imitate what used to be the method of firing guns in former days. [The lock was then primed.] Now, you see when I let the flint strike the steel the gunpowder is exploded by the sparks produced. In the same way tobacco smokers and others used to get a light by igniting tinder by means of the sparks produced from a flint when struck on a piece of steel.

Now, what is the meaning of this experiment? What is the theory of that gun-lock? It is this. You have seen that when I struck the lead I raised its temperature. A very great man who used to lecture in this room many years ago —Sir Humphry Davy— caused a lock of this kind to go off where there was no air, and when he examined the lock afterwards he found that the flint had struck away little bits of the steel from the part of the lock against which it struck; and when he examined those little bits of steel he found that they had been fused; so that really the percussion of this flint against the steel surface is so strong that it raises those particles of steel which it breaks off almost to a white heat. When steel or iron is thus raised to a high temperature it is affected by a certain substance which is round about it in the air. You must remember the name of that substance. it is so very important. It is called *oxygen;* and when iron or steel is raised to a sufficient temperature, this oxygen instantly attacks it—plunges against it. As before, I must ask you to exercise your imagination with regard to this oxygen. You must figure in your minds this oxygen as very small particles diffused throughout the air. Then, I say, when the iron or steel is raised to a high temperature, the oxygen diffused through the air plunges against it, and hits it so hard that there is a kind of percussion. The oxygen hits the iron or steel so hard as to produce this thing that we call heat, and produce it in such a degree as actually to render the body white hot. Now, I want to show you that this is the case. I have here the means of producing a flame of considerable size; and downstairs we have a pair of bellows. A man has just quitted the room to work those bellows. A current of air will pass through this tube, and we shall obtain here a flame of considerable power. Now, what I want you to understand is this,—that if by means of this flame I heat particles of iron or steel, you will find that those particles of iron or steel will shoot out like stars, because of the plunging upon them of the oxygen of the air. Here I have a vessel containing these iron filings, and as I throw them on the flame you see the sparks produced are very brilliant indeed. (Applause.) The iron is burned in this way. I have thrown in sufficient of it to illustrate what I have been saying. First of all these particles of iron were heated exactly as in the case of the gun-lock; and when they were heated the oxygen of the atmosphere plunged against them so violently as to produce these star-like forms which you have seen. Some call this force attraction or chemical affinity; but what I want you to see is this—that these particles of iron when heated to this temperature are showered down upon by the oxygen of the air. This wonderful substance of the air, called oxygen, forms but a small portion of the atmosphere—about one-fifth of it by weight. Hence, if we had the whole atmosphere composed of oxygen those effects of combustion would be very much greater indeed than they are at present. I have here some pure oxygen obtained by proper methods, and I will just ask you to observe how much more powerfully this atmosphere of pure oxygen acts upon a body than does the oxygen in the ordinary air, where it is diluted, as I have said, to a considerable extent. I have here a piece of wood which I set fire to. I blow the flame out then, leaving the end red. You see the air has no power to make it ignite again. If I bring it into the oxygen see what occurs. [The incandescent end of the stick was introduced into a jar of oxygen gas, and immediately burst into a brilliant flame.] The oxygen when it is not diluted has this wonderful effect. And so I might take paper or other combustible bodies instead of this wood. In fact I might use iron. I will produce here a flame from a mixture of oxygen and another gas called hydrogen, and I will cause the oxygen to burn, not a piece of paper or wood, but actually a piece of steel. I hold a piece of steel here in my hand. It is the spring of a watch. A man has now gone down to start the apparatus. I shall very soon have a jet of gas passing through here. I will ignite that jet of gas, and then you will see the flame of the hydrogen,—not a brilliant flame by any means. [A jet of hydrogen was then ignited.] I will presently mix with the hydrogen flame which you see a quantity of this oxygen, but I want first to raise this steel to a very high temperature, and then to allow the oxygen gas to act upon it. I will now throw into this jet of hydrogen a quantity of this wonderful oxygen. You will see that the flame becomes very much smaller; and now it is enormously hot. Observe what it can do with that piece of steel. Observe how it can burn it away. This substance called oxygen is playing upon that spring. If I take away the hydrogen you see no flame whatever, but we have only the pure, cold oxygen; but when once the temperature of the steel has been raised sufficiently, the force with which the oxygen particles, or atoms as I called them, plunge down upon the steel is sufficient to produce this wonderful effect. [The watch-spring continued to burn in the jet of oxygen.]

Well, now, we have the generation of heat exemplified in this way. I showed you first of all that it could be generated by friction to such an extent that you were able to melt metal with it. I then showed you that it was generated by ordinary mechanical percussion, as in the striking of two pieces of lead by the hammer. And now I ask your power of imagination to help me here in the case of the oxygen uniting with the iron or the steel, which is, to all intents and purposes, a case of percussion. It is, however, a case of percussion of atoms, instead of the percussion of a hammer descending upon a weight. Now, I think that if you have followed me I have not uttered a word that you can not perfectly understand. You can picture before your mind these little oxygen atoms showering down with this tremendous force upon the surface of the iron; and the object I have in lecturing to you boys and girls is that you may see with the eyes of your mind those things which are too small to be seen with the eyes of your body, and that is the power I referred to in the first instance—the power of imagination.

I have here a variety of jars of this oxygen gas. I do not want to spend too much time in operating with them, but one experiment I must make because it is of such importance and such historic interest in science. The great Sir Isaac Newton, regarding whom a great deal of nonsense and a great deal of wrong has been uttered lately in the newspapers and elsewhere, operated with a diamond in the course of his experiments on optics; and he concluded from his experiments on the diamond that that beautiful gem, the hardest of all substances, was an unctuous, peculiar substance like wax or grease. Long before the experiment was ever made, this Newton by that very power which exists in every boy and girl here present, and which I called upon in the beginning of the lecture, saw that this beautiful gem was a combustible substance; and now I want to show you that Newton was true in his prediction. I have here a small diamond—(for diamonds are very precious, as you know, and it would be a wasteful expenditure, of course, to use a large one); and I will first of all heat it by means of this very hot flame that we possess here. I have here some oxygen gas, and after heating the diamond I will plunge it into the oxygen gas, and I think you will find it will there glow like a little star. Perhaps the hydrogen can not heat it strongly enough, but we will try it. [The heated diamond was lowered into a jar of oxygen.] Yes, there is the diamond burning before you. And now how are you to figure that diamond? How are you to imagine the state of things going on there? At the present time it is surrounded by oxygen; and the oxygen atoms, as I have called them, are showering down upon the diamond, and showering down upon it with such percussive force as to render it that bright and brilliant star. Now, I think every boy and girl here present, can picture before his and her mind what is going on. Imagine these atoms of oxygen showering down upon the diamond, and the force with which they do so raises the diamond to that temperature.

In all these cases heat is actually generated. There is called into existence heat which did not exist before. It is, as I have said, a kind of motion which can be generated in the way which I have indicated.

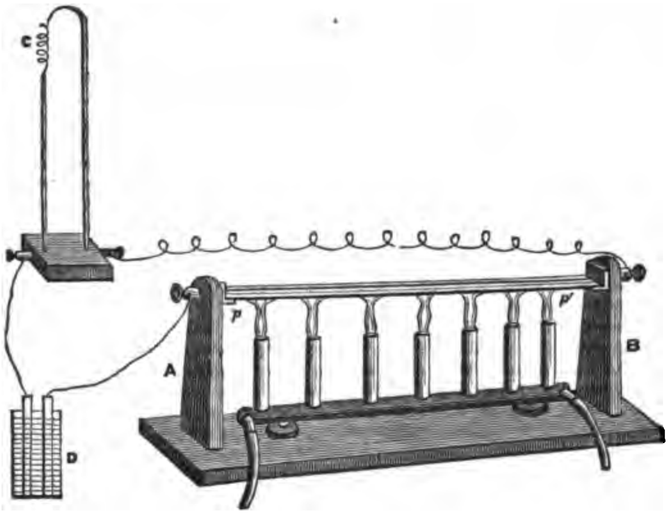
Having now obtained a general notion as to the methods in which heat is generated, we may pass on for a moment or two to investigate what it can do—how bodies are affected by it.

I have arranged an experiment here, in front of the table, which will enable you to see what heat can do; and here again I would call upon that wonderful power of imagination. Imagine the particles of a body getting gradually warmer, vibrating with greater and greater intensity. What is the natural consequence? That these particles should force themselves asunder, that the body should become bigger by being heated, that the volume of the body should be augmented by the augmentation of its temperature. Here I have a platinum wire stretched from this stand to this. You observe that at the end I have attached a straw with a piece of paper fastened on it. Here you observe a little wheel, and from that wheel you observe a weight descending. Round the axis of the wheel a platinum wire is coiled. Now the platinum wire is pulling in one direction, and the weight is pulling in the other direction, but if you relax the platinum wire the weight will instantly predominate and the index will rise up. Observe that index rises if I relax the wire by simply pressing this rod to which one end of it is fixed; and when I take my hand away the wire remains no longer relaxed, and the index falls back again. (A great portion of what we call "experimental science" consists of devices of this kind. This was devised by my assistant Mr. Cottrell.) But how shall I heat that wire? By a power which is far away from here, which I hope to be able to talk to you about at some future time. Coming up from the yard beneath there is a power which heats the wire; it is called an electric current. When the current comes the platinum wire will be heated and elongated, and the elongation of the wire will manifest itself on the index. You see this piece of paper smoking with the heat of the wire. If I stop the current, the source of heat is detached, and the wire cools. When the wire cools it contracts, and when it contracts the index falls in this peculiar way.

I have another experiment here to show how heat operates in causing bodies to expand. I have here two bars—one of iron and the other of brass; and at the present time you see here in front of the table a little piece of apparatus the meaning of which you will understand immediately. I will show you that this wire which you see here in front is a little coil of platinum wire. But before I show you this wire I should first like to show you what a power we possess for heating the platinum wire when we augment our current. This current comes from a battery downstairs, which I trust to have the pleasure of explaining to you, not this year, but perhaps in some future year. Now the assistant will give me a powerful current, and I think you will see that this wire will be raised to redness throughout its entire length. [The electric current was then passed through the wire]. The platinum wire is now red hot, and the index goes up in this prompt way. You will see the glow of the red-hot wire now the light is lowered. Now if I shorten the length of wire less and less resistance is thrown in the way of the current, and a greater amount of electricity passes through, and you have the wire raised to this much greater temperature. There is one thing to be observed here. You must not allow yourselves to suppose that this apparent thickening of the wire on being heated is due to a real thickening. The red hot wire looks as thick as a quill. This appearance, which I have no doubt is visible to you, is not due to a real thickening. It is an effect produced by a bright light on the eye. A bright body is always seen larger than it ought to be, and this particular wire now before you is seen thicker by those in more distant parts of the theatre than it is by those near at hand. This proves that it is a deception of the eye—a kind of illusion called "irradiation." It is not a real thickening

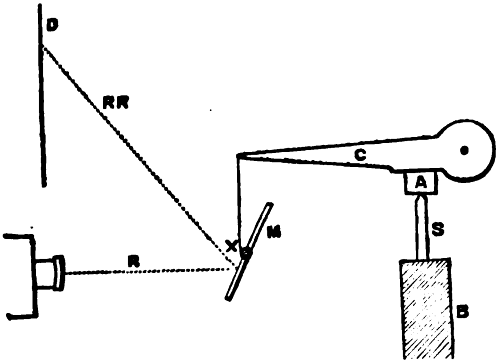
[The platinum wire was still further shortened and then parted asunder.] There, the wire is now fused by this electric current.

Fig. 2.



Now, I will call back your attention to this spiral C which you see here. Here on one of these supports A B is a piece of brass *p*, and here is another *p'*; and stretching across from support to support are two bars, one of brass and one of iron. At present they are not long enough to span the distance from one support to the other: but I will heat them, and then they will expand, and you will find that when they expand sufficiently to bridge this chasm from one support to another an electric current will pass, and then that spiral C will be like a voice telling us that the bars have expanded from one support to the other. We will now light the jets of gas underneath these bars, which at present are too short to span the distance between the supports. [After an interval]—Observe now that what I predicted a moment ago has occurred. The spiral is now ignited. If I remove this brass bar the spiral sinks. What I want to show you by this experiment is that the brass expands more than the iron. It was the expansion of the brass which bridged the chasm across.

I have told you that a great portion of experimental science is taken up by devices of this kind to render these small expansions evident. I think there is before you on the floor in front of the table a piece of apparatus more delicate than any that has ever yet been made. It is an apparatus intended to show, among other things, the expansion of volume by heat. You will understand this apparatus immediately by reference to this small sketch that I have drawn upon the black-board. I have taken simply the essential parts of the apparatus, and you will understand them, I am sure, perfectly well.



The bottom part B of the sketch represents the upper end of that upright bar of metal which you see between those two brass pillars in the apparatus in the middle of the room. On the top of this bar rests a little brass stem S; and the top of that stem is pointed and presses upon a very hard flat stone—a plate of agate A. Now, conceive the top of this bar to be lifted, and to push this stem up against the plate of agate. What will occur? You see the arm C above the piece of agate. That arm moves upon a pivot which you see marked by a dot; a very little pushing of this arm causes it to move through a greater space than the body which pushes it. Now, attached to this arm is a piece of the hair-spring of a watch, and that is carried round an axis X, attached to which axis is a piece of looking-glass—that is a mirror M. Upon that mirror a beam of light R is cast. The figure at the left of the sketch, I suppose to be the front part of a lamp from which the light will issue. The beam of light will fall upon that mirror, and will be reflected upwards RR, and will mark itself as a spot of light upon the screen D. Now, if you conceive the end of the bar to be lifted, and to push the arm upwards, it will cause the axis of the mirror to turn round, and cause the mirror to take another position; and when the mirror takes another position, this beam of reflected light will travel with the mirror, and will travel with twice the velocity of the mirror. Thus, in this experiment, instead of having a straw for an index, I use a beam of light. You will understand the apparatus when I make the experiment. I think, as I have said, it is the most delicate instrument of the kind that has ever yet been made. Now I will try and get the apparatus in proper order for showing the experiment. I throw a beam of light upon the mirror, and there you see it reflected and quickening on the wall. I will bring it down so as to get it on the screen. You see it is exceedingly sensitive. That constitutes our index. And now I will ask you to observe what I am going to do. I will not touch that heavy bar of lead; I will not heat it with a flame; I will simply breathe against it; and I believe that this apparatus is so exceedingly delicate that the mere breathing against this mass of lead (and it is very large) will cause the lead to expand upwards, and will bring down that spot of light from the top of the screen to the bottom. [The lecturer then breathed on the bar of lead, and the image of the beam of light gradually travelled down the screen.] The mere warmth of the breath is sufficient to produce this effect. Now I will pour upon the bar a little liquid that will chill it—make it cold; and I think you will find that as the bar cools it will contract, and that the beam of light will go back to the top of the screen. [The spot of light was successfully brought back to the upper part of the screen in the manner described.]

So much for these actions which this wonderful thing called heat produces. In our next lecture we shall endeavour to understand how this wonderful thing can be measured. We shall deal with the construction of thermometers and things of that kind, and I trust we shall get to know a great deal about them.

Lecture II.

Change of volume (continued)—The force of heat—How to measure heat—Boiling water.

I want you in the first place to pay attention to what Mr. Cottrell will do here in front of the table. There is a very thick bombshell, for which I am indebted to the great kindness of my friend Professor Abel of Woolwich. It is now filled with water, and the hole of the bomb is plugged. Mr. Cottrell will now place the bomb in this bucket, which contains a mixture of pounded ice and salt; and I want, if I can, to explode that bomb. Do not feel in the least alarmed about it. The explosion will not be such as to injure any one. I will ask him now to cover the bomb carefully with this freezing mixture of pounded ice and salt, and we will leave it there for half or three-quarters of an hour, first putting a blanket over it in order to keep the warm air of this room from acting upon it. And now on the top of this I will put these iron bottles and this leaden bottle, which also all contain water. Having placed them in the freezing mixture we will examine what occurs when the water within these bottles and this bombshell freezes. It will require, no doubt, half an hour or more to produce any action upon the bomb, because it contains a very considerable amount of water. We may possibly obtain an action more rapidly upon the iron bottles, though they are exceedingly thick. We made a similar experiment with a bombshell in the yard of the Institution, and there it occupied only half an hour to freeze the water and burst the bomb. The result is here in these fragments which are on the table. Look at the thickness of these pieces. I hope the bombshell now in the bucket will be pleasant and courteous and agreeable enough to burst before the lecture is ended, but in case it does not burst, these fragments must represent the effect I intended to produce. [At a subsequent stage of the lecture the success of the experiment was indicated by the bursting of the bomb. At the conclusion of the lecture the bottles were also found to have been burst by the freezing of the water.]

And now let me recur for a moment to our last lecture. I then attempted something very daring indeed. I dare say many of my elder hearers will have imagined that, in fact, I aimed too high,—that I endeavoured perhaps to make you understand too much; but I do not think that that was the case. I think it is possible for your minds to see the operations of this thing that we call heat almost—not quite, I think, but *almost*—as clearly as I see these operations myself, and for this reason I wish, as far as in me lies, to make you see what I see, when I think and talk of this thing that we call heat. It was for that reason that I endeavoured to cause you to picture to your minds first of all the motion of the particles produced by striking a piece of lead. You remember I put a piece of lead upon the anvil and struck it forcibly with the hammer, and in that way I produced heat. I then went on from that to what we call combustion; and I asked you to consider this combustion as something almost identical with the action of the hammer upon the lead,—that the combustion of bodies is due to the fact that our atmosphere contains what is called oxygen gas —the vital gas,—and that when certain bodies are raised in temperature this oxygen hits them with such force as to produce the effects that we call combustion. This, in point of fact, is the theory of combustion. If we remove the oxygen from a place where a body is burning, you will find at once that it can no longer burn. In order to make that evident to you, I have here a candle which I intend to place under what is called the "receiver" of an air-pump. Now you have the candle burning within the receiver of the air-pump. If I allowed it to continue burning, the oxygen enclosed in that receiver would by and by be exhausted by the burning of the candle, and the flame of the candle would die out as soon as the exhaustion of the oxygen took place. I will hasten that exhaustion by working the pump, and rendering the atmosphere around the candle rare; and you will find that presently the flame will become rather feeble. [The air-pump was then set in action.] You see the flame is already beginning to become dim. Now it is very dim. As I work on it becomes still dimmer, but if I let a little oxygen into that receiver I at once restore the brightness of the flame. [Some oxygen was caused to enter the receiver.] Now the flame is brighter than it was before. If I exhaust again you will find that as we have taken the oxygen away we remove the atoms that are now, as it were, showering down against the combustible matter of that candle. If we take those atoms away you see the flame becomes more and more feeble; and finally if I proceed farther I should be able, of course, to entirely extinguish the flame, for when these little oxygen atoms are no longer able to rain down upon that flame, then the flame inevitably goes out. I will readmit the air before the flame is quite extinguished. [At this moment the candle ceased to burn.] Ah! I am too late, and the flame has gone out. Now, you saw that just before that flame went out it was exceedingly feeble. It was exactly similar to the flame that you obtain at very high elevations upon the earth's surface. Many years ago Dr. Frankland and myself spent a whole night upon the top of Mont Blanc. We slept upon the top, and we there burned a number of composite candles such as we have here, and we also burned a number of them at Chamounix. The air upon the top of the mountain was very rare and very thin, and it was most wonderful to see the effect of this rarefied air upon the flames of the candles. They were exactly like the flame you saw here immediately before it went out. Strange to say, however, the quantity of stearine (the stuff of which these candles were made) consumed above in one hour was exactly equal to that consumed below. There was no sensible difference, in fact, between them, notwithstanding the enormous difference in the characters of the flames. So much for these flames.

We must now say one or two words with regard to the structure of this wonderful and beautiful thing—flame. If you look at the flame of a candle you will observe a particular portion of it to be much more luminous than the rest. At that particular part the flame gives out its greatest light; and if you light two candles, such as I have here, and look at the flame of one of these candles through the flame of the other, you will find that you can, with the greatest ease, see one through the other for a considerable distance upwards; but then you come to a very bright portion of the candle flame, and that bright portion almost wholly cuts off the vision of the other candle. Thus, through the part of the most intense brightness the light of the other candle cannot pass. There is something going on which intercepts the light of the other candle. Now, what is this something? This will lead us to a knowledge of the structure of this beautiful flame. The flame here is produced in this way. We have a wick in the centre of this column of greasy combustible matter. We ignite the wick. The heat first of all liquefies the greasy matter, and not only liquefies it, but reduces it to a state of vapour, or gas. The candle actually makes its own gas. This vapour comes from the candle straight upwards; and being heated and surrounded by the oxygen of the air, this heated vapour is immediately attacked by the oxygen; the atoms of oxygen plunge against the vapour, and what we see as light and heat is the result of this collision. But, let me say a word or two more with regard to flame. I have spoken of the vapour of the greasy matter of the candle. That vapour is composed mainly of two distinct substances. It is called a "hydrocarbon." We have there hydrogen, which is a gas, and we have carbon, which is also, under certain circumstances, a gas. These bodies are united together in the grease of this candle. Now follow me, please; and you will understand the structure of this candle-flame immediately. The vapour is attacked by oxygen; but the oxygen loves the hydrogen better than the carbon. It takes the hydrogen first, and liberates little solid particles of carbon in the flame. These carbon particles are the soot which you see sometimes in a smoky flame. You see the smoke here, in point of fact. If the combustion were perfect all that smoke would be burned, and it would be raised to a white heat in the flame. In that particular portion of the flame which gives out the maximum amount of light you have a crowd of these solid carbon particles raised to a white heat by the intense temperature of the flame. And then, finally, these carbon particles also become burned, and the products of combustion pass away into the air as gas. This is the structure of all flames—first of all, an inner core of unburned gas or vapour; and then round about that the oxygen of the air plunging, as it were, against the heated vapour, and forming a kind of luminous shell round about the interior ball.

If, when the carbon particles were heated and liberated from the hydrogen in the manner I have described, oxygen were at once to seize upon them, you could not have this intense luminosity that you find in the candle-flame. Here is a lamp, constructed by a particular friend of mine—Professor Bunsen, of Heidelberg—and you see it burns with a very small amount of light. The reason of that is that, by means of these apertures which he has made round about the central tube, he mixes the oxygen of the air with the gas before the gas is ignited, and the presence of this oxygen entirely destroys the existence of these carbon particles, to which the light of the flame is mainly due. If I cut off the air the gas alone will come out, and you see then at once that the light greatly increases. In the former case you have the carbon particles halting for a moment in the flame, and raised to a white heat before the oxygen seizes them; and thus you have a far greater amount of light than when you allow the oxygen to get in amongst them and seize them the moment they are liberated.

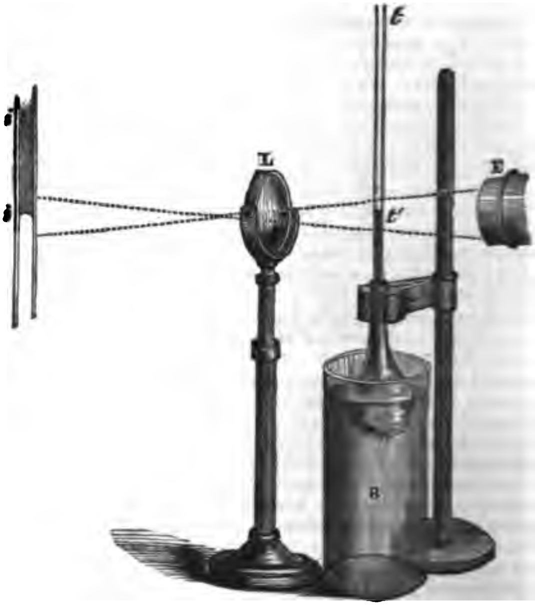
The combustion which I have just shown you is of a very vivid kind. There are also slow kinds of combustion going on. For instance, when the oxygen of the air attacks iron, it produces that red iron rust with which you are all very well acquainted. This is just as much a case of combustion as the combustion exhibited in the candle-flame. It is a case of slow combustion. When the earlier of the Atlantic cables was made it was surrounded by iron sheathing to protect it; and it was found in one case that the temperature of a great coil of this cable became very high indeed—so high as to imperil the gutta-percha and other substances that were employed to insulate the wire. This was found to be due entirely to the slow combustion—to the rusting, or "oxidation," as it is called, because oxygen is concerned in it—of the iron. The iron was slowly burned, and the heat could not get away because the coil was so large, and the consequence was that its temperature became dangerously high. Mr. Siemens has invented an exceedingly beautiful instrument for the purpose of testing cables for this heat. And so in the case of our own bodies there is going on as true a combustion as in the case of the burning candle. We take in food, it is conveyed into the blood, we breathe the oxygen of the air, that oxygen comes into contact with the food in the blood, and the food is there slowly burned, and consequently we are rendered warm. The heat of our bodies is derived entirely from this slow combustion.

Towards the close of the last lecture I passed on to a consideration of what heat does. The usual result, as I told you, is that bodies are made to expand with heat. I made several experiments in proof of this,—one with a very beautiful piece of apparatus made for me by Mr. Becker, by which we multiplied the action more than a thousandfold in order to enable you to see the expansion which occurred when I breathed against a pillar of lead. I now want to make clear to you the wonderful strength of this force with which bodies expand, and the wonderful strength of the force with which they contract. The forces which pull the atoms or molecules of a body together on its cooling are perfectly enormous. I will illustrate this by an experiment which you will understand by reference to this model. I place in a hole at the end of this iron bar a little piece of wood; you see the two ends of this piece of wood rest against these two edges; and if I pull the bar I break the piece of wood. You will observe that it first of all bends and then breaks. Now, what I am going to do is this: for this piece of wood I am going to substitute a piece of steel, and then I shall put a red-hot bar of iron across, and screw it on between these two points. It will cool, and the contraction will, I think, be so great as to break the bar of steel in the way in which I have broken this bar of wood. You see the construction of this iron apparatus is much the same as that of the model. [A red-hot bar of iron was screwed to the apparatus as described by the lecturer.] I will hasten the cooling by pouring a little water on the iron bar. [In the course of a few seconds the steel bar snapped.] There it is. The bar of steel is, in point of fact, smashed by the force with which the particles of the iron bar pull each other together when the motion of heat is taken away from them by cooling. That force is, as I have said, perfectly enormous.

Before we pass on to consider the expansion by heat of other bodies besides solid bodies, I should like to explain for the sake of the elder boys (not for the sake of the younger ones, because they will, perhaps, find it a little too difficult for them) the use of one term that is in common use in books that are written on the subject of heat. Suppose you have a piece of lead 3,510 inches in length, and suppose you augment the temperature of that lead one degree, you would find that its length would extend from 3,510 inches to 3,511 inches. That is, it would extend $\frac{1}{3511}$ths of its length. This is the fraction of its own length which the lead expands on having its temperature augmented one degree. Now, that fraction is what is called the *co-efficient of expansion* of the lead. This co-efficient of expansion is much less in many bodies than it is in the case of lead. For iron this co-efficient of expansion is not half what it is for lead. This difference renders it needful for engineers to be very careful not to unite different metals which have different co-efficients of expansion in such a way that on their expansion they would produce distortion and disruption, and, perhaps, fracture. Here, for instance, is a ruler which has one side of brass and one of iron; and when it is heated, in consequence of the brass expanding more than the iron, the ruler becomes curved or buckled up. Now, in an architectural structure different metals might be associated in such a way that on a change of temperature the edifice would be endangered in consequence of the metals expanding or contracting in different proportions. That fact is a very important one for architects to remember.

We will now proceed to a consideration of the expansion of liquids by heat. Here is a bottle containing water, another containing alcohol, and a third containing the liquid metal mercury. Here also is a bulb containing mercury. If I lay hold of this bulb the mercury within it expands, and this little column above the bulb is forced upwards. Now I want

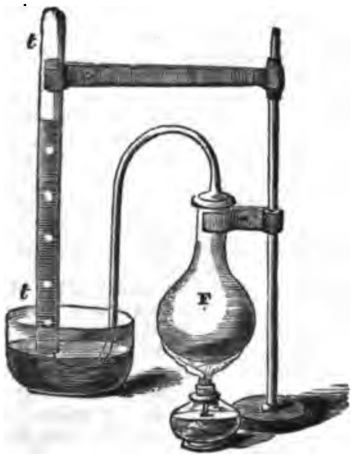
FIG. 3.



to show you, if I can, the motion of the mercury when the bulb is heated, and for that purpose I will throw an image of the column upon the screen. Now you have on the screen an inverted image *t'* of the mercury column *i i*, turned upside down by the lens L which you see in front of the lamp E, and I think you will see that when I heat the bulb, the column *i i* will go towards the lower part of the screen, owing to the expansion of the metal. It really goes upwards, but it appears to go downwards, owing to the image being inverted. I will now place the bulb in hot water,—observe the motion which I indicated. I will now take a bulb containing the liquid alcohol, which is much more expansible than mercury. Mr. Cottrell has coloured it blue, that you may see it better than you would if it were not coloured. The colour indicates the column of liquid. At the first moment of the bulb being heated the column of liquid will appear as if it contracted instead of expanded. This apparent contraction is due to the fact that when we first plunge the glass vessel containing the alcohol into warm water that vessel itself expands, and becomes, in fact, of larger capacity, and thus the column of liquid sinks in it. This sinking, however, will immediately disappear, and then the blue liquid will go up in the tube far more rapidly than the mercury rose. I might take other liquids and show you the same effects, but we must now pass on to the question of the expansion of gases.

You will understand in a moment that gases are capable of expansion by heat. For instance, I have here (Fig. 4) an empty bottle F, to which is attached a tube; and Mr. Cottrell is now placing the end of that tube underneath this column of liquid *t t*. The column of liquid is supported by what the elder boys know as the pressure of the atmosphere upon the liquid outside. Now, if I heat this bottle I cause the air in it to expand: it will ascend with force into the tube *t t*, the water will descend, and in that way I think I shall be able to transfer the air from the bottle into the tube now containing the column of water. Observe now the bubbles of air going up, and pressing down the liquid column. This pressure is due to the expansion by heat of the air in the flask. I might continue this process until nearly the whole of the air of the flask was transferred to the other vessel.

Fig 4.



In reference to this subject I might refer to this instrument, which is a thermometer made for the purpose of measuring heat by means of the expansion of air. Here at the top is a bulb filled with air. The liquid column now stands at a certain point. If I put my hand upon it the column descends. The warmth of my hand is causing the air to expand, and in doing that it drives down the liquid column.

Before proceeding farther, I must say one or two words with regard to a term I have just employed. I have used the term "thermometer." That is, *a heat measurer*. I have made use of this bulb of mercury, and the tube attached to it, purely for the purpose of enabling you to understand the common thermometer. If you take this bulb of mercury and plunge it into melting ice, or into water just frozen, at any part of the earth's surface, you will always find that the column of mercury stands at precisely the same height, so that this temperature of frozen water or melting ice is the same thing all the world over. Here, then, we have, so to say, a standard of temperature. First, suppose that our bulb of mercury is plunged into melting ice: that will give the freezing point of water. Then plunge it into boiling water under the same barometric pressure, and the height to which the column will rise under such conditions will be the same all the world over; and that point will indicate the boiling point of water.

We have three different kinds of thermometers. First of all there is the thermometer of Fahrenheit. In constructing his thermometer Fahrenheit made use of a mixture of ice and salt, and he found that this mixture gave him a far greater cold than that of ice itself. He thought this was the greatest cold possible, and he therefore marked that temperature as the zero of his scale, and began to number his degree from this zero which represented the temperature of pounded ice and salt. He then went upwards to the freezing point of water, which was 32 degrees above his zero. He then obtained the boiling point of water, and divided the distance between the freezing point and the boiling point of water into 180 equal parts or degrees. The 180 added to the 32 makes Fahrenheit's boiling point 212 degrees above his zero. The second thermometer is one which is in general use amongst scientific men, and I wish it was employed in all parts of the community. It is known as the Centigrade thermometer. This was invented by Celsius, and is sometimes called Celsius's thermometer. Here we have the distance between the freezing point of water and the boiling point divided into 100 equal parts or degrees. We have a third sort of thermometer which is known as Réaumur's. It is a very awkward one, but it is nevertheless used a great deal in Russia. In this instrument the distance between the boiling and freezing points is divided into only 80 different parts. The degrees in these three different thermometers—Réaumur's, the Centigrade, and Fahrenheit's—are in the respective proportions of 4, 5, and 9. So much then for the terms "degree" and "thermometer" which have been used in these lectures.

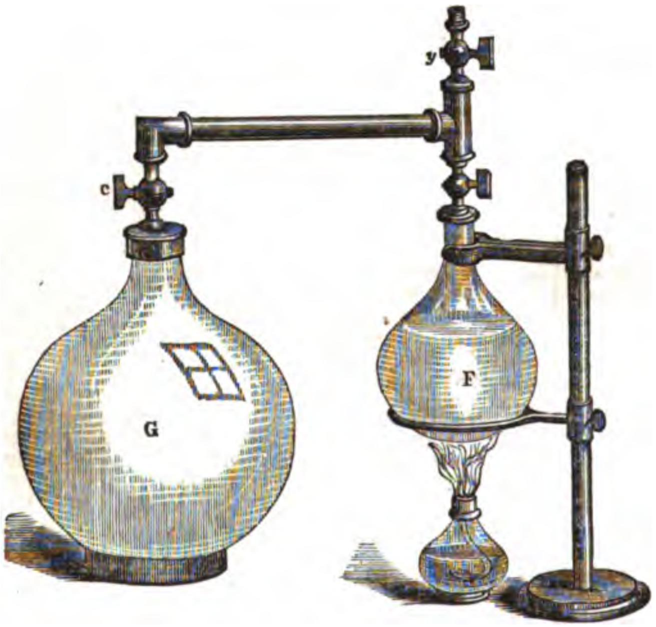
Now, if possible, I should like to show you heated air. You cannot detect it by looking at it directly in the atmosphere, but it can be made evident by a device which I intend now to employ. I can show you this heated air rising up in streams from a heated body. Here is a hot spatula. If you look directly at this hot body you can see no emanation whatever from it; but now my assistant will throw a beam of electric light upon this spatula, and we will observe the shadow of it upon the white screen. You now see above the image of the spatula a stream of heated air rising from the hot surface. This effect is quite invisible when you look at the spatula in the ordinary way.

I want now to show you another stream of air. I have here the means of giving you a still greater stream of heated air; and I want to make you acquainted with the celebrated invention of that eminent man, Mongolfier. He conceived the idea of catching these streams of heated air in a bag, and in this way the bag was carried up. From the chimney of this stove we get a stream of heated air. You observe by the effect on the screen how powerfully that stream is rising. I have here a paper balloon, and in this balloon I will catch the column of heated air. If I am successful we shall by-and-by get the balloon filled with the hot air, and then we shall make Mongolfier's celebrated experiment. You see the sides are swelling by this heated air being accumulated inside the balloon. I will now let it go out of our hands, and I venture to say it will sail upwards. There it goes. It has not gone so high as it ought to have gone, but still it will answer for philosophers as an illustration of the balloon of Mongolfier.

It is found that in the case of solids and liquids the expansion is exceedingly irregular. The co-efficient of expansion varies very much. But strange to say—(and I wish I could go into the reason and tell you why)—in the case of really perfect gases it is essentially the same for all. If you take 490 cubic inches of air and heat it one degree it becomes 491 cubic inches, so that the fraction $\frac{1}{490}$th is the co-efficient of expansion of air; and this co-efficient, as I have said, is almost exactly the same for all gaseous bodies whatever.

Now I have to direct your attention to some experiments with regard to the action of heat upon liquids; and with this view I have provided an apparatus (Fig. 5) which I will now ask Mr. Cottrell, the assistant, to place upon the end of the table. I will now cause the water in this flask F to boil, and I want to show you now what is meant by the vapour of water. We will apply heat to the flask, in which is a quantity of water, and after a little time the water will boil and bubble up. I want you to understand accurately the meaning of this bubbling up. What is going on at the present time in that flask of water is this. The water is heated. As the heat becomes more and more intense this shivering, quivering, vibratory motion becomes more and more intense, and then particles of water are jerked away from the upper surface, and carried away into the space here above. After a time the water begins to bubble. Here you have the bubbles of steam rising to the top. Now, the surface of the liquid is in communication with the air. Every square inch of the surface of that flask of water bears a pressure of about 15 lbs., and every little bubble there bears a pressure of several pounds. Why is it that the bubbles are not crushed? Simply because the pressure of the vapour within them is exactly equal to the pressure of the atmosphere without, so that the film of liquid is squeezed between the air on the upper side and the vapour on the lower side. If you lessen the pressure of the vapour within, you will have the bubble crushed by the pressure of the atmosphere. The boiling point of a liquid is precisely that temperature at which the pressure of the vapour of a liquid equals the pressure of the atmosphere. Now, by turning this tap *y*, I have enclosed in the flask some heated water; and you see that at the present time it is quite quiescent. The vapour in the flask is pressing upon the surface of the liquid. But if I take that pressure away, I have no doubt that the water will again boil. How can I do this? I have in connection with this flask of water another globular glass vessel G from which the air has been drawn by means of an air-pump. Hence the inside of this globe is a vacuum. Now, if I turn the cock *c*, which is between the flask and the other vessel, I open a way for the vapour in the flask to go from the surface of the liquid into the vacuum. Observe what occurs. The liquid is relieved of the pressure which was upon it, the water begins to boil, and the flask immediately becomes filled with the vapour of the water. The sides are now quite clouded. We can actually boil that water by cooling it. If the water in the flask were near its boiling-point, and we plunge cold water upon the upper part of the flask, we should condense the vapour above the liquid, and by thus relieving the water of the pressure on it we should cause it to boil. Here I have a tin vessel containing steam, and the air from which has been chased away by the steam. Mr. Cottrell will place it in front of the table. I will withdraw it from the flame, and I will in fact cause the water in it to boil by placing a piece of ice on the top of the vessel. [This was done.] The water is now boiling away, as the boys near at hand can see. Why? Because the vapour above the water has been condensed, and when the pressure is then removed from the surface of the liquid, ebullition takes place. If more ice is placed on the top the water will boil still more, but the atmospheric pressure will, perhaps, crush the vessel entirely in. This effect will be due to the reduction of the pressure of the vapour on the inner sides of the tin vessel. [The effect anticipated was not produced, but the experiment was repeated at the beginning of the next lecture, and the sides of the tin can were then successfully crushed. The lecturer informed the audience that it had been found that the failure in the present instance was due to an accidental air hole in the side of the tin vessel.]

Fig. 5.



I have now to pass on to a consideration of the vapour of water. I have here the two gases or substances of which water is composed. I will show you first of all that one of these is a certain gas which is inflammable, and this gas we call hydrogen. Mr. Cottrell is now getting me some hydrogen which has been actually produced by the decomposition [to use a learned term] of water. He will now give me this gas. We hold downwards the mouth of the vessel containing it, as it is excessively light, and would escape if the vessel were held upwards. I will ignite this hydrogen, and you see what occurs. It is an inflammable gas. There it is burning with a flame at the top of the tube. Now the assistant will give me some of the other gas which is a constituent of water, and here we shall find our familiar friend oxygen—that gas which causes bodies to burn so brightly when they are placed in it. I will introduce into the oxygen a small bit of wood with an ember at the end; and what is the consequence? [The glowing wood immediately burst into a bright flame.] This gas is the other of the substances of which water is composed.

Now I will take the two gases mixed together, instead of having them in separate tubes. I have here a wonderful instrument [a galvanic battery] which enables me to tear asunder the particles of water. Mr. Cottrell will now connect the vessel of water with the battery, and we will let the decomposing gases escape into soap-suds. [The mixed gases from the decomposed water were caused to form bubbles with the soap lather. The lecturer then placed a cluster of the bubbles on the palm of his open hand, and exploded them by the application of a light.] How must you figure this act of the combination of hydrogen and oxygen? I suppose you must figure it in this way. You must figure them rushing together with a great clash, and then quivering and recoiling in virtue of their resilience—their elasticity. As far as I can follow the thing in my mind the flash is due to the collision between the particles of the oxygen and hydrogen. It is due mainly to the enormous heat produced by the collision; and the heat produced by this collision is so great that for a time the molecules of water produced are so hot that they are preserved in a state of invisible gas. Water is composed of oxygen and hydrogen in the proportion of two atoms of hydrogen to one of oxygen; and two atoms of hydrogen and one of oxygen constitute what is called a "molecule of water." *Molecule* is the term employed to express that combination, and you must remember the term.

I want to show you the difference between vapour and invisible gas. This room is filled with invisible vapour; but here, early in the lecture, I placed this vessel containing something very cold—a freezing mixture; and this frost which you see upon the outside of the vessel is due to the condensation of the aqueous vapour which has come from the gas-lights and from the lungs of the persons here present. That vapour has been condensed on the cold surface of the vessel containing the freezing mixture, and then frozen into hoar frost. The fog through which you were kind enough to come on Thursday last to this place was not a true vapour. It consisted of particles of water. Here you see the same thing. The steam which you see rushing from this vessel is not a true vapour. It is due to the vapour cooling and being precipitated. If I allow the steam to pass through this flame it is converted into a true vapour. The steam is now water—now vapour. [Passing the steam-jet through the flame, and thus rendering the steam invisible.]

After a time we shall have that vapour cooling and falling into the state of water, and then if we cooled that water still more the particles would bring other forces and powers into play; and those are the forces and powers that I now want to illustrate before you. I want to exhibit to you the marvellous force of crystallisation. When we cool water sufficiently it becomes, as every boy knows, reduced to ice. That ice is one of the most wonderful things on the face of the earth, and in another lecture I shall dissect a piece of ice, and show you how wonderful it is. I want to show you something similar to what occurs on your chamber windows when they become frosted during the cold nights and covered with forms as beautiful as vegetable forms. I show you that in this way. If I took this piece of glass and poured a solution of common table salt upon it, and allowed it to remain, the water only would evaporate. The salt would be left behind incrusted on the surface of the glass. You can make the experiment at home with the greatest ease if you drop a little solution of sugar upon glass and allow it to stand. You get the water evaporated and the sugar remains behind. Now I want to do the same with a solution of another substance. First of all I must clean the glass plate perfectly, and this I do with potash; and then I shall put on it a film of a solution of something—not sugar, nor salt, but something which will give me crystals more beautiful than either of them. We will take a liquid containing a certain kind of salt in solution, and I will pour this liquid upon the glass plate. I want to evaporate this film of liquid before you, and show you the crystallisation of the substance. [An image of the moistened glass plate was projected on the screen. Crystals began to appear in the course of a few seconds, and gradually spread over the surface of the plate.] See how splendidly these crystals form. See them building themselves together in this wonderful way as if they were forming vegetable growths before your eyes. This salt is ferrocyanide of potassium. We will take another plate, and cover it in the same way with a solution of chloride of ammonium. I will warm the plate in order to hasten matters. [This plate also was represented on the screen, and a similar result was obtained as in the last case.] How beautifully these crystals run together. There they are, darting out like spears. This is an experiment which one makes hundreds of times, but still it is sufficient to strike one with wonder. How beautifully the crystals assume their determinate forms.

One minute more. I want to tell you that in passing from the liquid to the solid state—in falling together so as to form those beautiful crystals—certain bodies, comparatively few in number, become larger. Water is one of these bodies, and that is the reason why ice floats upon the water. When water freezes it expands with powerful force. The bombshell which I placed in the bucket before you was, as you see, burst by the expansion of the water in the act of freezing.

LECTURE III.

Winds and Breezes—Ice, snow, and glaciers.

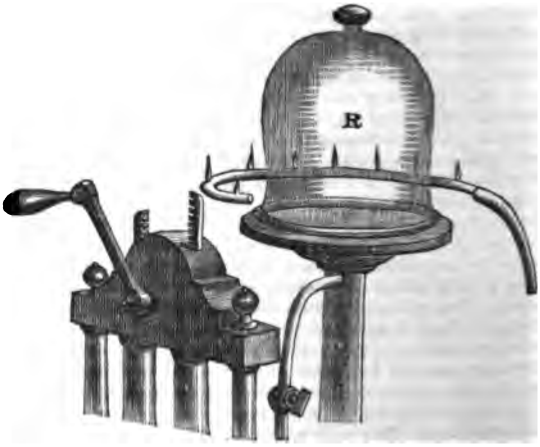
In the last lecture I showed you the change which takes place in water when it is gradually cooled; and I showed you in a very striking manner that water when it freezes and becomes ice, expands, and that the force of the expansion is so great as to burst the bombshell which was placed before you in the last lecture. Now follow me for a moment, please. Conceive water at the ordinary temperature; conceive it growing gradually colder and colder. Like almost all other bodies it becomes smaller and smaller; it shrinks as it becomes colder; but at a certain point, and some time before it turns into ice, it leaves off contracting. Suppose the water to go down from a temperature of 60°: it continues contracting until it reaches the temperature of 39° Fahr., or 4° Centigrade; and then the water instantly ceases to contract, and 7° F. before it becomes solid it begins to expand as it becomes colder. What is the consequence of this expansion? The water from 39° Fahr. downwards, becomes lighter, and it swims like oil over the surface of the water underneath, and there it is frozen; and when it freezes—when it passes from the liquid state to the solid state—a sudden and very great expansion occurs, so that eight volumes of water weigh about as much as nine volumes of ice, the ice being the lighter of the two, and therefore swimming upon the water.

I must ask you now to accompany me for a moment to some of the things that occur in nature in connection with this subject of heat. You know that at certain parts of the earth's surface the heat is very much more powerful than it is here in England; and you know that the reason of this is that at certain parts of the earth's surface the sun is overhead, and its rays come vertically downwards, and thus heat very much the surface of the earth directly underneath the sun. In the region of what is called the Equator we know that the sun is directly above the heads of the people living there at a certain distance on each side of it. Now, imagine this sun pouring down its heat through the atmosphere upon the sea. The surface of the sea is thereby warmed, a quantity of vapour is produced, and that vapour ascends with the air into the higher regions. When the surface of the earth at the equator is heated, the air also at that point becomes heated, and rises, as the air of this room rose from the surface of that heated spatula, in the last lecture. When the air at the Equator is heated by the sun, part of it goes towards the North Pole and part towards the South Pole, while underneath the air rushes in from the other direction to supply the place of the air which goes to the north and south. If you could see the air you would see it going one way and coming back another. A continuous circulation is thus going on, and the winds that are produced in this way have a particular name given them. They are called the "trade winds." The current above is called the "upper trade wind," and the current beneath is called the "lower trade wind." Now, as I have said, when the sun's rays act upon the ocean they convert its water into vapour, and this vapour is carried up into the air. What is the consequence? I want to show you one or two facts that will enable you to understand what must occur.

The first fact that I wish to show you is that if we compress air suddenly we develope heat; and I do this by means of the syringe that I have here. This is a small (Fig. 6.) glass tube bored very carefully, and furnished with a piston that fits air-tight into that glass tube: so that if I squeeze this piston down I compress the air underneath it. Now, here I have a piece of German tinder which I place in a little cavity made at the bottom of the piston; and I think I shall be able to ignite that German tinder by forcing down the piston and thus compressing the air. [The tinder was ignited as described.] Now, what we have done here is, indeed, nothing more than simply throwing the atoms (as we have agreed to call them) of the air into this intense state of vibration to which we give the name of heat. On the other hand, if we take a body having a certain amount of heat, and, instead of compressing the air, allow it to expand, then the expansion of the air produces cold. I will show you one effect of this expansion of air. I have here condensed in this vessel—forced in by a kind of syringe—a great deal more air than the vessel would contain naturally; and if I were simply to turn this cock and allow the air to issue from the vessel against an air thermometer, I should produce an effect which would, perhaps, be visible to my young friends immediately before me. If cold is produced in this way the column will rise a little. I will now turn this air out against the thermometer. The column has risen a little, which proves that the air which has come out of this vessel, and become expanded, has become chilled. A great man who used to lecture in this room many years ago—Sir Humphry Davy—described a machine which he saw at Schemnitz in Hungary, formed so as to allow a very strong current of compressed air to issue from it, and the amount of cold produced by the expansion of the air was such as to cause the vapour of the atmosphere to condense and congeal and form icicles. Now, I want you to remember that when air is condensed in the way I have described heat is developed, and that when an expansion of the air takes place an opposite effect is produced. Mr. Cottrell has here arranged a little experiment, but as I do not know whether it will be visible or not to you all, I will tell you what it is. This glass receiver contains air, and within is a small elastic balloon which also contains air. The air which the balloon has within it has a certain amount of heat, and in virtue of that heat it has a certain power of squeezing out the sides of the balloon. If we now pump the air out of the outer vessel, and so remove the air from the outside of the balloon, we take away the force which counteracts the force inside this balloon. It will then expand and almost fill the entire vessel. [The air was then exhausted by means of an air pump.] You see the balloon becomes larger and larger. You see it growing visibly before you, and the air within this balloon at the present time is being chilled because of its expansion. The assistant will go on pumping out the air from the glass receiver, and after a time the balloon will almost fill the receiver. It thus goes on swelling and swelling, the air within it expanding, and this air, by the act of expansion, becomes chilled. We will now allow the air to enter by turning this cock, and then the balloon will shrink to its first dimensions. See how small it becomes, because we get a pressure on the outside of the balloon squeezing it inwards, until now it is finally reduced to the same size that it had at the commencement. Mr. Cottrell will now remove that balloon altogether, as I want to show you what takes place within that receiver when the air is thus taken out of it. I want to show you the effect of the chilling produced by the rarefaction or expansion of the air in nature. But first I will tell you the effect produced on a body of air rising, we will say, from the surface of the sea to a certain height above it. We will take a definite height, such as we often find in the Alps—11,000 feet, the height of one of the higher Alpine passes. Conceive, then, a body of air rushing up the mountain, and going to the top of that pass. In climbing up this 11,000 feet the air gets into a place where it is not so much pressed upon as it was below. A portion of the atmosphere has been removed from above it, and the consequence is that the rising air expands, and the expansion is followed by a lowering of its temperature. The air becomes colder, and if it had in it as much moisture as it could hold, it would, in rising 11,000 feet, fall very nearly 40° Fahrenheit in temperature.

Now you must remember that in order to preserve the vapour of this room in an invisible state, a certain temperature is necessary. If you could at this moment introduce into this room the temperature of the polar regions, what would you obtain? First, the air of the room would thicken so as to form a fog, and then that fog would be chilled and fall as snow. This has occurred over and over again in Russia and elsewhere. So if you could only get the temperature of this room low enough you would see the now invisible aqueous vapour falling as snow. Even in London ball-rooms this may sometimes be observed. When the windows have been opened in the intervals of the dances the air has immediately become cooled, and a condensation of the vapour has taken place sufficient to make the atmosphere dim. Now imagine air charged with invisible vapour being carried up one of these high Alpine passes. If in this way it gets its temperature reduced to 32°, the air can no longer hold its vapour, that vapour then falls as snow, and that snow is deposited on the tops of the mountains.

Fig. 7.

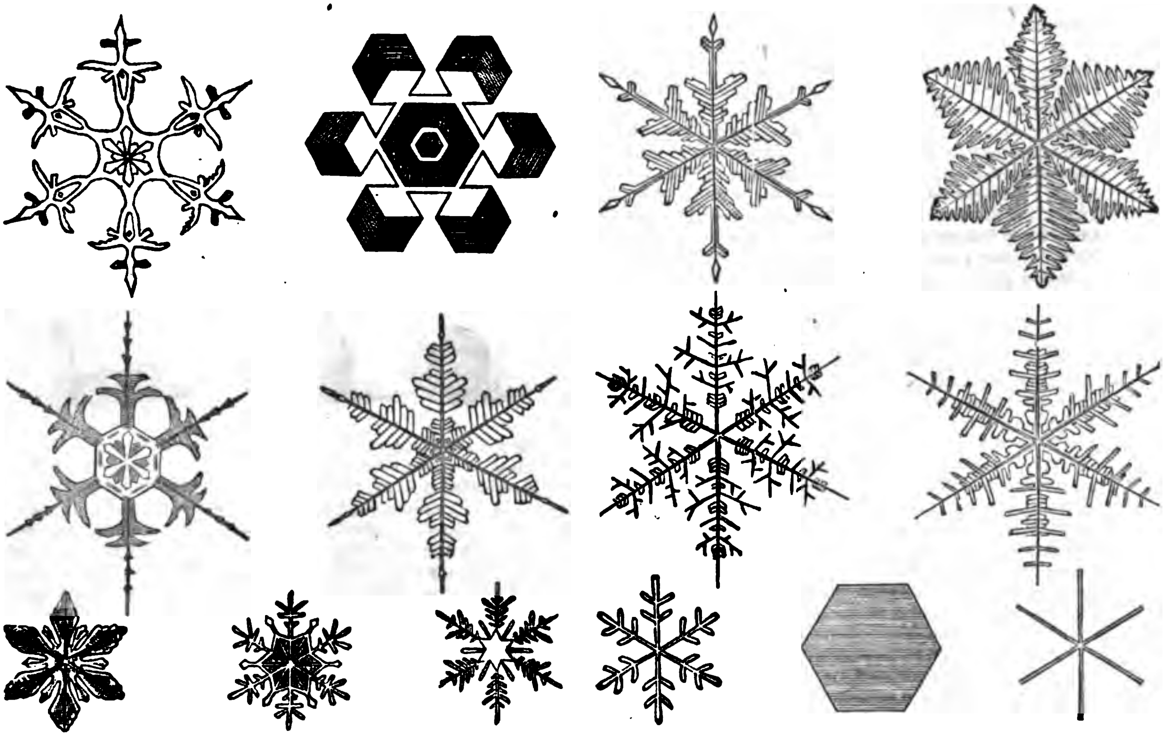


I want now to show you how clouds are formed by the condensation of vapour. Here we have the receiver of our air-pump, enclosing a quanity of air which is charged with invisible aqueous vapour. Mr. Chapman will now place a lamp behind this glass receiver. I will send a beam of light through the receiver, and let it fall on the screen. At first you will not see any appearance of anything inside the receiver. I will then ask Mr. Cottrell to work the air-pump, and exhaust some of the air, and thus cause the remaining air to expand. This will reduce its temperature, and then you will see that the vapour within the receiver will become a fog. You now see no sign of anything within the receiver; but we will now exhaust the air. [The air-pump was then put in action, and a condensation of the vapour became immediately manifest.]

You see a cloud has now formed in the receiver, and when the air is allowed to re-enter, it causes the cloud to go entirely away, although the vapour itself is still there. We will work the pump again, and you will see that the cloud is again formed, and will be again illuminated by the light from the lamp. There it is. That is a true cloud which is formed in this way from the air of the room, and it is in this way that clouds are formed in the atmosphere by the expansion and consequent cooling of the air which rises from the surface of the sea.

These clouds may fall as rain, but as I have said, they may also fall as snow. I suppose that snow is such a familiar thing to every boy and girl here present, that it may seem to be hardly worth thinking about; but still this substance is one of the most wonderful and beautiful things in the whole world; and when snow is formed in a very still atmosphere, as I have often had the pleasure of seeing it formed in the Alps, it takes the form of those beautiful figures which are represented in the diagram yonder. (Fig. 8.)

Fig. 8.

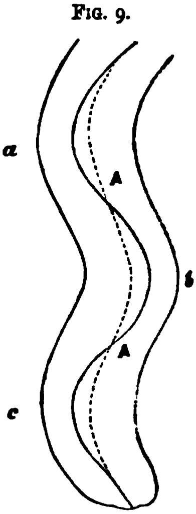


It forms as small six-rayed stars. This is the form of the snow which goes on loading the Alpine mountains year after year; and when we look at these mountains and at the valleys connected with them, we find that the most wonderful series of appearances presents itself. On very closely observing the snow upon the Alpine slopes we find that it is in a state of motion. We find that the snow has been incessantly moving down the Alpine slopes into the valleys; and hence we have the valleys filled with rivers of ice. On standing for the first time beside one of these rivers of ice you would imagine that it was perfectly motionless, and that a body so rigid as ice could not move at all; but when you make proper observations, you find that the ice is perpetually moving down, and thus we have these glaciers of the Alps. I have no doubt that every boy here will one day visit those glaciers for himself. I have here a sketch of one of the most famous of those glaciers. It is called the "Mer de Glace," and is situated near Chamounix. This Mer de Glace has its great feeders from the snows that fall upon Mont Blanc and the series of mountains which are rudely sketched in this diagram. Here is a great cascade where the snow, after being half consolidated—squeezed together so as to form ice—actually moves down, forming a cascade of ice which comes along this valley. Here is another basin where the snows collect, and where its particles are squeezed into ice, and you have this ice also always in a state of motion.

Now let us look at the lines which I have drawn on the diagram. The mountains beside the glaciers are always sending down stones and dirt, and consequently you always have lines of dirt carried down; and you see that where two glaciers have their sides turning and uniting as here shown, they form a line along the middle of the trunk of the glacier. Now, these lines which I have mentioned are called *moraines*. Those at the side are called *lateral moraines*, and those in the middle are called *medial moraines*. We have in the Mer de Glace these three moraines. If we examine this glacier we find that notwithstanding the rigidity of ice, it moves down like a river. Eminent men have worked at this subject; Saussure worked at it a little —not much, and was followed by Bordier, who observed that ice behaved almost like a viscous body. He was the first to propound the fact that ice was of this character. He was followed by Rendu, who also took up the idea that ice behaved like a viscous body such as honey, or treacle, or tar, or paste. Then he was followed by Mr. Agassiz and another, and they determined the velocity with which this ice falls. Then came Principal Forbes, an eminent Scotchman, and his measurements pushed the question far beyond its former stage. And then came Mr. Huxley and myself; and we pushed the matter a little forward; and afterwards I did a little on my own account in reference to this question. It is in this way that scientific knowledge is accumulated. It goes rolling and becoming bigger like a snow-ball, and thus it is that science grows and has grown to what it is at the present day.

I want to show you now how it is that ice can behave like treacle, or honey, or tar—how it is that it behaves like lava, or paste, or a viscous body. In order to make this plain I have asked Mr. Cottrell to bring me in a mass of ice; and I hope to be able to show you by experiments in this room that we can make ice behave almost like a piece of paste—that we can mould it into any form we please. Here is our ice, and we will place it on the table in this blanket. It is clinging to the blanket, being, in fact, frozen to it. I will show you how, from an apparently little thing, we can get an explanation of a fact observed in the glaciers. This explanation is due to a little fact first observed by the greatest experimental philosopher that this world ever produced—a man who is to my feeling almost living here amongst us at the present moment—a man who lectured to the boys here, and who himself had all the tenderness, and all the brightness, and all the joyousness of a boy. I say it is by a little observation of this great man that we are able to explain those facts observed in connection with the glaciers, and to show how it is that a body so brittle as ice can behave almost like lava. I will show you the brittleness of ice. I have here a pointed instrument, a small awl, and if I prick this into the ice you see that it chips off little pieces, and that the ice breaks as clearly as any crystal would break. Now just observe what occurs among these glaciers. If we make accurate measurements upon this *mer de glace* we ascertain a very striking fact. You see in the diagram a great white glacier. Here you see another, and you see another there. I measured the width of the first glacier, and it was 1,134 yards. The second glacier is 825 yards; and the third 638 yards. If you add these together, the sum of the widths of these three tributaries of the Mer de Glace is 2,597 yards. Now, all these three tributaries of the Mer de Glace are squeezed into a space, which measures only 893 yards,—a channel only one-third of the width of the sum of the three tributaries. Now it is one of the wonderful properties of this ice that it can be thus squeezed into a narrow bed. If we take a number of stakes and set them in a perfectly straight line across this channel, and allow them to remain there for a day, and observe their position on the following day, we shall find that they are no longer in a straight line. In the observation that was made there were no fewer than 16 stakes fixed in the ice in a straight line. The stakes nearest one side of the glacier moved at the rate of 7 inches in a day; the next stake moved at the rate of 8 inches—the next 13 inches—the next 15 inches—the next 19 inches, and the next 20 inches; and then the speed began to fall off, and fell back to 15 inches at the other side of the glacier. These numbers prove a fact which is also observed in the case of rivers—that the middle of the line moves more quickly than the sides. In the same way, as was proved by Principal Forbes, the top of the glacier moves more quickly than the bottom, or the part nearest its bed, which is held back by the friction of the bed. When I visited the Mer de Glace in 1857 there was a precipice of ice, and I measured the motion of that precipice at the top and at the bottom. The top stake moved 6 inches, while the middle stake moved 4½ inches, and the bottom stake moved 2½ inches. This shows that the top of the glacier moved more quickly than its foot. Furthermore—and this is a point of great importance—if you had a river flowing through a straight valley, the middle of the river would be its point of quickest motion; but if you had a river flowing through a valley of this kind (Fig. 9) the point of quickest motion would be always that point where it is curved. It is exactly the same with a glacier. This on a large scale will represent the bed of the Mer de Glace from actual measurement. At the parts A A the point of swiftest motion is really the centre of the glacier. Here, again, at *a* and *c*, the point of swiftest motion is on one side of the centre. Here, again, at *b*, it crosses to the other side of the centre. The dotted line is the centre, and the continuous line marks the points of the quickest motion on the Mer de Glace.

FIG. 9.



Now, how is it that a glacier is thus able to behave as a river? We will see. I will now cut two pieces from this block of ice. We see that the ice is now melting in the atmosphere of this room, and there is no surplus cold in it to enable it to freeze again; and still, strange to say—(and this was the observation that Mr. Faraday made)—if we place those pieces of ice together, though the surfaces are now melting, they instantly freeze together. Although there is no surplus cold in the ice, the mere bringing them together causes the film of water, which a moment ago was moisture, to become ice. This curious freezing together has received the name of "regelation," a term for which those who first worked at the subject were indebted to Dr. Hooker. In consequence of this freezing together you can actually convert snow into ice. Every boy knows the state of snow which is fit for a snowball. It ought to be soft, and yet by proper pressure you can make it perfectly hard if you are wickedly inclined. Now, I have no snow here, but I will try and obtain snow by scraping the surface of the ice. In this way I get a kind of snow, and here is a flannel in which to receive it. I will take this snow and put it into a proper mould C D, and squeeze it together. In the absence of real snow I make the snow required for the experiment by crumbling the ice in this way. I will now make a snowball, and I am enabled to do this by the power which the small particles of ice have of freezing together in the manner I have just indicated. I cannot by my hand squeeze strongly enough the mould containing these particles of scraped ice; and therefore I will place the mould under the hydraulic press, as this machine is called. In this way I hope to obtain a snowball. [The operation described was then performed, and the mould, on being withdrawn from the press, was found to contain a ball of solid ice.] Now, here we have a snowball (B), such as you have never seen before, and this is due to the fact that on bringing the surfaces of the little particles of ice in contact they freeze together. This is not an ordinary snowball at all, and it is one which no boy would like to be hit with. It is a ball of solid ice, produced from the small particles which have frozen together in virtue of this wonderful property called regelation; and it is in virtue of this property that ice on the surface of water, though shattered into pieces, will mend itself; and all the tearings and ruptures of the glaciers are mended by means of this quality of regelation which was discovered by Mr. Faraday. I have here several experiments arranged to illustrate this subject.

FIG. 10.

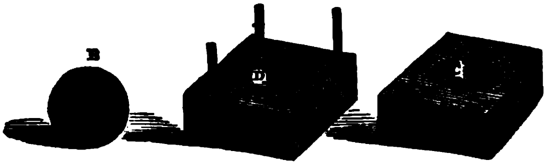
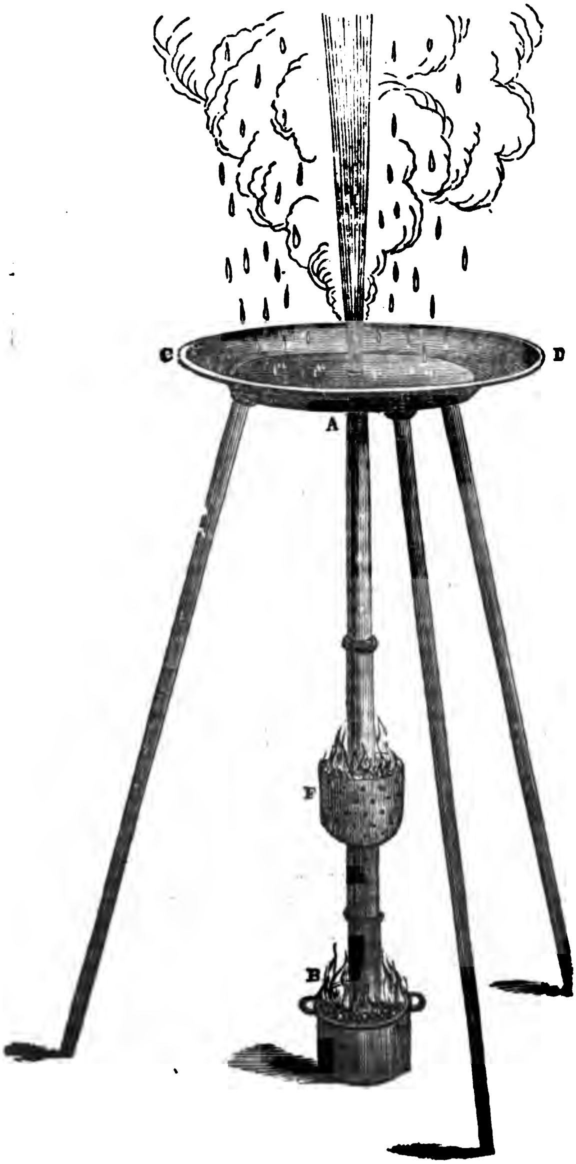


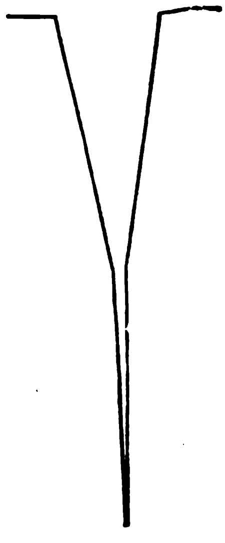
FIG. 11.



[Particles of scraped ice were then moulded into the form of rings and hemispherical cups, by the same means as had been employed in the production of the solid ball. Two hemispherical cups were afterwards placed with their edges in contact, when they froze together and formed a hollow sphere of ice.] These experiments will show you on a small scale how possible it is for particles of a substance perfectly brittle to freeze together wherever they touch, on account of the substance possessing the power of regelation. You see that a substance of this character behaves as if it were not brittle at all, and acts like a paste. In this way we might make statuettes, or, in fact, mould the ice into any form we pleased. You might drink out of these cups, and the ice of which they are made would cool the water for you. I am sorry I have not a little cooled wine to offer you from a cup of this kind. (Laughter.) I have made champagne glasses and all manner of things by thus compressing ice. In this way by these small experiments we illustrate and make plain to ourselves those wonderful things that go on among the glaciers of the Alps; and we entirely clear up the difficulty as to how it is that a body so brittle as ice can behave as a viscous body. I must now leave this subject of ice and its properties.

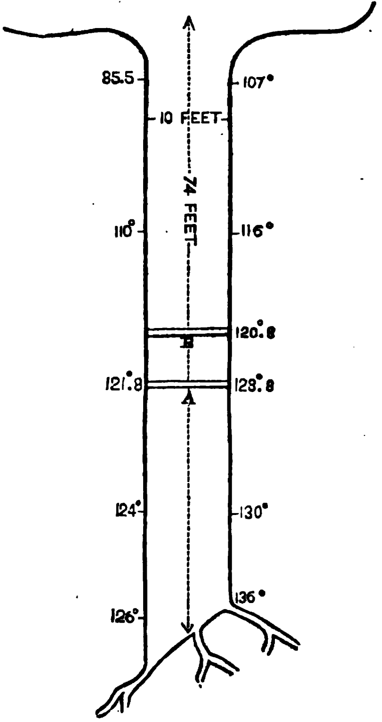
There is in operation before you an apparatus for illustrating the action of the geysers in Iceland; and in the other room is a beautiful painting of the geysers, presented by our president, Sir Henry Holland, who was there in 1810 with Sir George Mackenzie. In a short time this tube will throw out a column of water, but I do not think I shall be able to make the operation plain to you in this lecture. When Sir Henry Holland and Sir George Mackenzie visited the great geyser, Sir George Mackenzie supposed that the geyser had underneath it a great cavern, and that this was partly filled with water, the geyser itself being a tube. He supposed the water to become heated beneath, and the steam to force the water up into the tube. This is the theory given by Sir George Mackenzie; but it is not at all necessary to suppose the existence of this cavern. The spring itself has built its own tube, and the tube is a sufficient apparatus to produce these wonderful eruptions that astonish everybody who has ever seen them. The geyser tube is represented here in section (see Fig. 13). It is seventy-four feet deep, and is lined with a most beautiful plaster. It opens out at the top into a basin fifty-two feet wide in one direction, and sixty feet wide in the other. [The apparatus for illustrating the geyser was then put in action, and a thick stream of boiling water was presently ejected upwards. (See Fig. 11)]. Now I must make another eruption for you. I want to produce an imitation of the spring called the strokkur (shown in section at Fig. 12). This is a very celebrated spring which you will see in Sir Henry Holland's painting beside the real geyser. (I must explain in the next lecture how it is that we have two fires in this apparatus.) It is usual for the natives of Iceland to stop the mouth of the strokkur by throwing in clods. I will now imitate that practice by putting in a cork at the end of the tube. In a short time the cork will be ejected, and I should not be at all surprised if the water reached the ceiling. I think the last experiment made at the strokkur was made by Commander Forbes. He wrapped a leg of mutton in a towel and stopped the mouth of the strokkur by means of that leg of mutton. The leg of mutton came out well cooked, and was projected to a great height in the air. Various people have estimated the height of these eruptions in Iceland Sir Henry Holland tells me that he saw one of more than one hundred feet; and Sir George Mackenzie gives ninety feet as the height of the eruption. The earlier observers made the height very much more. Two Danes, named Aulafsen and Paulson, who were the first to observe the height, state that the geyser pitched its water to a height of 360 feet. Two observations, which may be regarded as perfectly trustworthy, were made by Bunsen, of Heidelburg, and the height was measured by a theodolite. In the last of these observations, which was made on the 16th of July, 1841, the height was estimated at 162 feet, and we may rely upon this observation as being accurate. Now, as I have said, the tube of the geyser is the cause of the eruption; and when we see an eruption produced by a small tube, as in this model, we may regard it as proved that it alone is a sufficient cause, and that there is no need for the supposition that there is a cavern underneath.

FIG. 12.



Bunsen suspended thermometers at various depths below the basin of the geyser to ascertain the temperature of the water. I have marked on this diagram the various temperatures

Fig. 13.



which he found at different depths. At the top the temperature was 8·45° C., and extended to 126·5° C. as the depth increased. Now, how is it that the water does not boil in the geyser when the temperature is over 100° C.? Every boy here will be able to tell me that it is because the water at that depth has to bear not only the pressure of the atmosphere, but also of the mass of water which is above it in the tube. For this reason it cannot boil at the temperature which Bunsen ascertained. At the depth at which the water in the geyser was found to have a temperature of 126·5°, the boiling temperature would be 136°. At no point does the temperature of the water reach the boiling point for the pressure to which it is subjected.

[At this stage of the lecture the cork flew from the mouth of the model of the strokkur, and a copious stream of boiling water was projected to the ceiling of the theatre.]

I must defer the explanation of the action of geysers until the next lecture.

LECTURE IV.

The Geysers of Iceland (continued).—The mechanical equivalent of heat.—Consumption of heat.

In our last lecture I intended, if time permitted, to explain the action of the geyser of Iceland, but at the end of the lecture I found that the time was insufficient for the purpose; and I promised then to explain this wonderful spring in the lecture of to-day; but when I came to look at the other matter before me I found that it was so abundant that I really could not get the subject of the geyser into it. In order to help myself, therefore, I printed 500 copies of an account of the geyser which I gave in this room 14 or 15 years ago; and I trust each young philosopher present has furnished himself with a copy of this description of the geyser, from which I have no doubt you will understand its philosophy—particularly by the help of your friends—when you read this paper at home, just as well as if I had tried to explain it to you here.

"The surface of Iceland slopes gradually from the coast towards the centre, where the general level is about 2,000 feet above the surface of the sea. On this, as a pedestal, are planted the Jökull or icy mountains of the region, which extend both ways in a north-easterly direction. Along this chain the active volcanoes of the island are encountered, and in the same general direction the thermal springs occur, thus suggesting a common origin for them and the volcanoes. From the ridges and chasms which diverge from the mountains mighty masses of steam are observed to issue at intervals, and where the escape takes place at the mouth of a cavern and the resonance of the cave lends its aid, the sound of the steam is like that of thunder. Lower down in the more porous strata we have smoking mud pools, where a repulsive blue-black aluminous paste is boiled, rising at times into huge bladders, which on bursting scatter their slimy spray to a height of fifteen or twenty feet. From the base of the hills upwards extend the glaciers, and on their shoulders are placed the immense snow-fields which crown the summits. From the arches and fissures of the glaciers vast masses of water issue, falling at times in cascades over walls of ice, and spreading for miles and miles over the country before they find definite outlet. Extensive morasses are thus formed, which add to the monotony of the dismal landscape. Intercepted by the cracks and fissures of the land, a portion of these waters is conducted to the hot rocks underneath; here meeting with the volcanic gases which traverse these underground regions, both travel together, to issue at the first convenient opportunity either as an eruption of steam or as a boiling spring.

"In the Great Geyser we have a tube ten feet wide and seventy feet deep; it expands at its summit into a basin, which from north to south measures fifty-two feet across, and in the perpendicular direction sixty feet. The interior of the tube and basin is coated with a beautiful smooth plaster, so hard as to resist the blows of a hammer. The first question that presents itself is, how was this wonderful tube constructed? How was this perfect plaster laid on? A glance at the constitution of the Geyser water will perhaps furnish the first surmise. In 1,000 parts of the water the following constituents are found:—

Silica	0·5097
Carbonate of Soda	0·1939
Carbonate of Ammonia	0·0083
Sulphate of Soda	0·1070
Sulphate of Potash	0·0475
Sulphate of Magnesia	0·0042
Chloride of Sodium	0·2521
Sulphide of Sodium	0·0088
Carbonic Acid	0·0557

"The lining of the tube is silica, evidently derived from the water; and hence the conjecture may arise that the water deposited the substance against the sides of the tube and basin. But the water deposits no sediment, even when cooled down to the freezing point. It may be bottled up and kept for years as clear as crystal, and without the slightest precipitate. A specimen brought from Iceland and analysed in this Institution was found perfectly free from sediment. Further, an attempt to answer the question in this way would imply that we took it for granted that the shaft was made by some foreign agency, and that the spring merely lined it. A painting of the Geyser, the property of Sir Henry Holland—himself an eye-witness of these wonderful phenomena,—was exhibited. The painting, from a sketch taken on the spot, might be relied on. We find here that the basin rests upon the summit of a mound; this mound is about forty feet in height, and a glance at it is sufficient to show that it has been deposited by the Geyser. But in building the mound, *the spring must also have formed the tube which perforates the mound:* and thus we learn that the Geyser is the architect of its own tube. If we place a quantity of the Geyser water in an evaporating basin, the following takes place; in the centre the fluid deposits nothing, but at the edges where it is drawn up the sides of the basin by capillary attraction, and thus subjected to a quick evaporation, we find silica deposited; round the edge we find a ring of silica thus laid on, and not until the evaporation has continued for a considerable time do we find the slightest turbidity in the central portions of the water. This experiment is the microscopic represeentant, if the term be permitted, of Nature's operations in Iceland. Imagine the case of a simple thermal spring whose waters trickle over its side down a gentle incline; the water thus exposed evaporates speedily, and silica is deposited. This deposit gradually elevates the side over which the water passes until finally the stream has to choose another course; here the ground becomes elevated by the deposit as before, and the stream has to move forward—thus it is compelled to travel round and round, discharging its silica and deepening the shaft in which it dwells, until finally, in the course of centuries, the simple spring has produced that wonderful apparatus which has so long puzzled and astonished both the traveller and the philosopher.

"Before an eruption, the water fills both the tube and basin, detonations are heard at intervals, and after the detonation a violent ebullition in the basin is observed; the column of water in the pipe appears to be lifted up, thus forming an eminence in the centre of the basin and causing the water to flow over its rim. The detonations are evidently due to the production of steam in the subterranean depths, which rising into the cooler water of the tube, becomes suddenly condensed and produces explosions. Between the interval of two eruptions, the temperature of the water in the tube gradually increases, but even immediately before an eruption, at no part of the tube is the water at its boiling temperature. How then is an eruption possible? Bunsen succeeded in determining the temperature of the water a few minutes before a great eruption; and his observations furnish the key of the entire enigma. A little below the centre he found the water within two degrees of its boiling-point, that is, within two degrees of the point at which water boils under the pressure of the atmosphere, *plus the pressure of the superincumbent column of water*. The actual temperature at thirty feet above the bottom of the Geyser was 122° Centigrade, its boiling-point being 124°. We have just alluded to the detonations and the lifting of the Geyser column by the entrance of steam from beneath. These detonations and the accompanying elevation of the column are, as before stated, heard and observed at various intervals before an eruption. Imagine, then, the section of water at thirty feet above the bottom to be raised six feet by the entrance of a mass of vapour below. The liquid spreads out in the basin, overflows its rim, and thus the elevated section has six feet less of water pressure upon it; its boiling-point under this diminished pressure is 121°; hence in its new position its actual temperature (122°) is a degree above the boiling-point. This excess is at once applied to the generation of steam; the column is lifted higher, and its pressure further lessened; more steam is developed underneath; and thus, after a few convulsive efforts, the upper part of the column of water, through the sudden boiling up from the middle downwards, is ejected with immense velocity, and we have the Geyser eruption in all its grandeur. By its contact with the atmosphere the water is cooled, falls back into the basin, sinks into the tube through which it gradually rises again, and finally fills the basin. The detonations are heard at intervals, and ebullitions observed; but not until the temperature of the water in the tube has once more *nearly* attained its boiling-point is the lifting of the column able to produce an eruption.

"In the regularly formed tube the water nowhere quite attains the boiling-point. In the canals which feed the tube, the steam which causes the detonation and lifting of the column must therefore be formed. These canals are in fact nothing more than the irregular continuation of the tube itself. The tube is therefore the sole and sufficient cause of the eruptions. Its sufficiency was experimentally shown during the lecture. A tube of galvanised iron six feet long was surrounded by a basin; a fire was placed underneath and one near its centre to imitate the lateral heating of the Geyser tube. At intervals of five or six minutes throughout the lecture eruptions took place; the water was discharged into the atmosphere, fell back into the basin, filled the tube, became heated again, and was discharged as before.

"Sir Geo. Mackenzie, it is well known, was the first to introduce the idea of a subterranean cavern to account for the phenomena of the Geyser. His hypothesis met with general acceptance, and was even adopted undoubtingly by some of those who accompanied Bunsen to Iceland. It is unnecessary to introduce the solid objections which might be urged against this hypothesis, for the tube being proved sufficient, the hypothetical cavern disappears with the necessity which gave it birth.

"A moment's reflection will suggest to us that there must be a limit to the operations of the Geyser. When the tube has reached such an altitude that the water in the depths below, owing to the increased pressure, cannot attain its boiling-point, the eruptions of necessity cease. The spring, however, continues to deposit its silica, and forms a *laug* or cistern. Some of these in Iceland are of a depth of thirty or forty feet. Their beauty is indescribable; over the surface a light vapour curls, in the depths the water is of the purest azure, and tints with its own hue the fantastic incrustations on the cistern walls; while at the bottom is observed the mouth of the once mighty Geyser. There are in Iceland traces of vast, but now extinct, Geyser operations. Mounds are observed whose shafts are filled with rubbish, the water having forced a way underneath and retired to other scenes of action. We have in fact the Geyser in its youth, manhood, old age, and death, here presented to us:—in its youth, as a simple thermal spring; in its manhood, as the eruptive spring; in its old age, as the tranquil *laug;* while its death is recorded by the ruined shaft and mound which testify the fact of its once active existence.

"Next to the Great Geyser the Strokkur is the most famous eruptive spring of Iceland. The depth of its tube is forty-four feet. It is not, however, cylindrical like that of the Geyser, but funnel-shaped. At the mouth it is eight feet in diameter, but it diminishes gradually, until near the centre the diameter is only ten inches. By casting stones and peat into the tube and thus stopping it, eruptions can be forced which in point of height often exceed those of the Great Geyser. Its action was illustrated experimentally in the lecture, by stopping the galvanized iron tube before alluded to loosely with a cork. After some time the cork was forced up, and the pent-up heat converting itself suddenly into steam, the water was ejected to a considerable height; thus demonstrating that in this case the tube alone is the sufficient cause of the phenomenon."

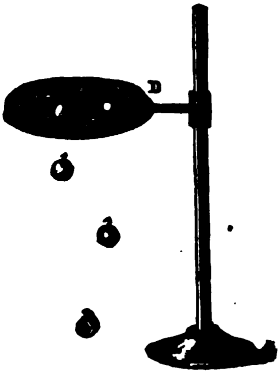
Throughout the lectures that have been hitherto given I have had occasion to admire the attention and patience of my younger hearers. My hearers are of different ages, but although I have been obliged to mention certain things that could not possibly be understood by the very young boys, and to mention some elementary facts which were, perhaps, very well understood by the older boys, yet the young boys have been patient when I spoke to the elder ones, and the elder ones have been patient when I spoke to the younger boys; and for this I feel very thankful. With reference to the present lecture I have to address all the boys, especially the elder ones, for I have to explain a term or two very much used at the present time in connection with the subject of heat.

If you carry a pound of any substance whatever to a height of 772 feet above the earth's surface, and allow it to drop down upon the earth from that height, you always get the same amount of heat generated, and that amount of heat would be just sufficient—I mean neither more nor less than sufficient—to raise the temperature of one pound of water one degree Fahrenheit. Thus, if you conceive a pound weight falling from this great height, 772 feet, and conceive all the heat generated by its collision with the earth collected together and put into a pound of water, that pound of water would have its temperature elevated one degree. Now, by proper means we can reverse this process, and by means of heat we can lift the pound weight. If we lift the pound weight to a height of 772 feet, of course we should then be pulling it, as it were, away from the earth which attracts it; and in order to lift this pound weight to that height we should consume—in fact, annihilate, destroy—an amount of heat equal to that which would raise a pound of water one degree in temperature; so that the amount of heat consumed in lifting the weight 772 feet is exactly equal to what is generated when the weight falls from a height of 772 feet. Now, if we lift one pound of matter one foot from the ground, a certain term is employed. It is called "*the foot-pound;*" and if you lift a pound weight to 772 feet it is 772 foot-pounds; or if you lift 772 pounds to the height of a foot you have 772 foot-pounds. Now, this quantity of 772 foot-pounds, which would raise the temperature of a pound of water one degree, is termed "*the mechanical equivalent of heat.*"

In lifting a weight from the earth we are overcoming attraction of the earth, and in doing this we consume heat, if heat be the agent which lifts the weight. Now, I have asked you over and over again to figure the atoms of solid bodies such as this I hold in my hand. As a general rule, when heat is communicated to a body the atoms are forced asunder. You know the enormous power and force with which these atoms may attract each other, for I showed you that when an iron bar was cooled the contractible force pulling together its atoms—the mutual attraction of its atoms on cooling—was sufficient to smash the steel bar which you saw broken in front of the table. Now, we have amongst the atoms of bodies pulling each other together an action substantially the same as that which occurs when we separate the weight from the earth. To this action we may give a name. Let us call this work which occurs in a body "atomic work" if you like—work done on the atoms. This work necessitates a consumption of heat. Heat is consumed in this way; and what I want you now to bear in mind is that the amount of heat consumed is very different indeed in different bodies; and consequently some bodies, in order to raise them one degree in temperature, require more heat than others. In order to raise one pound of the liquid metal mercury one degree in temperature a certain amount of heat must be imparted to it. It would require *thirty times* that amount of heat to raise a pound of water one degree in temperature. Water requires thirty times the quantity of heat required by mercury, simply because the work to be done is a great deal more than that necessitated in the case of mercury. Now I want to show you what follows from this action. It would appear, in consequence of this atomic work which I have been speaking of, as if the water had a power of storing up heat thirty times greater than the power possessed by mercury; and, indeed, formerly people thought that heat *was* something stored up, and they called the amount of heat which it was needful to impart to a body to raise its temperature one degree its "capacity for heat." They looked at a body as a kind of vessel for heat, and hence they used this term "capacity for heat." It was found by experiment that the capacity for heat (as the term went) was very different in different bodies; and the amount of heat which a body had stored up was determined by what the body could do—by the amount of ice or wax which it could melt.

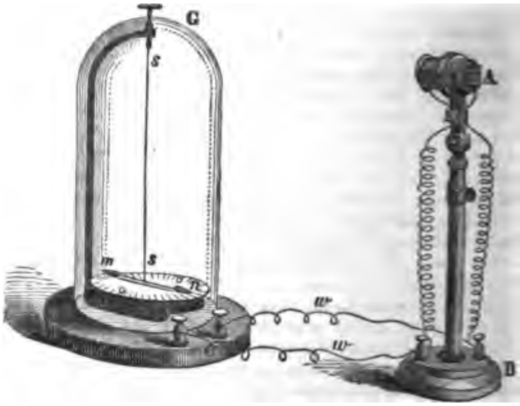
I have here a vessel of hot oil, and in it I have spheres of metal of different kinds. They are all equally hot at the present time; but you will find that these spheres of metal have very different powers in melting bodies. They will be placed on a flat piece of wax, D (Fig. 14), and their heat will act upon that piece of wax. Some will force their way through, and others will not. This ball of copper will go through the wax first. The tin will go partly through. The bismuth certainly will not go through, although it is just as hot as the copper. Here, too, we have a ball of lead which is not competent to melt its way through the wax. The ball of iron will go through. Here is a ball of zinc; I think that will go through; but I am sure that the lead and tin and bismuth will not do so. [The balls of copper, iron, and zinc melted their passage through the slab of wax, and fell to the ground one after the other. The three other balls did not perforate the wax.] This illustrates the different amounts of heat possessed by these bodies, although they are all at the same temperature.

Fig. 14.



We must now go on considering the heat consumed; and I must rapidly make a few experiments illustrative of the consumption of heat in this work of forcing the particles of bodies asunder or changing their position. One of the most remarkable causes of the consumption of heat occurs when a body is caused to pass from the solid state to the liquid. Here, A B (Fig. 15), I have a beautiful instrument) the thermo-electric pile), which has been introduced to your attention before. It is a kind of thermometer, and I want to show you how we can make use of this instrument for the purpose of ascertaining whether we have cold or heat. I cannot go into the full explanation of the thing; but if you observe the needle *m n* of the galvanometer G, to which it is connected by the wires *w w*, you will see how wonderfully delicate the instrument is. It is more delicate than any thermometer whatever. I will turn the face of that instrument towards

Fig. 15.



me, or I will breathe against it, or I might allow any young philosopher present to breathe against it. The warmth of his breath would at once make itself evident by causing that magnetic needle to move. Now, as I breathe against this pile, you observe that the red end of the needle comes towards me. When the needle returns to its former position and comes to rest, I will try the effect of cold upon the instrument, which, you will remember, is called a thermo-electric pile. (You see I can stop the needle by means of this other needle in a moment.) I will now put a piece of this ice in a spoon, and on the cold spoon coming in contact with the face of the pile you will see that the red end of the needle will move towards you, and away from me. Thus, in this instrument we have the means of telling whether heat or cold has been imparted. We now again bring the needle to rest. And now we have made the acquaintance of this beautiful instrument, I will proceed to experiment with it. Here is a little flat basin, B, which I place upon the face of the pile thus; and you observe that although that dish has been up to the present time resting upon the table it has become a little warm, and causes the red end of the needle to move towards me. But when I pour a little cold water into this dish you see the suddenness of the movement of the red end of the needle towards you. I will now warm this water by dipping my finger into it, and after a time you will see that the needle will come down in consequence of the warmth imparted to it by my hand, and come back on the other side of the middle line. [After a pause.] You see that the needle now comes to my side, showing that the water is warmed by my finger. And now I might take sugar, or salt, or saltpetre, which would be still better, and put a little of the powder of that saltpetre into the water. That powder would become liquefied, and on its melting the warmth of the water is consumed—is used up, and the water is thereby chilled. Now, in making this experiment I will confine myself to a particular substance called sulphate of soda. You see that there is now a very great deal of heat imparted to the water by my finger, and that the needle comes very much on my side of the middle line. I will now pour into the water some powdered sulphate of soda, and you find that the water immediately becomes chilled by melting that sulphate of soda. This, then, is a consumption of heat by the act of liquefying or melting the sulphate of soda. I want now to make another experiment. It is a very instructive one. I want to show you the reverse of the last experiment. When dissolved sulphate of soda is permitted to solidity—become solid—you get out of it the heat that was expended in rendering it liquid. I have in this flask, B (Fig. 17), some dissolved sulphate of soda. It was carefully melted last night, and has been carefully kept apart from anything which could disturb it. We will allow the face of the pile to rest against the bottle; and now I want to cause that body to solidify before your eyes. I can cause it to become crystallised sulphate of soda, like that which was dissolved in that dish a moment ago. You will see the liquid in the flask become more and more opaque, and when it begins to solidify opposite the face of the pile it will give out heat—the heat that was expended in melting it, and you will then see the red end of the needle come towards me. I will now open the neck of the flask, and throw a crystal of sulphate of soda into the solution. [This was done, and the contents of the flask began to solidify from the top downwards.] You now see the compound crystallising; and the moment that portion opposite the face of the pile becomes solid, heat will be communicated to the face of the pile, and we shall get a deflection (as it is called) of the red end of the needle in the direction in which I stand. [After a pause]—What I predicted was quite right. There we get out of the sulphate of soda the heat that was expended in melting it. There is the movement of the needle caused by the heat.

FIG. 16.

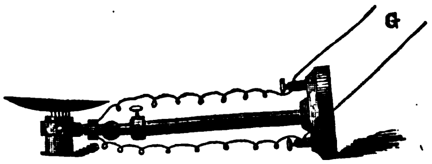
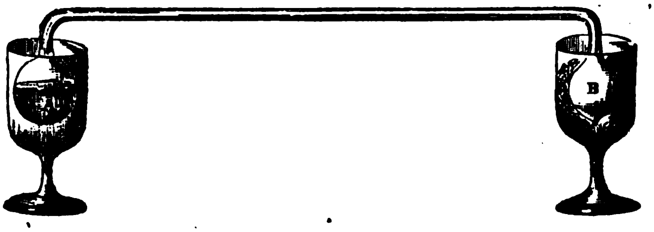


FIG. 17.

I might go on in this way, and show you that when a body is evaporated you also get a very large amount of heat consumed—used up—in order to evaporate it. In order to convert a pound of water at 212° Fahrenheit into steam at 212° Fahrenheit, an enormous amount of heat is required. It requires as much heat as would raise 967 pounds of water 1° Fahrenheit; and this heat is insensible to the thermometer, although it is so great. The reason that I employed a mixture of ice and salt as a freezing mixture in a former experiment, was that the action of the salt produces a liquefaction of the ice, and on that liquefaction taking place a large quantity of heat is consumed—so much that the temperature of the liquid is reduced far below the temperature of the ice itself. I am going to illustrate this point by the development of cold by vapourisation; and if things go fairly I should not wonder if I could freeze water before your eyes by means of its own evaporation. An experiment has been arranged there for the purpose. Here are two bulbs, A and B, in this apparatus (Fig. 18), and the water which was in one of them has been frozen in this room since the lecture began. One end of this has been placed in a freezing mixture far away from the bulb where the water is frozen. This instrument is called a "cryophorus," or ice carrier. Water was placed in one bulb, and the air was taken from the interior of the instrument. The other bulb was placed in a freezing mixture, and as the vapour came up from the water it was condensed by the freezing mixture, and the vapourisation which took place has been sufficient to freeze the water.

FIG. 18.



So much, then, for the heat consumed in causing a body to pass from the liquid state to the state of vapour. I have on the table various substances which would enable me to illustrate this in a very satisfactory manner. For instance I will take a little alcohol, and warm it by placing my finger into it, thus. I see there is a great amount of heat in the face of the pile. I have no doubt that the evaporation of the alcohol will very soon cause the end of the needle to come down; or if I take a substance that can vapourise more rapidly than alcohol—this substance, ether—it would not take an instant in order to overcome the heat which is the cause of that deflection. I will cause evaporation to go on a little more quickly, and if the needle be not held fast by some accident we shall soon find the heat which causes the present large amount of deflection entirely abolished, and the needle will move down. Now you see the needle comes back. We get an enormous amount of cold by the evaporation of ether, so much that we can easily freeze water by it.

(To be continued.)

DR. LETHEBY ON FOOD.

Lecture I.

(FROM OUR OWN REPORTER.)

On Monday evening, the 20th January, Dr. Letheby, at the rooms of the Society of Arts, John Street, Adelphi, delivered the first of a series of four lectures "On Food." The Chairman—W. Hawes, Esq., President of the Society—having briefly introduced the lecturer as "our best authority on the subject about to be illustrated," Dr. Letheby commenced his lecture by observing that the economy of food was a subject of national importance; that muscular strength was co-equal with the amount and quality of food taken into the body; and that calamity and actual want, the absence of proper diet, the neglect of protection against weather, and disordered sanitary appliances, fell heaviest upon those least able to bear the burden. Bad, however, as the immediate consequences are, they are nothing to the sickly race which comes afterwards. We must not overlook the nutritive value of our raw materials of foods, and we must therefore endeavour to erect a standard of comparison between different articles. The erecting this standard is a subject of the greatest difficulty. First, where some article may be considered without any other—milk or bread for example. Then the difficulty is that the proportions of these several constituencies differ in a great degree from the constituents of most other articles by certain different proportions of the several constituents. Again, as nitrogenous matter in food is the most important object, chemists ask how much nitrogenous matter is there in any given article of food? But even this test was not correct, even though it bore the support of Liebig, for upon his principle that an adult must take into his frame per diem 1,200 grains of nitrogenous matter; provided that he lived solely on beer or porter, he would have to drink 180 pounds of the said liquid to make up the required number of grains. The question now comes, what is the exact relative proportion of nitrogen and carbon necessary to sustain man without much labour. Dr. Lyon Playfair has sought in our workhouses, Dr. Edward Smith amongst the Lancashire weavers, to see the minimum a person can exist upon. Dr. Lyon Playfair says 4,100 grains of carbon to 190 grains of nitrogen; and Dr. Edward Smith (who observed the Lancashire weavers just at the point when the food was failing to support life), that the proportion an adult woman required was 3,900 grains of carbon and 180 grains of nitrogen; whilst for an adult man 4,300 grains of carbon and 200 grains of nitrogen were required; the average of which is 4,100 grains of carbon and 190 grains of nitrogen (thus agreeing with the statement of Dr. Lyon Playfair), contained in two pounds three ounces of bread. This statement almost agrees with another set of observations taken on a different principle; but as regards the chemical properties of the nutritive value of food, it may be convenient to observe that the relative proportions of the two in any article must be as 100 of carbon to 5½ of nitrogen.

I will now proceed to take a glance at the various kinds of food, first of all remarking that there is no such thing as animal food, but that all food, directly or indirectly, belongs to the vegetable kingdom. Animals have no power to construct food; their functions, instead of building up, pull down food, and although therefore whilst we are eating meat we may say we are eating animal, yet indirectly we are eating animal food; and it is with this primary idea that all food belongs to the vegetable kingdom that I commence my review of the raw materials of food.

Wheat is the first and the most important. There are two kinds, the summer and the winter wheat. Seasons, soils and climates affect the quality of the crop; the warmer the weather, the richer the grain. Here (pointing to the wall) is a diagram showing the construction of wheat. A cone, the outer covering of which is composed of fibrous and woody matter, in the interior of which a farina peculiar in its form is to be observed. There are various kinds of wheat, the best white tails, fine pollards, coarse pollards, and bran; and although taken per bushel the bran is the cheapest, yet taken as per 20 lbs. it is found to be dearer than many shades superior quality. Dr. Letheby then went into the details of the chemical composition of wheat, showing that it was composed upon a principle which made it a most wholesome food, stating that the relative proportions of nitrogen and carbon were as 1 of the former to 6$\frac{7}{10}$ths of the latter. He also observed that whilst the best wheat was made into flour, he thought there was a fact which proved that the coarser kinds were rather too much neglected, for, in proportion as we went lower down the scale in coarseness it was found at the same time that the quantity of nitrogenous matter increased in a large degree. Dr. Letheby then successively went through the various kinds of grains, barley, oats, rye, maize, rice, showing in each case the chemical composition which indicated their respective nutritive values; showing the reasons that they either possessed no gluten, or else too much fat—why it was impossible to make them into bread; showing also, as in the case of rice, that a combination with some substance containing a great amount of nitrogenous matter was required to make them palatable; passing to a consideration of succulent vegetables, devoting some space to the consideration of the potato, stating what an effectual remedy it was against scurvy, and saying a few words respecting the other well known vegetables; then proceeding to a brief consideration of fish food; and turning his attention lastly to animal food, referred to the preference given by the poorer classes to bacon instead of butcher's meat, for the reason that it was cheaper to buy, easier to keep, was easier to cook, and less of it wasted away during this operation, and possessed a greater relish; concluding by observing that whilst the large amount of provisions that were required to feed near upon three millions of souls, were brought almost to our very doors with the regularity of clock-work, that this regularity was effected not by Governmental help nor municipal interference, but by the mighty influence of free trade.

Loud and prolonged applause greeted Dr. Letheby at the close of his lecture, and he informed his audience that the very perfect specimens by which he had illustrated his lecture had been kindly lent him for the occasion by Mr. Twining from his Economical Museum at Twickenham.

Lecture II.

Dr. Letheby delivered this, his second lecture "on Food," at the rooms of the Society of Arts, John street, Adelphi, on Monday evening, the 27th instant, in the presence of a numerous audience. Dr. Letheby commenced by stating that the relative properties and digestive functions of food were purely physical and chemical in their character. There was, he remarked, a greater number of solvents in the alimentary canal than one would suppose. The saline and gastric juices acted with great force, and the quantity of those foods poured in for the purposes of digestion amounted to rather more than 3 gallons in the twenty-four hours. The amount of saline was 3½ lbs., in which there was 231 grains of solid matter; 14$\frac{1}{10}$ lbs. was made up of the gastric juices containing 3,000 grains of solid matter, and 316 grains of pepsine; the pancreatic acid 8·82 lbs., in which was 6,000 grains of solid matter, and 784 of a peculiar principle of fat and starchy matter; bile 3½ lbs.; and the intestinal mucus ½ lb. Food in the alimentary canal is not only submitted to the action of solvents but also that of water, drenching the food rather than dissolving it. Saline matter is secreted by many of the glands, and is composed of foods all slightly alkaline at the time of digestion; though at this time the potato is strongly acid in the interval of passing from the stomach to the alimentary canal. The potato is composed of liquid and solid matter, half of which is inulin, which is something like the peculiar principle found round the germ of all seeds, and which is of such a character that it will not pass through the membranes of the body, but is discharged through the alimentary canal. There were some articles which served artificially to promote digestion, such as Liebig's extract of malt, which proved to be a powerful solvent. The gastric juice which is secreted from the glands is a thin transparent fluid, and from possessing a large proportion of organic matter called pepsine this fluid has the power of changing albumen, which is known as pepsine or albuminous peptone, and which differs from ordinary albumen in this respect that it is not coagulated by heat. This ordinary albumen will not, however, pass through the membranes till acted upon by pepsine. The digestive power of the gastric juice is destroyed if the temperature gets above 120° Fahrenheit or below 100° Fahrenheit. At the latter point the digestion does not take place very rapidly. Dr. Letheby then described how this gastric juice was obtained from the stomachs of pigs and sheep, stating that it was when the animal had been kept unfed for some time, and when his whole nervous system was excited by the smell of some savoury dish, that it was killed, and that the quantity of the pepsine produced from the stomach of the pig was better than that obtained from the sheep. He then proceeded to say, after a few words respecting pancreatic acid, observing that it was a clear colourless fluid, possessing the power of being either alkaline or acid whose functions were not well known. Twenty years ago Bernard had thought it possessed the power of making soap, but lately Mr. Schweitzer, of Brighton, had carefully gone into the subject, and found that it made fat. It possessed a powerful solutive action; and it may—though this is generally denied—act on nitrogenous matters. Dr. Letheby then proceeded to devote his attention to what he said was a very complex subject, and on which he should say but little—the bile. It was of a yellow colour, and slightly alkaline, which came into the body daily to the quantity of 3½ lbs., of which about 14 per cent. was solid matter, of which 12 are organic. Several hypotheses were presented to the meeting, and the lecturer, whilst not being able to pronounce positively upon the matter, seemed inclined to believe it to be a residuum from the liver in making blood to circulate through the body. After a few words bestowed upon the intestinal mucus, Dr. Letheby proceeded to apply the facts already gathered to an explanation of the phenomena of digestion, first taking protinaceous or nitrogenous matters digested by the gastric juices, of which there are several divisions, their order of easiness in digestion being as follows, viz.:—Liquid and soft albumen, then hard albumen, followed by fibrine, gelatine, cartilages, and appendages of the skin, in the order just named, showing, as regards the latter division, that all appendages, such as hair, feathers, and wool, were thoroughly insoluble; citing as an example, the case of the boa at the Zoological Gardens, who some years ago swallowed his blanket, which a few days afterwards was cast out of the alimentary canal uninjured; that starch was converted into the low form of sugar, known as glucose; that gums and all bodies of a similar character were indigestible, and were therefore not only worthless but did harm, causing other food to be hurried in its passage through the body, and that saline matters were digested by the acid of the stomach. Dr. Letheby then proceeded to state what were the most digestible kinds of food, quoting Dr. Bowman's observations made under highly advantageous circumstances. Dr. Bowman had a patient whose wound affected the stomach, and which required to be kept open. By these means Dr. Bowman was enabled to see the processes the food swallowed by the man underwent during the time of digestion; and also to learn the time required for the digestion of the food taken into the body. The result of his observations was that soused tripe was the most digestible of all foods, taking but one hour to digest; that venison ranked next, taking one hour and a half; then raw eggs or raw oysters, taking one hour and three-quarters; ox liver, two hours; poultry, lamb, and hard boiled eggs, two hours and a half; beef and mutton two hours and three-quarters to three hours and a quarter; pork, three hours and a quarter; and lastly, salt beef, four hours.

The question, How digestion may be helped on? possesses the following answer. There must be a proper selection of foods as regards tenderness and flavour; a proper variation of the food from day to day, and by carefully watching how this food is cooked; and by the maintenance of a proper temperature of the body and of a cheerful temper.

The chief constituent of food is water, and the liquid makes its appearance in all parts of the body to the extent of 75 per cent. Thirty pounds per diem is wanted to properly carry on the process of digestion, and its importance may be fully appreciated when we see that it dissolves tissues, and carries the blood into circulation; that it carries out of the body waste; that it cools the body when unduly heated; and that it lubricates the whole system. Like everything else, however, it must be taken in a proper manner; least in the morning, more at noon, and most at night, when it is greatly required to carry off the accumulated waste tissues, and leave the body clear for the next day's operation. Dr. Letheby then passed on to nitrogenous or plastic matter, and showed how gradually, from the belief that the nitrogen within us supplied our muscular force, it had come to be disputed, questioned, experimented upon; and that finally in the year 1866 two professors of Zurich, Fick and Wislecenus, had taken the trouble to put the matter to a practical test by ascending the Faulhorn, one of the Bernese Alps, and at an altitude of 6,417 feet above the level of the Lake of Brienz. During the day before, the time whilst they were at their work, and for a few hours after, they religiously abstained from eating any thing containing nitrogenous matter. The ascent of the mountain took six hours, and during that period, and for some time afterwards, they collected all the secreted nitrogenous matter, which by the most liberal computation but provided for half the strength requisite for these two gentlemen to reach the top of the mountain; and to prove that more nitrogen was evolved before and after the work was done, the quantity of nitrogen sensibly increased after a meat meal. Dr. Letheby then proceeded to give some interesting statistics to prove the muscular force these quantities of nitrogen represented; showing that calculation should be made for the beating of the heart, respiration, and so forth, all of which went to prove the fallacy of the assertion that the burning of nitrogenous matter gave us our muscular force. Nothing of the sort could occur till the hydrocarbons in the blood were burnt, and then the nitrogenous matter could ignite; but it would be even then the hydrocarbons which were creating the muscular power; and if a test was required to prove that it was the carbon that was thrown off by exertion, there was one at hand. Take a man: during sleep he will but exhale 293 grains of carbon in the hour; let him be lying down in a state approaching sleep, and the rate increases to 355; let him sit up, to 448; let him walk two miles an hour, 1,088; let him walk three miles an hour 1,552; and if you let him work at the treadmill at the rate of 28·65 feet per minute, 2,926. What better proof can be afforded than this, for here we have, speaking in round numbers, when all is calm and at rest, the man but breathes 300 grains of carbon per hour, whilst when he is working hard at the treadmill, 3,000. The result of the calculations must be, that the chief agent of heat and force is hydrocarbon, and that nitrogenous matter goes to replace muscles; that muscles do not decay or oxidise during working, but afterwards; and that nitrogenous matter comes from food, and not from worn-out muscle. The nitrogen must be present, there can be little doubt; our habits necessitate our eating meat, and the best example of this fact is, that a party of Hindoos who commenced to make a line of rail were obliged to dispense with the laws of their caste, and live like English navigators to enable them to complete the work. Nor can there be any doubt that too rich nitrogenous food produces force, and that a nation of meat-eaters are more pugnacious than one of vegetable or carbon eaters. A brief review was then taken of the functions of fat, starchy and saccharine matters, saline substances, and those beverages we are accustomed to indulge in at meal times, the lecture being concluded with a consideration of the question, what amount of work an average man can do in a day? The answer, based upon a comparison of the previous calculations undertaken to show the work done by the two professors whilst ascending the mountain, being that the average work a man is capable of performing, provided he is properly fed, is 10,000 lbs. lifted 1 foot high. In the inquiry, however, we learn this, that a man's external force is but the ⅕th portion of the whole force he possesses, and that an ordinary 10-horse steam engine will do the same amount of work at a cost of 5d. per hour, whereas the expense incurred by using a man would be £2 sterling.

FOREIGN SCIENCE.

PARIS, JAN. 1, 1868.

New minerals accompanying cryolite.—Fluosalts of antimony and arsenic.—Preparation of indium.—Electrical jewels.—Estimation of nicotine in tobacco.

AMONG the many valuable and very interesting researches that have recently been made in organic chemistry in this portion of the scientific world, there are some interesting facts in mineral chemistry for your correspondent to mention. M. Hagemann has discovered two minerals accompanying cryolite, they have been named by him *dimetric pachnolite* and *arksutite*. The first resembles the pachnolite described by M. Knofs: it occurs in prisms or in quadrangular pyramids, cleavable in the direction of the base, of a pinkish white colour and very brilliant. Its density is from 2·74 to 2·76, and its hardness the same as cryolite. Sulphuric acid easily attacks it. The specimen analysed contained 2 per cent. of silica, which M. Hagemann considers foreign to the composition of the mineral, to which he assigns the formula,

$$Al_2Fl_3+2(\tfrac{1}{2}Ca+\tfrac{1}{2}Na)Fl+2HO$$

Arksutite is granular, white, and crystalline, and, like the other mineral, very brilliant. Its density is from 3·03 to 3·17; its hardness is equal to cryolite. At a dull red heat it fuses, without loss of water. Analysis gave numbers corresponding to the formula $2(Ca,Na)Fl+Al_2Fl_3$. These two minerals occur at Arksut-Fiord in South Greenland, and are probably the result of the decomposition of cryolite.

M. Marignac has made an elaborate research upon the fluosalts of antimony and arsenic; some of his results will be mentioned in a future letter.

M. Richter has published a method of extracting indium from zinc; the rare element occurs in blende. The zinc is dissolved in sulphuric or hydrochloric acid, and the residue, which is composed of zinc, indium, and other metals, is treated with nitric acid. The solution is evaporated with sulphuric acid, diluted, and a current of sulphuretted hydrogen gas passed through. The indium is almost completely precipitated with the cadmium and copper. The precipitate is dissolved in hydrochloric acid, and precipitated by ammonia. By repeating the process several times the whole of the zinc and cadmium is separated. Finally, the small quantity of iron still mixed with the indium is removed by a partial precipitation with ammonia and carbonate of soda. Indium is obtained by reducing the oxide; this may be effected by heating in a current of hydrogen gas, or by the power of a voltaic battery.

A curious application of electricity has been made by a jeweller in the Rue Therèse, M. Trouvé. He makes scarf-pins, etc., with heads upon them which at the will of the wearer move their eyes. They are delighting fashionable Paris. The electro-motor is usually carried in the waistcoat pocket. It is formed of one couple, either zinc and carbon or zinc and platinum. The carbon is fixed in the vessel which holds the exciting liquid—a saturated solution of sulphate of mercury—there being an outer case in which this vessel is placed. The zinc is fixed to the lid of the case, and does not plunge into the liquid, which only fills the lower half of the vessel. So long therefore as the apparatus is in an erect position, there is no action, but when placed horizontally the current is formed. The whole apparatus makes a little case of the most trifling size. A scarf-pin with electro-motor and connections, costs from 60 francs upwards.

A process for the estimation of the nicotine contained in tobacco has been devised by M. Liecke. He exhausts the dry tobacco leaves with water acidulated with sulphuric acid, renewing the water three times, and evaporates the solution just to the consistence of an extract. This extract is treated with an equal volume of alcohol, the alcoholic solution filtered, and the residue washed with alcohol. The alcoholic solution contains all the nicotine as sulphate. The solution is evaporated, and the residue obtained from it decomposed by caustic potash in a retort heated by oil to 260° C., the nicotine being received in dilute sulphuric acid.

PARIS, JAN. 8, 1868.

Fluosalts of antimony and arsenic—Detection of salicine in quinine—New reaction for alkalies and alkaline earths—Method of estimating sulphur in iron.

So many contradictory conclusions have been arrived at by different investigators with regard to the fluorine compounds, that English chemists will have noticed with pleasure the mention made in my last letter of M. Marignac's research. The former researches of this chemist on the fluorides of niobium and tantalum led to the conclusion that they contained five atoms of fluorine. It appeared to him interesting to study the analogous combinations which antimony and arsenic seemed capable of forming. The hope of meeting in these compounds relations isomorphous with the fluoniobates and fluotantalates has not been realised: the question, however, still remains somewhat uncertain, by reason of the very restricted number of fluantimoniates and fluarseniates which it is possible to obtain well crystallised.

The antimonic fluoride M. Marignac has not been able to obtain crystallised; its solution, evaporated quickly in the cold, becomes syrupy. If heated, it decomposes, forming a white insoluble deposit, which is probably an oxyfluoride. By adding potash, soda, or ammonia to the acid solution of this fluoride and concentrating, crystals may be obtained. These fluantimoniates are deliquescent. Neither acids nor alkalies precipitate their solutions. The alkaline carbonates, after a considerable time, cause a precipitate in the cold—speedily upon boiling. The crystallised salts dissolved in water exhale the odour of hydrofluoric acid; by dissolving and evaporating repeatedly, several of these salts pass into the state of fluoxyantimoniates. M. Marignac has only studied the alkaline fluantimoniates, not having been able to obtain the others crystallised. The following is the analytical process adopted:—The water is determined by calcination with pure anhydrous protoxide of lead. For the estimation of the antimony and alkaline metal sulphuric acid is added in excess, and heat applied until the whole of the hydrofluoric acid is expelled. Flouride of antimony is not disengaged under these circumstances. The residue is suspended in water, and a current of sulphuretted hydrogen passed through the milky fluid. It is necessary to digest the fluid with the reagent for a long time before filtering. The antimony is determined in the sulphide collected, and the filtered solution is evaporated, ignited, and the alkaline sulphate obtained, weighed.

The fluorine in these compounds must be estimated, at least approximately, to distinguish the fluantimoniates from the fluoxyantimoniates. The following is the method which M. Marignac employs to effect this; he is aware the results it gives are not quite satisfactory:—A solution of pure sulphydrate of sulphide of calcium is prepared by passing sulphuretted hydrogen gas into pure milk of lime. For the analysis of 1 gramme of fluosalt the lime required is obtained by the ignition of 2 grammes of pure carbonate of lime. The filtered solution of the calcium sulphide is mixed with the solution of the fluantimoniate, and 1 gramme of pure carbonate of potash is added. A precipitate of fluoride of calcium and carbonate of lime results, the alkaline sulphantimoniate remaining in solution. The precipitate is treated by the method of H. Rose for the determination of the weight of the fluoride of calcium. The solution can be precipitated by dilute acid, and the antimony determined again.

Monopotassic fluantimoniate is obtained by dissolving antimoniate of potash in hydrofluoric acid, and concentrating the solution. It is anhydrous, and possesses the composition $SbFl_5, KFl$.

Bipotassic fluantimoniate is produced when a solution of the preceding salt is added to an excess of fluoride of potassium. It forms shining crystals; heated to 90°, they fuse in their water of crystallisation; becoming dry they lose water and hydrofluoric acid. The residue is not entirely soluble in water, a gummy substance remaining which retains fluorine. Analysis leads to the formula

$$SbFl_5, 2KFl+2H_2O.$$

Monosodic fluoxyantimoniate is obtained on adding carbonate of soda to a solution of antimonic fluoride containing excess of hydrofluoric acid. By concentration, the solution yields little crystals which are regular hexahedral prisms, terminated sometimes by a very acute rhombohedron, sometimes by a six-sided pyramid. The salt is very deliquescent. The determination of the antimony, sodium, fluorine, and water yielded numbers closely agreeing with the formula

$$SbOFl_3+NaFl+H_2O.$$

Monosodic fluantimoniate results from the solution of the preceding salt in hydrofluoric acid. Crystals are deposited upon concentrating, which at first sight would appear to be cubes. They possess the property of double refraction. The composition of the salt is expressed by the formula

$$SbFl_5, NaFl.$$

Monammonic fluantimoniate forms slightly deliquescent acicular crystals—hexagonal prisms terminated by rhombohedrons. Analysis showed this salt to contain no water of crystallisation; the numbers obtained agree with the formula $SbFl_5, NH_4Fl$. By adding to a solution of this compound ammonic fluoride in excess, and evaporating, rectangular plates are obtained which are the biammonic fluantimoniate. Analysis leads to the formula

$$2(SbFl_5, 2NH_4Fl)+H_2O.$$

M. Marignac finds the fluarseniates to be even more soluble than the fluantimoniates, and more difficult to obtain in the crystalline state. The ammoniacal salts are only obtainable as gum-like masses. Sulphuretted hydrogen decomposes the fluarseniates, but only slowly. At the end of two days the precipitation is not complete. They may be analysed by a method similar to that indicated for the fluantimoniates. No loss of arsenic is sustained in heating with sulphuric acid under redness. These salts are capable of preservation in the dry state, but their solutions evolve hydrofluoric acid, and then furnish by concentration fluoxyarseniates. Monopotassic fluarseniate is obtained by dissolving arseniate of potash in hydrofluoric acid. It crystallises out upon concentrating the solution. Analytical results correspond with the formula

$$2(AsFl_5, KFl) + H_2O.$$

Upon heating, water and hydrofluoric acid are disengaged.

When arseniate of potash is dissolved in an insufficient quantity of hydrofluoric acid, the corresponding fluoxyarseniate is formed; it may also be obtained by acting repeatedly upon the preceding compound with water. The composition is expressed by the formula $AsOFl_3, KFl + H_2O$. Heated in a tube it melts easily, evolving hydrofluoric acid vapours abundantly. Bipotassic fluarseniate results when excess of fluoride of potassium and hydrofluoric acid are added to a solution of the monopotassic fluoxyarseniate. Analysis yielded numbers agreeing with the formula $AsFl_5, 2KFl + H_2O$. Bipotassic fluoxyarseniate is produced when the preceding salt is submitted to repeated solution and evaporation; it is also formed when neutral fluoride of potassium is added to a solution of monopotassic fluoxyarseniate. Analysis leads to the formula $As_2OFl_8, 4KFl + 3H_2O$.

Mr. Bœttger has discovered a reaction of great sensitiveness for alkalies and alkaline earths. He finds an alcoholic extract of the leaves of the ornamental plant known as *Coleus Verschaffelti*, possesses the property of becoming green under the influence of alkalies. To prepare this reagent, the fresh leaves are agitated with absolute alcohol mixed with a few drops of sulphuric acid, and left digesting for 24 hours; paper soaked in the tincture becomes red, and strips of this paper are the media of applying the test. This reagent is not influenced by carbonic acid, so that the earthy carbonates contained in water may be detected with it. The sensitiveness of the reagent is so great that a strip of the test-paper presented to a jet of coal gas speedily becomes green from the presence of ammonia.

M. Parrot has indicated a method of detecting the presence of salicine in the sulphate of quinine. In effecting this he takes advantage of the action of chromic acid on salicine; by his process a quantity as small as ½ per cent. is discovered. To make the examination, the quinine salt is introduced with a little water into a flask, 2 c.c. of sulphuric acid, diluted with 4 parts of water, are added, and 4 c.c. of a concentrated solution of bichromate of potash. To the flask is fitted a curved tube which dips into a few grammes of distilled water contained in the little flask serving as receiver. Heat is applied; at the end of three or four minutes, hydride of salicyle is produced which distils. By adding to the water in the flask a few drops of solution of perchloride of iron, a more or less deep violet colour is developed.

M. Eggertz has published a paper on a method of estimating sulphur in iron and its ores. This paper is one of great practical value, and your correspondent is engaged in making a full translation which you will receive shortly; it is, therefore, unnecessary to outline the process in this place. M. Kopp prefaced it by a few sentences eulogising M. Eggertz's services in the improvement of the quantitative methods of analysing iron.

PARIS, JAN. 14, 1868.

Double Sulphocyanide of Chromium—Ozone and Antozone—Experimental demonstration.

AN extended series of compounds, which may be termed chromo-sulphocyanides, has been obtained by M. Rœsler. This chemist finds, in the first place, that when concentrated solutions of 6 parts of sulphocyanide of potassium and 5 parts of chrome alum are mixed, the violet colour gradually passes to a wine red; heat quickens the reaction. The solution filtered from the sulphates which have been precipitated by alcohol, and evaporated just to crystallisation, yields sulphocyanide of chromium and potassium. It may be purified by recrystallisation from alcohol. The salt crystallises in deep coloured quadrilateral prisms, almost black; seen by transparent light, they are of a ruby-red colour. They are not altered by the air; submitted to heat they become very dark coloured, but during cooling take a fine red tint. At 110° the water of crystallisation is driven off, the salt becoming opaque: at a more elevated temperature it is decomposed. This salt dissolves in ·72 parts of water and ·49 parts of alcohol.

Sulphocyanide of chromium and potassium is not affected by sulphide of ammonium nor carbonated alkalies, even upon boiling. A dilute solution does not change in the cold, but sesquioxide of chromium is deposited upon heating. Ammonia only destroys the combination after ebullition. Weak hydrochloric acid has no action in the cold, but upon heating there is decomposition. When to a concentrated solution of the salt, concentrated hydrochloric acid is added, chloride of potassium is separated, a yellow powder adhering which contains much sulphur; this pulverulent matter appears to be persulphocyanic acid. When the potassium salt is evaporated with hydrochloric acid, there is complete decomposition, with formation of chlorides of chromium and potassium.

Sulphocyanide of chromium and potassium does not precipitate the solutions of the alkaline earths, nor those of cadmium, cobalt, nickel, zinc, maganese, and iron. With sulphate of copper the red colour passes into violet blue. After the lapse of some time, oxide of copper is deposited. If heated, it is formed more rapidly.

Mercuric salts cause a voluminous red precipitate which collects upon ebullition; it dissolves but little in nitric acid. Mercurous salts give a yellow precipitate, changing into greenish brown; nitric acid oxidises this compound to the red one described above. The salts of tin slowly give rise to a white precipitate. The sulphocyanide of chromium and ammonium has been formed. It resembles the preceding compound crystallographically and chemically. It is prepared in a similar manner. Sulphocyanide of chromium and sodium is obtained by dissolving oxide of chromium in sulphuric acid and adding sulphocyanide of sodium. The mixture is boiled for some time, and on cooling, the sulphates are deposited. Alcohol is added, which dissolves the double sulphocyanide. The salt crystallises in small plates; it is deliquescent. In an atmosphere dried by sulphuric acid it loses water and falls to a powder of a clear red colour. At 110° the water of crystallisation is driven off, no further alteration being induced at this temperature. With reagents it manifests less stability than the salts already described. Sulphocyanide of chromium and barium is obtained by dissolving oxide of chromium in hydrochloric acid, removing the excess of hydrochloric acid by evaporation, and decomposing with sulphocyanide of barium. It is separated from chloride of barium by crystallisation. This salt crystallises in short four-sided prisms; it is deliquescent.

The barium salt furnishes double sulphocyanides by decomposition with the sulphates. A number of other metallic sulphocyanides have also been combined with sulphocyanide of chromium. The sulphocyanides of silver, lead, and zinc have been combined in this way.

All attempts to separate chromo-sulphocyanic acid failed; but M. Rœsler has found that in the decomposition of a solution of the lead or silver salt by sulphuretted hydrogen, an acid liquid of a deep red colour is obtained, which he thinks undoubtedly contains the acid.

An experiment of M. Schönbein's, illustrating the simultaneous formation of ozone and antozone, is said to be the following:—Into a flask of 500 c. c. capacity, and 3 or 4 centimetres in diameter across the neck, a little ether is poured, just enough to cover the bottom, and a spiral of red hot platinum is plunged into the vapours. It is necessary to avoid heating the flask too strongly. The platinum glows until all the ether has been destroyed. The experiment is repeated two or three times, and now the question is to demonstrate that both ozone and antozone are formed in this slow oxidation of the ether. The first is, of course, easily shown to be present by means of the iodide of potassium and starch paper. To show the presence of antozone, the flask is rinsed with a small quantity of ether, which will then be sufficiently charged with peroxide of hydrogen, to give clearly the perchromic acid reaction. Some solution of bichromate of potash is placed in a test tube, and a drop of sulphuric acid added, the ether with which the flask has been rinsed is then poured in, when the etherial layer becomes coloured a beautiful violet blue. The conclusion to be arrived at from this experiment is, that during the formation of ozone, antozone is also formed—this in the presence of water, being converted into proxide of hydrogen.

Paris, Jan. 28, 1868.

Science in the prisons.—The gallic fermentation.—Spectra of flames issuing from furnaces.—Action of the Alkaline Silicates on the molecular theory.

As showing an advantage, unrecognised, perhaps, by many, of living under enlightened rulers in a country where chemical science is appreciated, the mention of a strange fact related in one of the scientific journals may find place here. The narrator visiting a prison asked his guide, are the prisoners well nourished? "Mon Dieu, Monsieur," the man replied, "the bill of fare for each day has been prepared by a special commission, 33 per cent. nitrogenous matter, 27 albuminoid, 15 of gelatin, 18 of fibrin, 7 of hydrated matter." The guide also informed him that each prisoner had, besides, the right to 10 cubic metres of respirable air, 10,000 litres!

Some account of M. Van Tieghem's memoir on the gallic fermentation, presented to the Academy of Sciences, has been promised for these columns. M. Van Tieghem has treated the subject elaborately. At the outset he alludes to the diverse opinions which have been expressed with regard to the causes, besides the oxygen of the atmosphere, which lead to the transformation of tannin into gallic acid. One opinion attributes this result to a slow oxidation, and to the pre-existence of a soluable ferment; and some have admitted, while others have denied, the presence of sugar in the products.

1. Tannin does not undergo the metamorphosis when protected from the atmosphere. If a series of flasks be filled entirely with a solution of tannin or a filtered infusion of nut-galls, and placed in vacuo for 24 hours, then saturated with carbonic acid, carefully corked and heated, and finally sealed while hot, the solution will remain unchanged for any length of time. The transformation of tannin into gallic acid is not, then, due to the pre-existence of a soluble ferment.

2. Tannin does not undergo metamorphosis by simple contact with the air.

A solution of tannin introduced into a seris of flasks drawn out at the neck and curved, boiled for some minutes, and left in a quiet place at a temperature of about 25° C., will remain unchanged for any length of time.

3. For tannin to undergo the metamorphosis, the development of a species of fungus in the solution is essential and sufficient. The gases composing the atmosphere alone effect no change, but the atmosphere carries to the solution spores, and these require for their germination oxygen. Under these influences the tannin splits up into gallic acid and glucose, the elements of water becoming fixed. When the transformation is complete, the whole of the galic acid indicated by theory is found, but the glucose is always in less and somewhat variable proportion; the vegetation assimilates a part of it. Thus the sugar from from the tannin furnishes the hydrocarbon aliments necessary to vegetable life. For the reaction described to take place, the plant must be developed in the interior of the solution; if only developed on the surface, the amount of vegetation germinated is immensely greater, but the reaction then takes quite a different form. Large quantities of carbonic acid are exhaled, and from a concentrated solution only a small quantity of gallic acid and traces of sugar remain after a few days' exposure. It remains to be shown that the fungus, during development and life, splits up the tannin, and that the change is not the result of soluble principle secreted by the latter, capable of acting without the organism. To establish this it is only necessary to intro-

[English Edition, Vol. XVII., No. 424, pages 32, 33; No. 426, page 56.]

duce into a solution of tannin some of the vegetation from an active fermentation, and exclude the air as in the first experiment. The fungus developed is that known as the *Penicillium glaucum* or the *Aspergillus niger*.

The Austrian Professor, M. Lielegg, has made some observations with the flames coming from furnaces in which iron is worked solely by the Bessemer process. This flame is carbonic oxide in a state of incandescence. The appearance and disappearance of spectral lines mark the progress of the metallurgical oprations. At the moment when the decarburisation of the iron commences, and when it has reached the propper limit, these lines seem essential modifications. The appearance of a group of lines and of one distinct line at the violet end, marks an important stage during the formation of malleable iron; these lines disappear sooner than any of the others, this effect taking place within the last five minutes of the operation, so that they serve to denote the termination.

The action of the alkaline silicates on the animal economy has been studied by M. Husson. His experiments were made upon dogs. Solutions of silicate of sodium were administered to them; they were afterwards killed, and the organs submitted to chemical examination. These are some of the results M. Husson has arrived at. The alkaline silicates given in quantity so minute, that the contents of the stomach remain acid are completely decomposed, the same is the case when they are in very dilute solution: the intestinal juices are unable to redissolve the silica. It follows that the alkaline silicates can only enter into the blood when administered in sufficient quantity to be alkaline in the small intestine. Traces only are found in the blood. No deposit forms in the brain, the liver, the bile, or the bones; but the muscles contain appreciable quantities of precipitated silica, as does the spleen. By far, however, the largest quantity of silica is precipitated in the urine as silicic acid and silicate of lime. M. Husson explains the precipitation in the muscles as being due to the acid developed during exertion, biphosphate of sodium playing the same part in causing the urinary deposit. The symptoms produced are—turbidity in the urine, difficulty in passing the same, and congestion of the kidneys.

REPORTS OF SOCIETIES.

ACADEMY OF SCIENCES.

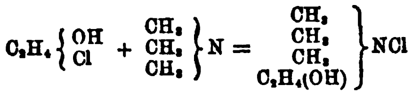
December 16, 1867.

(From our own Correspondent)

Capillary Action—Solar Spots—Synthesis of nevrine—Action of hypochlorous acid on essence of turpentine and camphor —Volumetric estimation of nitrogen—Gallic fermentation—Amalgamation of voltaic piles.

At the meeting held on the 16th December, besides the memoirs, of which abstracts are given in this letter, there was a note relating to a particular effect of capillary action, from M. Deflnis; and from M. Kirchhoff a communication on solar spots. M. Wurtz presented a memoir entitled, *Synthesis of Nevrine*,[*] this will doubtless possess great interest for scientific chemists. M. Adolph Wurtz has in fact succeeded in the synthesis of one of the proximate principles of the brain. M. Liebrich in 1865 obtained from the brain a crystallisable definite compound containing phosphorus and nitrogen, to which he gave the name of protagon. By acting upon this body with strong baryta water, phosphoglyceric acid, and a powerful base which he named nevrine, were obtained. M. Bayer has recently demonstrated that nevrine is an oxethylenic base, being in fact hydrate of oxethyl-ammonium in which three atoms of hydrogen are replaced by three groups of methyl; it is therefore hydrate of oxethyl-trimethyl-ammonium.

This fact led M. Wurtz to suppose that the synthesis of nevrine might be effected by treating hydrate of oxethyl-ammonium (formed by acting upon ammonia with oxide of ethylen) with iodide of methyl. This reaction was only partially successful, since it yielded only small quantities of the base in a state of purity. M. Wurtz succeeded, however, in performing a beautiful synthesis by another process, which he has indicated for the preparation of oxethylenic bases—the treatment of monochlorhydride of glycol by ammonia. The chloride of the base nervine, *i.e.*, the chloride of oxethyl-trimethyl-ammonium, is formed by the direct addition of the elements of mono-chlorhydride of glycol and trimethylamine.



$$C_2H_4\left\{\begin{array}{l}OH\\Cl\end{array}\right. + \left.\begin{array}{l}CH_3\\CH_3\\CH_3\end{array}\right\}N = \left.\begin{array}{c}CH_3\\CH_3\\CH_3\\C_2H_4(OH)\end{array}\right\}NCl$$

5 grammes of trimethylamine are heated in a sealed tube, by a water bath, with 10 grammes of chlorhydride of glycol. At the end of 24 hours the tube is allowed to cool, when beautiful prismatic crystals, perfectly colourless, make their appearance. The crystals dissolve easily in boiling absolute alcohol, and they are partially deposited on cooling from concentrated solutions. Ether precipitates this solution, but if the liquid contains a trace of water, a heavy thick liquid precipitates instead. The crystals are chloride of oxethyl-trimethyl-ammonium, which is a very deliquescent compound. When to a solution of this chloride a moderately concentrated solution of chloride of gold is added, a precipitate of a pure yellow colour is formed,—the double chloride. This precipitate has been shown by M. Bayer to be characteristic of nevrine. It is soluble in boiling water, the solution depositing little yellow needles. M. Wurtz has compared his aurochloride prepared from the artificial nevrine with the compound obtained from the brain substance. The crystals of the two salts under the microscope exhibit rhomboidal plates; they are identical save in the size of the crystals.

A solution of chloride of platinum added to a concentrated solution of chloride of oxethyl-trimethyl-ammonium, causes no precipitate, and no crystals are deposited upon concentration to a syrupy consistence, but addition of alcohol causes a precipitate which by analysis gives 31·8 per cent. of platinum. The formula,

$$(CH_3)_3,(C_2H_4OH)N,Cl + PtCl_2$$

contains 31·8 per cent. Pt.

Chloride of oxethyl-trimethyl-ammonium by the action of moist oxide of silver, is decomposed, hydrato of oxethyl-trimethyl-ammonium being set free. By evaporating the solution, a syrupy liquid is obtained, which upon heating evolves a strong odour of ammonia.

M. Wurtz thinks that the mode of formation, and the analyses he has made, leave no doubt as to the compound being really nevrine.

The eminent author of the preceding paper presented a note, by M. C. G. Wheeler, on the action of aqueous hypochlorous acid on the essence of turpentine and on camphor.

Essence of turpentine, by the action of aqueous hypochlorous acid, is converted into a yellow viscous liquid, probably a mixture of the bi- and trichlorinated compounds; at the same time another product is formed which is retained by the water. This can be completely separated by agitating the aqueous solution with ether, in which it is very soluble, and separating the etherial solution and distilling. The residue is a yellow syrupy substance, neutral, very soluble in ether and alcohol, and slightly soluble in water. Analysis shows this compound to be $C_{10}H_{16}Cl_2O_2$. This chlorhydride cannot be distilled without decomposition, hydrochloric acid is lost in the operation. Nitric acid oxidises it, producing a resinous substance. The whole of the chlorine is removed with great difficulty, the author failed when acting upon it with sodium during several hours. He obtained in this way an acid which appeared to have the composition $C_{10}H_{16}O_3$, but the quantity was too small to admit of a decisive examination.

Camphor added little by little to hypochlorous acid is lique-

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[*] M. Wurtz proposes the word nevrine as the correct translation of the German neurine.

fied and falls to the bottom of the vessel. After a time, especially if agitated, it forms a solid mass presenting the appearance of camphor itself. By two or three crystallisations from alcohol, this product may be obtained pure. It is monochlorinated camphor $C_{10}H_{15}ClO$, and is formed according to the following quotation:—

$$C_{10}H_{16}O + ClHO = C_{10}H_{15}ClO + H_2O.$$

Mono-chlorinated camphor is a white body, indistinctly crystalline, soluble in ether and in alcohol, and nearly insoluble in water; it crystallises much better from alcohol containing a little water, than from absolute alcohol.

It melts at 95°, and is decomposed at a temperature approaching 200°, emitting vapours of hydrochloric acid.

By treating mono-chlorinated camphor with a solution of alcoholic potash at a temperature approaching 80° during six or eight hours, the author obtained products containing no chlorine; one of them he has been able to separate with certainty, its composition is $C_{10}H_{16}O_2$. It is isomorphous with the camphinic acid of M. Berthelot. The author gives to this compound the name Oxycamphor; it crystallises in white needles, soluble in alcohol, and insoluble in water; it fuses at 137°, and sublimes without decomposition, yielding fine crystals. The odour resembles that of camphor.

M. Prat, at a meeting of the Academy on the 25th, addressed a memoir on a general method for the volumetric estimation of nitrogen in its various combinations, and on a new process for the preparation of this gas in a state of purity in laboratories. M. Van Tieghem sent a communication upon the Gallic fermentation. Your correspondent promises some account of these in a future letter. There was also a note on the amalgamation of voltaic piles by M. Demance. If M. Demance says his process is new, one might justly add, query. The process is simply the placing of metallic mercury in the cell, when, by the galvanic current, the zinc becomes amalgamated. The explanation of the manner in which this is effected, as given by him, is certainly not without interest. He has found that there is no previous conversion of the mercury into a salt, that in fact the action is nothing else than a transference of metallic mercury. Furthermore, the amalgamation only takes place under the influence of the current.

DECEMBER 30, 1867.

The Mineral Woodwardite—Electrolysis of Tartaric Acid—Passage of Electric Currents Through Incandescent Gases—Re-establishment of the Voltaic Arc.

THE memoirs relating to chemistry and physics brought before the Academy of Sciences on the 30th of December, were the following:—"On the Woodwardite of Cornwall," by M. Pisani; "Electrolysis of Tartaric Acid," by M. Bourgoin; "On the Passage of Electric Currents through Incandescent Gases," by M. Becquerel; and "The Spontaneous re-Establishment of the Voltaic Arc after an Extinction of Short Duration," by M. Le Roux. M. Pisani commenced by referring to the description given by Mr. Church of this mineral, and compared an analysis by this chemist with his own; the numbers only differed in one constituent, viz., the alumina, which in his specimen was less and which contained silica; this latter, however, was in sufficient quantity to constitute a silicate with the alumina.

He also gave an analysis of a Cornish mineral resembling Woodwardite. These are the percentage results:—Oxide of copper, 17·4; sulphuric acid, 4·7; alumina, 33·8; silica, 6·7; water, 38·7. The greatest difference, as shown by the analysis between this mineral and Woodwardite, is in the alumina. M. Pisani observed that the mineral might be considered as a mixture of Langite with a very basic silicate of alumina analogous to Scarbroïte or to Schrœtterite, or as a mixture of Langite with a hydrate of alumina and mixed with a silicate of the allophane species. He does not imagine Woodwardite to be a new species of mineral, it may be considered as composed of a mixture similar to the new mineral just described.

M. Bourgoin's memoir is a continuation of his research on the electrolysis of organic acids. He has studied the action of the current upon neutral tartrate, on a mixture of tartrate and alkali, lastly, on free tartaric acid. To examine the fundamental action of the current on tartaric acid, a concentrated solution of the neutral tartrate of potash is conveniently operated upon. As soon as the current passes, the solution becomes alkaline at the negative pole; only a moderate disengagement of gas is produced at the two poles. The principal result is the formation of a white precipitate, which is slowly but continuously deposited from the positive electrode. Analysis shows this substance to be wholly cream of tartar. The solution at the positive pole remains neutral during the experiment. The gas evolved at the positive pole was composed of carbonic acid, oxygen, carbonic oxide, and nitrogen. Nearly the whole of the loss takes place at the positive pole. M. Bourgoin gives the following equation as expressing the fundamental action—

$$C_8H_4K_2O_{12} = (C_8H_4O_{10} + O_2) + K_2;$$

this secondary reaction follows—

$$C_8H_4O_{10} + H_2O_2 = C_8H_6O_{12}.$$

The tartaric acid thus regenerated at the positive pole forms with the neutral tartrate, cream of tartar; there is, however, some tartaric acid destroyed by oxidation. The action of the current on a mixture of neutral tartrate and alkali produces quite different results to those obtained with neutral tartrate only, notwithstanding that the fundamental action is the same. At the positive pole a mixture of carbonic acid, carbonic oxide, oxygen and hydride of ethylen is evolved. M. Berthelot discovered acetylen also in the sample of the gas sent him by M. Bourgoin. The decomposition of free tartaric acid yielded the same products as the neutral tartrate, though in different proportions. The carbonic acid is the dominant product from the first: the carbonic oxide diminishes as the experiment proceeds; the same is the case with the oxygen and nitrogen, though to a less extent. Acetic acid is formed at the positive pole. After the fifth day the experiment had been in progress, the solution in the neighbourhood of the positive pole contains a large quantity of acetic acid, which was isolated as acetate of baryta. The following equations explain the action of the current upon free tartaric acid: the fundamental reaction taken place is $C_8H_6O_{12} = (C_8H_4O_{10} + O_2) + H_2$, followed by the secondary reaction—

$$C_8H_4O_{10} + O_2 = 2C_2O_4 + C_4H_4O_4.$$

M. Becquerel's paper referred to one recently contributed by M. Bouchotte, who has observed that by the introduction of a voltameter of acidulated water in the circuit developed by a magneto-electric apparatus, producing two series of alternating currents, these may be made to give place to one series, or to currents of only one kind. M. Becquerel devoted himself chiefly to the explanation of the phenomenon.

M. Le Roux finds that results in regard to the establishment of the voltaic arc, similar to those given by induction currents, may be obtained with the ordinary currents from voltaic piles. With a nitric acid battery of 50 elements, the current may be interrupted for a time which may extend to about $\frac{1}{15}$th of a second, and then leap from one carbon to the other, although they be three millimètres apart.

JANUARY 6, 1868.

On the molecular theory of bodies.—Employment of the Electric light.—Estimation of small quantities of peroxide of hydrogen.—Action of chloride of cyanogen on zinc ethyl.—Sitting of Jan. 13th.—Electro-capillary actions.—Blue colouring matter of certain dead woods.—Sitting of Jan. 20th.—Titles of papers.

M. GULDEBERG sent a very mathematical paper on the molecular theory of bodies. M. Becquerel presented a note

[English Edition, Vol. XVII., No. 422, page 9; No. 424, pages 33, 34: No. 426, page 57.]

by M. Le Roux on experiments relating to the employment of the electric light. M. Houzeau made known a method of estimating small quantities of peroxide of hydrogen.

In the presence of an acid peroxide of hydrogen decomposes either in the cold, or when heated, a solution of neutral iodide of potassium; iodine is set at liberty, and potash formed, which combines with the acid according to the equation—

$$HO_2 + KI + \text{acid} = HO + KO, \text{acid} + I.$$

As a consequence of this it is evident that by simply estimating the potash formed, the amount of peroxide of hydrogen is arrived at. The process is conducted as follows. Titrated acid is first added to the neutral oxygenated solution, and then a slight excess, usually a few drops, of neutral iodide of potassium. The mixture is heated to aid the reaction, and the iodine completely expelled by ebullition. Finally a titration is performed with an alkaline solution. Thus the amount of residual acid is estimated. The solution of iodide of potassium is made by dissolving three grammes of the salt in 100 grammes of water. When the sulphuric acid and neutral iodide of potassium are sufficiently diluted, they do not react upon each other, either in the cold or upon heating. Contrary to what has been observed with regard to ozone, oxygenated water does not seem to react with iodide of potassium when the solutions are neutral. But the vapour of peroxide of hydrogen blues, notwithstanding at the same time that ozone does the iodide and starch-paper. Neutral iodide of potassium can equally serve to detect oxygenated water when this has been previously acidulated. In most cases the yellow or pinkish colour given to the solution may be considered characteristic of peroxide of hydrogen; but the sensitiveness of the reaction is augmented by the employment of chloroform, which is rendered violet or rose-coloured by traces of iodine invisible in water. Nitrites, hypochlorites, and other analogous salts react on iodide of potassium in the same manner as oxygenated water. This source of error may be removed by operating as follows:—3 or 4 c.c. of liquor is rendered acid, if neutral or alkaline, by a sufficient quantity of very dilute sulphuric acid. The addition of a few drops of iodide of potassium is the next step. A yellow or red colouration produced indicates the presence of oxygenated water, or nitrites and analogous salts. The experiment is then repeated after previously boiling the acidulated solution for a few minutes to expel nitrous acid, etc. If upon the addition of the iodide it still produces a colouration, this indicates the presence of peroxide of hydrogen. If in the cold there is no colouration, the solution is heated; if the reaction takes place, oxygenated water is present. If no colouration is produced under this treatment, a drop of chloroform is added, and the mixture agitated for a few minutes about 40° C.; a rose tint indicates the presence of oxygenated water; if no tint is produced, it may be concluded that the solution does not contain oxygenated water, or that quantity is too minute to be detected. A solution may be concentrated either in vacuo or over quick-lime. Concentration may also be effected by heat. A very dilute solution of oxygenated water containing sulphuric acid may be boiled for some minutes without suffering an appreciable decomposition.

A paper entitled "Researches on the action of chloride of cyanogen on zinc ethyl," by M. Gal, was presented by M. Fremy. M. Gal recounted the experiments he had made. He finds by acting upon zinc ethyl by gaseous chloride of cyanogen, a liquid is obtained, boiling at 98°, identical with hydrocyanic ether; the reaction is the following—

$$C_2NCl + C_4H_5Zn = C_4H_5C_2N + ZnCl.$$

The author undertook the research in order to throw light upon the constitution of hydrocyanic acid, and to determine which of the two isomeric ethers of this acid should be considered as its homologue: he has not been able to decide. One of the ethers boils at about 82°, the other at 98° C. On the 13th of January, M. Becquerel made known the results of further experiments upon electro-capillary actions; this is the fourth memoir upon the subject. In decomposing mixed metallic solutions, he has observed that the metals are deposited separately: from a solution containing nitrate of silver and nitrate of copper, the metallic silver is alone deposited. M. Thénard presented a paper, by M. Romier, on the blue colouring matter of certain dead woods. M. Romier thought of the matter being applicable to dyeing purposes, and specimens of silk owing their tints to it were examined by the Academy. At the meeting on the 20th of January, Father Secchi contributed a second note on stellar spectra. There was also a note on the colouration effects which are produced when the sparks from an induction coil pass between the surface of a liquid and a platinum pole, from M. Becquerel. M. Dupré presented a memoir on molecular attractions and chemical operations. M. Miergnes addressed a communication describing a new pile composed of zinc and carbon. Having already covered the space accorded to him in these columns, your correspondent must content himself at present with simply announcing these interesting papers.

CHEMICAL SOCIETY.

Thursday, January 16.

Dr. Warren de la Rue, F.R.S., &c., *President, in the Chair.*

The minutes of the previous meeting were read and confirmed. Messrs. G. W. Child, Edward Chapman, W. G. Mason, P. Griess, and Captain Alexander Walker were duly elected.

Mr. Martin Murphy, of the College of Chemistry, Liverpool, was proposed for election, and the certificate read for the first time. The names of the candidates read for the second time were—Herbert M'Leod, Thomas Charlesworth, Robert Schenk, and John Wallace Hozier.

A paper on the "*Isomeric Forms of Valeric Acid*," by Mr. Alexander Pedler, was read by the Secretary.

The author separated the two varieties of amylic alcohol known as active and inactive, by conversion into baric sulphamylates, and fractionally crystallising. The amylic alcohol was then separated from these salts. By oxidation, valeric acid was obtained from the two varieties. The valeric acid yielded by the inactive variety (*i.e.*, that resulting from the further treatment of the less soluble sulphamylate) boiled at 175° C., and had no action on a polarised ray. The valeric acid yielded by the rotating alcohol (separated from the soluble sulphamylate) boiled at about 170° C. It rotated the ray 43° to the right.

Dr. Debus advanced some hypothetical views concerning thioformic acid—the acid which is obtained in combination with lead, when formiate of lead is treated with sulphuretted hydrogen. It contains the elements carbon, hydrogen, sulphur, and oxygen. Dr. Debus referred to the inability of Professor Limpricht and Mr. Herst, who have analysed this substance, to obtain concordant results. He showed that a relation might be traced in the analytical results between the carbon and the hydrogen, while in the case of the sulphur and oxygen there was an utter absence of this. But if the numbers of the sulphur and oxygen were added together they then gave figures bearing constant relation to the carbon and hydrogen. Dr. Debus gave the following formulæ as the expression of the analytical results obtained for two specimens:—

(1.) $CH_2O+2(CH_2S)$.
(2.) $CH_2O+3(CH_2S)$.

Dr. Frankland's lecture "*On Water Analysis*" followed.

Dr. Frankland said—"Having for some time past been engaged, in conjunction with my late pupil, Mr. Armstrong, in endeavours to place some of the determinations of water analysis upon a sounder basis, I proposed to give the results of our experiments to the Society in the usual form of a paper, when our indefatigable senior Secretary suggested that the paper should be elevated to the rank of a lecture. To this suggestion I was at first much opposed, considering that the Society had already, and even quite recently, received several important communications on this branch of chemical analysis. At last, however, I yielded to Dr. Odling's persuasion, but, in doing so, distinctly cast upon his shoulders the responsibility of summoning the Fellows to hear, under the title of a lecture on water analysis, what I fear will merely prove a dry communication on a few points only connected with this large subject; for, in the first place, I have no intention of discussing the whole subject, but only that portion of it which deals with those determinations comprehended under the term 'partial analysis of a water;' and secondly, even in reference to this corner of the subject, I have, except in one department of it, little that is novel to bring forward. So many difficulties surrounded the subject at the time this investigation was undertaken, that it was perhaps considered the least satisfactory of analytical processes. The difficulty chiefly experienced by water analysts was the determination of the organic matters and the mineral products derived from these, viz., nitrous and nitric acids and ammonia." The lecturer mentioned the names of Hofmann and Blythe, Weltzien, Dr. Miller, and Dr. Angus Smith, and the ways in which they had severally contributed to the improvements of the analytical processes regarding water. The determinations usually made in the partial analysis of a water are: *a.* The total solid constituents. *b.* The organic and other volatile matters. *c.* The oxygen required to oxidise organic matter. *d.* The nitrous and nitric acids. *e.* The ammonia.

These processes were considered *seriatim.*

(*a.*) In the method usually adopted for the determination of the total solid constituents—the evaporation of the water with addition of sodic carbonate and drying at 120° to 130° C.—there are two prominent sources of error; firstly, the salts of ammonia are converted into carbonate, which volatilises during the operation; secondly, urea when present is decomposed, and some of the products are volatilised. In an experiment made to test this point 44 per cent. of urea was lost. These defects are lessened by not adding the alkaline carbonate, and by drying at 100° instead of at 120°—130°. A residue dried at 100° sometimes retains the elements of water, but these are chemically combined, and the amount obtained can therefore be fairly considered as representing only the solid constituents. With the exception of water containing much calcic and magnesic chlorides and sulphates, the difference made by drying at the one temperature or the other is not great. A water analysed (Thames water) gave as the total amount of solid matter in 100,000 parts, 27·02 parts when dried at 100°C.; and 26·54 parts at 120°—130°C.

Dr. Frankland did not, however, consider the information afforded by this determination as great. In estimating the loss upon ignition, suffered by this residue, it must be previously heated to the higher temperature.

(*b.*) In the determination of the volatile matter by the loss upon ignition, the magnesic and calcic carbonates are causticised, and have then to be recarbonated. In this operation all the organic matter cannot be expelled,—notably the case with water containing urea. Experiments were made with water containing known weights of urea and sodic carbonate. In three experiments only 14·6, 28·2, and 42·1 per cent. respectively became expelled; 85·4, 71·8, and 57·9 per cent. remaining in the residue in these cases. Dr. Frankland suggested the possibility of some of the elements of the urea being fixed in the condition of cyanate and cyanurate. A remarkable error is introduced in the case of some waters in recarbonating the alkaline earths, even with a pure solution of carbonic anhydride, the apparent amount of earthy carbonates being greatly in excess of the real amount. In such a case of course the process cannot be used.

(*c.*) Estimating the amount of oxygen required to oxidise organic matter. Potassic permanganate has been commonly used in making this determination. A close examination of the process, however, has led to its indications being found unreliable. In experiments made upon nine kinds of organic matter, only one, oxalic acid, was completely oxidised in six hours. In the case of urea, hippuric acid, and creatin, the oxygen, abstracted from the permanganate, only represented $\frac{1}{80}$th of that required by theory.

(*d.*) Estimation of the nitrous and nitric acids. In this determination, Pew's process has been much used. It turns upon the conversion of stannous chloride into stannic chloride in the presence of nitric acid. Messrs. Chapman and Schenk have pointed out that this change is effected by many organic substances.

The lecturer recorded experiments upon this subject, which showed that the indications obtained in treating starch and sugar by this process were incorrect. 1 grm. starch digested for 20 minutes in a sealed tube with 3 c.c. of a solution of tannous chloride produced an oxidation equivalent to ·00375 grm. nitric anhydride; ·1 grm. sugar at 150° C. produced an oxidation equivalent to ·007 grm. nitric anhydride.

(*e.*) Estimation of the ammonia. This is usually effected by distilling with baryta water or sodic carbonate, and determining the ammonia either by neutralisation or by Nessler's test. It is liable to give inaccurate results in the case of waters recently contaminated with sewage, owing to the gradual decomposition of urea. Dr. Frankland, in describing the processes and modifications he proposed to substitute, divided the water analysis into four, viz., the following determinations:

1. The total solid constituents.
2. The organic carbon and nitrogen.
3. The nitrogen in the form of nitrates and nitrites.
4. The ammonia.

(1.) *Estimation of the total solid constituents.*—For this purpose ½ litre of water is evaporated as rapidly as possible, and the residue dried at 100° C.

(2.) *Estimation of the organic carbon and nitrogen.*—The lecturer had no process to offer for the direct determination of the organic matter, but he was able to estimate the carbon and nitrogen, which were its most important elements, with accuracy. It is necessary in the first place to expel the carbonic anhydride. Sulphuric acid has been found to effect this easily, and for many reasons it has been found the most convenient acid. The solution is boiled for a couple of minutes with a small quantity of sulphuric acid and then evaporated; for this purpose hemispherical glass dishes have been found far more convenient than platinum. The evaporation is conducted in vacuo, and the residue dried at a steam heat. The heat is applied at the *top* of the bell, by means of a current of hot air; applied in this way the water never boils. Five samples of water could be evaporated at the same time in the apparatus shown to the Society.

The residue is mixed with plumbic chromate and transferred to a combustion tube, the dish being rinsed with the chromate; a layer of pure cubric oxide is also added. The tube is sealed at one end and drawn out at the other to about the same size as the tube of the Sprengel's pump which it has to join. The anterior portion of the tube (the position of the layer of pure cupric oxide) is heated, and the tube then exhausted for 5 or 10 minutes. The combustion is now made, and the tube again exhausted, and the resulting gases collected over mercury.

A gaseous mixture is obtained, containing free oxygen. After absorbing the oxygen by pyrogallic acid, the volume of the gaseous mixture is accurately measured. The whole of the carbon is obtained in the form of carbonic acid, the nitrogen partly as such, with nitric acid and nitric oxide. The amount of nitrogen found is made up of the nitrogen of the ammonia and the organic nitrogen; the former must therefore be subtracted.

Cupric oxide made from nitrate is not admissible. It is best obtained by oxidising sheets of copper. It may be obtained tolerably pure from blue-vitriol makers. In experiments made upon solutions of known weights of sugar treated by the whole process, these results were obtained:—

	Found.	Calculated.
Carbon	·01306	·01460
	·00440	·00480
	·00530	·00510

A solution of a known weight of sugar and chloride of ammonium gave—

	Found.	Calculated.
Carbon	·00434	·00484
Nitrogen	·00254	·00246

Dr. Frankland had examined Messrs. Wanklyn, Chapman, and Smith's method of estimating the albuminoid nitrogen in water. Numerous experiments were made side by side with the combustion process now brought forward. These are some of the results obtained in samples of water artificially made, representing water of average quality:—

Permanganate Method.	Combustion.
·006	·010
·002	·010
·016	·068
·016	·042
·022	·076

Where the quantity was small they agreed better.

·001	·001
·004	·009
·003	·004
·012	·012

The permanganate method in several cases gave nitrogen where the combustion process showed none.

(3.) Estimating the nitrogen in the form of nitrates and nitrites, Dr. Frankland has found a process, devised many years ago by Mr. Walter Crum, to give very good results.

A concentrated solution of the nitrate or nitrite is mixed with rather more than an equal volume of sulphuric acid and agitated with mercury in as finely divided a state as possible. It is convenient in this operation to use the residue obtained in the determination of the total solid constituents. Chlorides must not be present. To remove chlorine, argentic sulphate is added to the residue dissolved in 15 or 20 c.c. of water, the solution filtered, and evaporated to a small bulk. It is then transferred to a vessel standing over mercury. The vessel may be described as a narrow eprouvette drawn out at the top into a narrow tube with a stopcock, carrying above a cup-shaped piece. The last portions of the fluid are washed in with the acid itself. The tube has been filled up to the tap completely with mercury, and care is necessary in allowing the descent of the fluid from the cup to the lower vessel to allow no air to enter the latter. The tap being carefully closed, the thumb is slipped under the end of the tube, which is then withdrawn and shaken. In this operation a short column of mercury must always remain between the thumb and the solutions. A strong pressure is produced, and the tube is occasionally returned to the trough, and the egress of some of the mercury permitted. The pressure is due to the formation of gaseous oxides of nitrogen. The nitrogen is determined in the resulting gas. The reduction of the nitrates and nitrites by this means was shown to the Society, the process described being performed experimentally, and a considerable quantity of gas was obtained.

This process has been tried with known quantities of nitre, also with uric and hippuric acids, and found to give satisfactory results.

(4.) Determination of the ammonia. Dr. Frankland considered it advisable to decolorise the water before using Nessler's colour test, using for this purpose calcic chloride, sodic carbonate, and a few drops of potassic hydrate. The distillate from this gave accurate indications.

The PRESIDENT, in returning the customary vote of thanks, took occasion to inquire whether the reduction of the nitrates was carried to nitrogen.

The subject being evidently a fertile one for discussion, the speakers were limited to a few minutes each.

Mr. WANKLYN wished to know whether the comparative experiments with the process devised by himself and colleagues were made with natural or artificial waters. He maintained that their process gave constant results with from 1 to 6 parts of albumen in 100,000 of water.

Professor ABEL had thought the process alluded to by the last speaker might have been serviceable to him, and had instituted experiments to check their results. He had been totally unable to obtain concordant results.

Mr. DUGALD CAMPBELL'S experience with the process was similar to Professor Abel's.

Mr. CHAPMAN attributed the different results obtained by Dr. Frankland by the combustion process and their process, to sources of error in the former.

Mr. THORPE, calculating the nitrogen as albumen, obtained results agreeing with other determinations, and found the process valuable as a method of controlling his results.

Dr. VŒLCKER remarked that M. Nessler had told him of a simple method of separating the ammonia when present in moderately large quantity. The Nessler test, shaken in a bottle with the water, gave a precipitate which contained all the ammonia. By separating this by deposition, and treating it with sulphide of potassium, and distilling, all the ammonia will be separated, and may be collected in a solution of standard acid.

Mr. SMEE and Mr. HAWKSLEY, the engineer, also took part in the discussion.

The lecturer replied to the many remarks that had been made. The question put by the President was one of great interest, but he was unable to say definitely whether the reduction was carried to nitrogen. He should act upon the point suggested by Dr. Vœlcker. At a late hour the Society adjourned. Mr. Wanklyn wished to continue the discussion, and he therefore possesses the right to speak at the commencement of the next meeting.

MANCHESTER LITERARY AND PHILOSOPHICAL SOCIETY.

Ordinary Meeting, December 24th, 1867.

E. W. BINNEY, F.R.S., F.G.S., *Vice-President, in the Chair.*

"*On the Examination of Water for Organic Matters,*" by Dr. R. ANGUS SMITH, F.R.S.

THE author repeated his opinion that the mere expression organic matter had no such meaning as would allow chemists to measure the impurity of water by its amount. He went more fully into the division of the organic matter into various portions, some acting as unwholesome agents, others being entirely innocent. He said he was glad to find that other chemists were also attending to the quality as well as the quantity of the organic matter, and he insisted also on the condition of the matter being observed. He discussed the methods of Professors Frankland and Wanklyn, considered, however, that they did not supersede his own methods, which made a greater number of subdivisions. He explained the mode in which the organic matter is entirely removed from water, leaving frequently none of its elements behind, unless we include amongst them the inorganic bodies with which they were combined. The body which remains is chiefly common salt, which cannot be removed, and by which more than any other substance animal matter is to be detected in water under certain precautions. He also showed the importance of finding the amount of atmospheric oxygen in water, and its meaning; but as the paper was not concluded the notice is here left incomplete.

M. R. Radau has just published a most interesting work on Acoustics (Paris, Hachette) with 114 wood-cuts; it contains the new experiments by M. Regnault on sound and kœnigon vowels.

[English Edition, Vol. XVII., No. 425, pages 46, 47; No. 423, page 22; Vol. XVI., No. 414, page 242.]

CHEMICAL NOTICES FROM FOREIGN SOURCES.

Cement.—Sorel describes a new cement which he prepares by mixing magnesic oxide with a more or less concentrated solution of magnesic chloride. The hardness of the cement increases with the strength of the solution; 20 to 30° Baumé is found most suitable. Its blinding power is greater than that of any other cement, it being capable of producing hard blocks with more than twenty times its weight of sand or other inactive material.—(*Comptes R.* lxv. 102).

Glycogen.—The amyloidical matter found in mollusks is, according to Bizio, glycogen. If the latter, after precipitation with alcohol, is allowed to dry gradually, it coheres together in lumps. Rapid desiccation leaves it as a fine powder, in which condition glycogen has commonly been observed. In contact with white of egg or casein, lactic acid fermentation slowly sets in. Dried at 100° C., its composition is $\Theta_6H_{10}\Theta_5$.—(*Comptes R.* lxv. 175).

Monamines derived from Aldehydes.—H. Schiff. Prolonged action of alcoholic ammonia upon acetic aldehyde, at ordinary temperature, gives rise to the formation of two bases—Θ_8H_9N or Θ_6H_7N (Picolin), distilling at 60°—70° C., soluble in water, and $N_2(\Theta_2H_4)_3$ remaining in the residue after distillation. The latter (which has not been obtained pure) is decomposed by water and acids with formation of another soluble base—$\Theta_6H_{11}N\Theta$. The derivation of this third, a tertiary monamine from aldehydeammonia, is explained by the following equation:—

$$N\left\{\begin{array}{c}\Theta_2H_4,\Theta H\\ H_2\end{array}\right. + 2\Theta_2H_4\Theta = 2H_2\Theta + N\left\{\begin{array}{l}\Theta_2H_4,\Theta H\\ (\Theta_2H_3)_2\end{array}\right.$$

When aldehyde is treated with ammonia at 100°, two other bases are obtained—$\Theta_{10}H_{15}N\Theta$, and $\Theta_8H_{13}N\Theta$, of which the latter has already been noticed by Heintz and by Wislicenus. Hydro-œnanthamide $N\,(\Theta_7H_{12})_3$ is decomposed by boiling water, and the compound

$$N(\Theta_7H_{13})_2(\Theta_7H_{14},\Theta H)$$

formed, which shows no basic properties.

Related to these are valeral-ammonia and Erdmann's trioxyamylidene, to which the formulæ

$$N\left\{\begin{array}{c}\Theta_5H_{10},\Theta H\\ H_2\end{array}\right.$$

and $N(\Theta_5H_{10}, \Theta H)_3$ are given.

The reaction between acrolein and ammonia is somewhat different, inasmuch as first a combination of the two in equivalent proportions takes place, which then acts on an excess of acrolein, thus:—

$$2N\left\{\begin{array}{c}\Theta_3H_4,\Theta H\\ H_2\end{array}\right. + 2\Theta_3H_4\Theta = 2H_2\Theta + N_2\left\{\begin{array}{l}2(\Theta_3H_4\Theta H)'\\ 2(\Theta_3H_4)''\end{array}\right.$$

This base resembles closely those derived from acetic aldehyde. Ammonic sulphhydrate converts acrolein and œnanthol into acrothialdin $\Theta_9H_{13}NS_2$, and œnanthothialdin $\Theta_{21}H_{43}NS_2$.

From the reactions of these bodies the author concludes that the thialdines likewise are tertiary amines, in which three typical hydrogens are replaced by three radicals containing the sulphur, as sulphydril (SH), just as the bases above mentioned contain oxhydril (ΘH).—(*Comptes R.*, lxv. 320.)

Influence of Coloured Light on the Decomposition of Carbonic Anhydride by Plants.—L. Cailletet. The red and yellow rays of light are the most favourable in promoting the decomposition of carbonic anhydride by plants. Light which passed through a solution of iodine in carbonic disulphide prevents decomposition altogether. Under the influence of green light, not only does no decomposition take place, but new quantities of carbonic anhydride are formed. A fresh leaf exposed to sunlight, under a bell-jar of green glass, exhales nearly as much carbonic anhydride as it would in the dark.—(*Comptes R.* lxv. 322.)

Synthesis of Diethylated Toluol.—Lippmann and Longuinine. With a view of arriving at a clearer conception of the constitution of the radical amyl, and of finding a new method for the synthesis of aromatic hydrocarbons, the authors investigated the action of zincic ethide upon chlorobenzol (chloride of oil of bitter almonds). The reaction that takes place is the following—

$$\Theta_6H_5,\Theta HCl_2 + Zn(\Theta_2H_5)_2 = \Theta_6H_5,$$
$$\Theta H(\Theta_2H_5)_2 + ZnCl_2$$

The hydrocarbon $\Theta_{11}H_{16}$ must be considered as toluol in which 2 atoms of hydrogen of methyle are replaced by 2 of ethyl. Its density was found = 5·1107, calculated = 5·1245. Its boiling point when pure, is at about 180° C. or 15° lower than that of Fittig's amylbenzol, which has the same composition. It follows from this that the two are differently constituted, and that the formula of amyl is not correctly represented as—

$$\Theta\left\{\begin{array}{c}(\Theta_2H_5)_2\\ H\end{array}\right.$$

(*Comples R.* lxv. 349.)

Aldehyde and Cyanhydric Acid.—M. Simpson and A. Gautier. Equal mol. of acetic aldehyde, and dry cyanhydric acid unite by direct addition, when exposed for ten to twelve days to a temperature of 20° to 30° C. The body thus formed has the composition $\Theta NH,\Theta_2H_4\Theta$. It is a colourless, oily liquid, boiling at about 183°, but rapidly resolving itself into its constituents at that temperature. It is soluble in water and in alcohol, absorbs ammonia readily, and when heated with it to 100°, is converted into a syrupy mass of basic properties. The action of chlorhydric acid and water upon cyanhydric aldehyde gives rise to the formation of *lactic acid* according to the following equation:—

$$\Theta NH,\Theta_2H_4\Theta + HCl + 2H_2\Theta = NH_4Cl + \Theta_3H_6\Theta_3$$

(*Comptes R.* lxv. 414.)

Conversion of Gallic Acid into Tannin.—T. Löwe finds that gallic acid in aqueous solution is converted into tannic acid by the oxidising influence of argentic nitrate. The oxidation is more complete if a salt of gallic acid is employed.—(*Journ. Pr. Chem.* cii. 111.)

Acetylene.—R. Rieth. The imperfect combustion of coal gas which takes place when the flame of a Bunsen's burner has gone down, so as to burn within the tube, has been found to be a rich source of acetylene. The escaping gases are collected by means of a funnel placed over the burner, and connected with an aspirator. The quantity of the silver compound of acetylene obtained from one burner in twelve hours amounted to 100 grammes.—(*Zeitschr. f. Chem.* N. F. iii. 598).

Oxidation of Potassium and Sodium.—The oxidation of potassium and sodium, when exposed with a clean surface to the air, is accompanied, according to H. Baumhauer, with evolution of light.—(*Journ. Pr. Chem.* cii. 123).

Viridinic Acid.—O. Cech. This acid may be obtained direct from coffee by pulverising the beans, extracting them with ether alcohol, to remove fat, and exposing them in moist condition to the air. After a few days the mass, which has assumed a green colour, is exhausted with acetic acid and alcohol, which takes up the viridinic acid formed.—(*Ann. Chem. Pharm.* cxliii. 366.)

Preparation of Iodhydric Acid.—C. Winkler. Instead of preparing this acid by passing a current of sulphuretted hydrogen through water, containing iodine in suspension, the author proposes the following plan of working. Iodine is dissolved in carbonic disulphide, water placed on the top of this, and the sulphuretted hydrogen passed to the bottom of the vessel into the iodine solution. The dark colour of the latter gradually becomes lighter, while the iodhy-

[English Edition, Vol. XVII., No. 422, page 9; No. 424, page 34; No. 426, page 59.]

dric acid formed is completely absorbed by the water. The sulphur which separates remains dissolved in the carbonic disulphide. – (*Jour. Pr. Chem.* cii. 33.)

Derivatives of Xylol and Dimethylbenzol.—R. Fittig. A careful comparison of methyltoluol, or dimethylbenzol $\Theta_6H_4(\Theta H_3)_2$ (obtained by replacing in toluol one atom of hydrogen by one of methyl) with xylol from coal-tar, has shown that these hydrocarbons are not indentical. Both, however, are converted by oxidation with diluted nitric or chromic acid into the same derivatives, *i. e.*, toluylic and terephtalic acid. Amongst the compounds prepared and examined were nitroamidoxylol, nitroamidomethyltoluol, diamidoxylol, dibromxylol, dibrommethyltoluol, parabromtoluylic acid, nitroparabromtoluylic acid, paradibromtoluylic acid, monobromnitroxylol, dixylyle.—(*Zeitschr. Chem.* N. F. iii. 523.)

Derivatives of Sulphurous Chloride.—Fr. Grauhe. Sulphurous chloride is prepared by passing a current of sulphurous acid into phosphoric perchloride, and subjecting the products of the reaction to fractional distillation. The pure chloride boils between 78° and 79° C. Argentic cyanide converts it into sulphurous cyanide $S_2O_2(C_2N)_2$, which is insoluble in water, soluble in alcohol and ether. From the latter it crystallises in long needles. Zinzic ethide decomposes sulphurous chloride with formation of ethylic sulphide according to the equation—

$$S_2O_2Cl_2+3ZnC_4H_5=S_2\left\{\begin{matrix}C_4H_5\\C_4H_5\end{matrix}\right.+2ZnCl+ZnO+C_4H_5O$$

An experimont in which sulphurous chloride and benzol were made to act upon each other with a view of obtaining phenylsulphurous acid, was unsuccessful. — (*Ann. Chem. Pharm.* cxliii. 263.)

Double-chlorides of Platinum.—K. Birnbaum. Plumbic chloride dissolves readily in a concentrated neutral solution of platinic chloride. On evaporation crystals of plumboplatinic chloride $Pb\ Pt\ Cl_6+4H_2\Theta$ separate. An ammoniacal solution of argentic chloride added to ammoniacal platinic chloride, causes the formation of a yellow crystalline precipitate, which after desiccation over sulphuric acid had the composition—

$$2NH_3+2AgCl+PtCl_4+H_2\Theta.$$

No definite compound could be obtained with mercuric chloride.—(*Zeitschr. f. Chem.* N. F. iii. 520).

Cymol from Camphor.—Longuinin and Lippmann. Equal molecules of camphor and phosphoric perchloride are intimately mixed, and the mixture very slowly distilled from a retort. The distillate is washed with water, dried, and rectified over sodium. It is then quite pure, having its boiling point between 175° and 178° C.—(*Bull. Soc. Chim.* vii. 374.)

Isoxylol, Preliminary notice.—R. Fittig. Mesitylenic acid, the product of the reaction of diluted nitric acid upon mesitylene, is decomposed by being heated with caustic lime according to the equation—

$$\Theta_9H_{10}\Theta_2=\Theta_8H_{10}+\Theta\Theta_2.$$

The new hydrocarbon isoxylol resembles its isomer xylol very closely in many points, but widely differs from it in its behaviour towards oxidising agents. Chromic acid, for instance, converts xylol into terephtalic acid, while isoxylol is oxidised to isophtalic acid, isomeric with the former. This new acid is readily soluble in alcohol, almost insoluble in cold, sparingly soluble in hot water. From the latter it crystallises in long needles, which fuse above 300°C. A dibasic homologue of isophtalic of the composition $\Theta_9H_8\Theta_4$ has been obtained by slow oxidation of mesitylenic acid, besides tribasic trimesinic acid, described on a former occasion.—(*Zeitschr. Chem.* N. F. iii. 526.)

NOTICES OF BOOKS.

First Principles of Modern Chemistry. A Manual of Inorganic Chemistry for Students and for Use in Science Classes. By U. J. Kay Shuttleworth. London: John Churchill & Sons. 1867. (Pp. v. and 214.)

This little book is mainly founded on Dr. Williamson's lectures at University College in the session 1864–5, and on those delivered by Dr. Frankland at the Royal College of Chemistry in the following winter. It was originally intended to supply the want of a strictly elementary manual for the use of science classes—a want, however, which, in our opinion, has had no real existence since the appearance of Professor Roscoe's excellent "Lessons in Elementary Chemistry." The great and rapidly-increasing popularity attained by Dr. Roscoe's book is no less an indication of the reality of the want of a manual of this character, than a measure of the success with which that want has been met. Already the book has been translated and favourably received in Germany, and we understand that Professor Beilstein is about to prepare a Russian edition for the use of his students at St. Petersburg. Without laying claim to any great degree of originality, the author of the book before us has attempted to indicate how the study of the non-metallic elements may be facilitated by the aid of modern theories, and thus the student's early steps rendered less tedious and more suggestive than they commonly are. Dr. Frankland's system of notation is employed throughout the work, together with Dr. Crum Brown's method of graphic formulæ; the author considering that the advantages of the former ought to insure its universal adoption, whilst what is sometimes urged as a fundamental objection against the use of the latter, namely, that students are prone to regard graphic formulæ as physical arrangements of the atoms, he believes not to be warranted by the experience of those who have given the method an impartial trial.

No detailed directions for manipulation are given, the author justly considering that such directions are seldom very intelligible, except when given orally in presence of the objects used. Practical study in a laboratory should invariably accompany a course of reading in chemistry, although the manifest advantages of such method of study have hitherto, in the system of cram so much in vogue, been too frequently lost sight of. In the few instances in which they have been attempted, the detailed descriptions of apparatus are fairly given. The author, however, has erred with other compilers of chemical manuals, when describing the method of determining the composition of water by volume (p. 83) in ascribing the invention of the pear-shaped vessel with its elaborate system of screws, glass stoppers, brass and glass stopcocks, etc., to Cavendish. True the apparatus here referred to is the one selected by the Cavendish Society as their emblem, and appears on the title-pages of its publications, but withal Cavendish never employed such an instrument.

The apparatus, as described by him in his memoir in the *Philosophical Transactions*, consisted simply of a glass globe provided with a stopcock, wires for the passage of the spark, and an arrangement for suspending it to the beam of the balance. The eudiometer figured in the pages of its publications (and in Mr. Shuttleworth's book) represents the instrument as constructed at the period of the formation of the Society, and not as it was actually employed by Cavendish.

Before entering on the more special part of the subject two chapters are devoted to a consideration of such of the principles of physics as may be deemed indispensably necessary to the student. The author believes these chapters to contain nearly all the knowledge of heat required by the University of London for its matriculation examination, and moreover the subject is treated very much in the order laid down in the University Calendar. In the description of the different thermometric scales in use, the commonly received opinion is that Fahrenheit fixed his zero-point at the temperature of a mixture of snow and salt or sal-ammoniac, on the supposition that in such a mixture no heat remained. It is, however, the opinion of at least one well-known Professor of Natural Philosophy (moreover a London University Examiner) that Fahrenheit had other and far more philosophical reasons for thus defining his zero, but what those were it is impossible at this distance of time to determine, since the Dutch physicist left scarcely any papers at his death.

The study of the laws regulating chemical affinity is deferred until the student has gained a preliminary knowledge of the principal facts concerning hydrogen, chlorine, and their compound hydrochloric acid. This method of procedure has unquestionably the merit of simplicity over the more usual plan, and materially facilitates the subsequent consideration of the laws of combining proportions, atomic volumes, etc.

Considerable space is justly afforded to a consideration of the properties of water, and their influence in the economy of nature; of the peculiarities of the several kinds of natural waters, together with the methods for their purification from natural impurities and artificial contaminations. The author very properly insists upon the injurious effect of allowing the gases, which enter through the waste-pipes descending into drains and sewers, to pollute the water in our cisterns. "The arrangement to which this abominable nuisance is due is briefly this; cisterns are filled daily by means of a tap, to which is fitted a ball-cock to arrest the supply of water so soon as the cistern is nearly full; just above the level where the rise of water in the cistern is thus arrested is the opening of a pipe which leads straight into a drain communicating with the sewer; lest the atmosphere of the house should, through this pipe, be placed in direct continuity with that of the drain and sewer, it is usual to make the pipe that enters the drain curved at its lower end, in the shape of the letter J; it is imagined that this bent extremity of the tube will always be kept filled by water flowing down from the waste-pipes, and will act as a tolerably effectual valve so as to exclude the sewer gases. But what is really the fact? On account of the very ingenious regulation of the ball-cock, no water can ever pass into the waste-pipe at all, nor even rise to the level of its orifice; the consequence is that if there ever was any water in the bend of the waste-pipe it cannot have remained there long, nor can it ever be renewed; hence no valve is interposed between the atmosphere of the house and that of the drain, and fires, and chimneys—creating the draught which enters the house at every chink and opening—suck into it the foul gases of the drain and sewers to pollute the air we breathe, and not only so, but also—seeing that it is across the surface of our cisterns that all these gases (many of them highly soluble in water) are dragged—to render the water which we drink, and which we associate with the idea of purity and cleanliness, unclean and deleterious. The only cure practically applicable and fairly reliable for water thus clumsily contaminated, after it has, perhaps, for the sake of a pure supply, been brought from a great distance and filtered with extraordinary care, is filtration through animal charcoal, renewed regularly at short intervals. But since prevention is better than cure, and (so far as I know) the only *sure* way of avoiding these dangers of contamination in towns is to allow water to be drawn straight from the mains, without the intervention of any apparatus of cisterns and waste-pipes, and to have a constant instead of an intermittent system of delivery, this plan, already followed in Manchester and some other towns, and at the public drinking fountains in London, should be universally adopted." (pp. 98—99.)

A large portion of the chapter on the atmosphere is avowedly compiled from Dr. Roscoe's article in "Watts's Dictionary of Chemistry," but the author errs in ascribing the gravimetric process for the determination of the principal constituents of the air to Dumas and Peligot. Dumas's collaborateur in the famous research was, as every tyro could tell, not Peligot but Boussingault. On p. 123 we see repeated the statement of Liebig that the absolute quantity of carbon contained in the atmosphere as carbonic acid amounts to more than the weight of all the plants and of all the strata of mineral and brown coal existing on the earth. A moment's calculation will suffice to show the incorrectness of this statement. The relative volume of carbonic acid contained in the atmosphere is usually stated at four volumes in 10,000 of air, equivalent to ·0612 per cent. by weight. It certainly cannot exceed this amount, indeed the recent researches of Angus Smith on the air of mountains, and of Thorpe on sea air, render it highly probable that this number as an average is somewhat too high. From the barometric pressure of 30 inches of mercury, and the known area of the globe, the weight of the atmosphere is found approximately to be 5,260,000,000,000,000 tons, and hence the weight of carbonic acid would be 3,220,000,000,000 tons. Now Mr. Hull, in his "Coal fields of Great Britain," estimates the amount of available coal in the English and Welsh coal fields at 60,000,000 tons for 1,000 years, and states the American coal fields to be 72 times greater than our own. Assuming it to contain 80 per cent. of carbon, this coal by its combustion would produce 12,845,000,000,000 tons of carbonic acid—a quantity nearly four times as much as that actually existing in the atmosphere at present. Bischof has also demonstrated the falsity of this statement of Liebig, by calculating the probable amount of carbon contained simply in the thick strata, occurring both in the clay slate and in the more recent schistose formations. Assuming the average quantity of carbon contained in these strata to be 0·1 per cent.—a quantity without doubt far short of the actual amount—and assuming also the thickness of the entire sedimentary formation to be eight miles, it may easily be shown from these data that the carbon contained in these strata would amount to nearly seven times as much as the absolute weight of carbon in the atmosphere.

But enough has been written to show the general character of this book. On the whole, it fairly represents such of the leading features of the science as may be gleaned from the study of the non-metallic elements. We cannot, however, anticipate for it a permanent place among established manuals. The book is doubtless adapted to the requirements of students preparing for the matriculation examination of the University of London, and intending to proceed to an Arts degree, although we are slow to believe that this constitutes the main idea of the author, since any book, however ably compiled and arranged, professing merely to facilitate the process of cramming, is unworthy of much respect or toleration.

CORRESPONDENCE.

Crystallisations produced by the Blowpipe.

To the Editor of the Chemical News.

Sir,—With reference to the miscellaneous memorandum, under the above head in your journal of the 27th *Dec., 1867,* (*Amer. Reprint, February,* 1868, *page* 74), in which it is stated that the sudden opacity of beads, of borax, P. salt, or soda, is found by M. G. Rose to be due to *crystallisation of* contained substances, allow me to state that the fact has been long known, and is to be found in the works of Berzelius, Plattner, and other reliable writers on the blowpipe. *Berzelius says* (page 64):—"Titanic acid combines with soda with effervescence, and forms a clear dark green glass. This glass has the property of crystallising exactly at the moment *that it ceases* to be ignited. . . . This property is common to all bodies which crystallise at a very high temperature, as, for instance, phosphate of lead." Again, of apatite, Berzelius *says* (*page* 214):—"It is dissolved in large quantity by the salt of phosphorus to a transparent glass, which, when nearly saturated, becomes opaque on cooling, and acquires a *crystalline appearance*, less distinct, however, than that of phosphate of lead."

Plattner alludes to the same fact in more than *one place.*

I am, &c.,

W. A. Ross.

Woolwich, Dec. 28, 1867.

New Volumetric Assay of Iron.

To the Editor of the Chemical News.

Sir,—The two methods at present known, viz., that by the bichromate of potash, known as Dr. Penny's process, and that by the permanganate of potash, which are both based upon the same principle of the oxidation of a ferrous solution and its consequent conversion into a ferric one—involve the necessity (to the travelling chemist) of carrying about a large quantity of expensive and unstable standard solutions, or the trouble and inconvenience of dissolving fresh portions of the crystallized reagent, whenever required, upon the spot.

A little circumstance which occurred at Cawnpore, in 1862, suggested to me another and apparently more simple method, which I beg to recommend to chemists and assayers, especially those travelling in out-of-the-way countries. You are aware that rooms in India are floored with a substance called "chunam," which is a kind of hydrate of lime. In a room of this kind, without a carpet, I was amusing my children by showing them the beautiful deep red colour which a drop of the solution of sulpho-cyanide of potassium bestows upon one of peroxide of iron, and which I told them (in fun) was "the blood of the theatres." A few drops of the red sulphocyanide of iron happening to fall upon the lime floor, I observed that they were immediately decolourised, and this naturally led me to make an experiment similar to that upon which Parkes' volumetric assay of copper is based.

I dissolved some sulphate of iron, "green vitriol, in distilled water, and added a few drops of nitric acid to peroxidise the solution, to which a single drop of the sulphocyanide solution was then sufficient to impart a deep red colour. This colour I removed effectually by the addition of about half the quantity of common lime-water, leaving a perfectly clear solution. I have not had time or opportunity since to carry out the experiment to a practical result by standardising a solution of lime with one of sulphocyanide of pure iron (piano wire); but I hope shortly to do so, and, in the meantime, would feel much obliged by the opinion of better chemists than myself if there is any difficulty or serious objection in the way of such a process? If not, there can be little doubt that it would form the most simple and economical method of assaying iron ores, as lime-water is procurable in almost any part of the world, and the quantity of sulphocyanide of potassium required is extremely small.—I am, &c.,

W. A. Ross, Captain, R.A.

Woolwich, 28th December, 1867.

Friction in Vacuo.

To the Editor of the Chemical News.

Sir,—In the very interesting lectures by Dr. Tyndall, now appearing in your columns, an experiment is attributed to Sir H. Davy which was made long before his time. I allude to the friction of flint and steel *in vacuo*. We owe this remarkable experiment, not to Sir H. Davy, as stated in the lecture, but to Hauksbee, who communicated it to the Royal Society in 1705, as I have shown in my work on "Phosphorescence," p. 204. In Hauksbee's experiments, as described in the *Philosophical Transactions*, when the receiver was well exhausted of air, then, although a more violent motion was given to the steel than before, yet not the least spark appeared to be struck from it, "but a small continued light was visible on the edge of the flint that was rubbed by the steel." On admitting the air the sparks re-appeared.—I am, &c.,

T. L. Phipson, Ph.D.

The Cedars, Putney, S.W., Jan. 4, 1868.

Is Healthiness dependent on Strata?

To the Editor of the Chemical News.

Sir,—In the report of the meeting of the Local Board of Health of this town the following paragraph appears:—"The surveyor said that Dr. Buchanan, from the office of the Privy Council, waited upon him to make inquiries respecting the nature of the soil at Sheerness. By permission of Messrs. Ward and Brightman he had shown that gentleman the different strata forming the soil of Sheerness. Dr. Buchanan has now stated that after a careful examination he is convinced that in Sheerness there are fewer cases of consumption than in any town in England, and as a whole that Sheerness is one of the most healthy places in the kingdom." If I read correctly, it seems that the healthiness of a place is dependent somewhat on the strata of the locality. Can you, or any of your readers, give me any information or the name of any work in which the subject is treated on? Ague is very prevalent here, and two medical gentlemen inform me that they always endeavour to remove, as soon as possible, all consumptive persons from Sheerness, which perhaps to a certain extent may account for the few cases of consumption mentioned by the authority in question as found in Sheerness.—I am, &c.,

John Bray.

Mile Town, Sheerness, Dec. 21, 1867.

Crystallography and the Blowpipe.

To the Editor of the Chemical News.

Sir,—Below I have the pleasure to send you the translation of a letter I have just received from Professor Richter, head of the University of Freiberg, where he succeeded the late celebrated Plattner. It is indeed gratifying to me to think that my trifling endeavours should engage the attention of such a man.

His want of success in making the vesicles at first, may be explained by the fact of my not having mentioned (as I ought to have done) that, in blowing the vesicles, the bead should not be operated on until it has partially cooled down to a red heat, as at a high temperature the current of air is too strong for the small density of the fluid borax, which it soon bursts.

I take this opportunity of mentioning two facts in addition to those stated in my paper on this subject, published in the Chemical News of the 20th December (*Amer. Repr., Feb.* 1868, *page* 74). One, that the smallest imaginable particle of reduced metal, as for instance copper, may be clearly observed in one of these vesicles by a microscope, whereas in a bead it might be buried in the centre and escape observation. The other, that eight or ten vesicles kept by me for three weeks at Christmas, became first black and then clear after efflorescing, on being re-melted by a blowpipe flame projected from a spirit lamp,—showing the presence of free carbon, or organic matter, which could not have been there when the vesicles were formed. I will, with your permission, continue this subject shortly.—I am, etc.

W. A. Ross.

(Copy.)

Freiberg, 1st January, 1868.

Dear Sir,—If I have not answered your letter of the 9th December before to-day, I trust you will kindly excuse the delay, which is owing to my having been so busy. The observation that various earths and metallic oxides may be dissolved in borax and phosphor salt, and preserved crystallised under certain circumstances, was made and described some years ago by George Emerson,* an American, and published in the *Proceedings* of the American Academy of Arts and Sciences, March, 1865, vol. vi., where drawings of the beads may be found. Also, G. Rose, of Berlin, has lately described the production of crystallized bodies by means of the blowpipe in borax and phosphor salt. The article is found in the monthly report of the *Proceedings* of the Royal Academy of Sciences in Berlin, for March and July, 1867, with drawings. I have verified by my experiments the statements of Messieurs Emerson and Rose, and suspect that

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* This, as stated by me in the Chemical News of Jan. 3 (*American Reprint, Mar.*, 1868, *page* 146), is a misapprehension, the crystals having been formerly pointed out by Berzelius and others.

the interesting discoveries made by you are intimately connected with those phenomena.*

I have also attempted to make borax vesicles from your description, but have not realised a satisfactory result, probably from unskilfulness in manipulation.

It would certainly prove a grateful task to study more closely this relation of bodies to each other, in order to be able to use it at the same time as a means of their recognition. I recommend to you the perusal of the above-mentioned experiments of Messieurs Emerson and Rose, and I shall be glad if you will inform me of the result of your further labours.—With great esteem, I remain, yours, etc.

(Signed) Theodore Richter.

To Captain W. A. Ross, R.A., Woolwich.

Volumetric Determination of Iron.

To the Editor of the Chemical News.

Sir,—The method proposed in the last week's Chemical News, by Captain Ross, for the volumetric assay of iron by the decolorisation of the iron solution colored red by sulphocyanide by means of a standard solution of lime water, appears to be inapplicable, inasmuch as the amount of lime requisite depends, not on the quantity of iron present, but on that of the acid in the solution to be tested. The bleaching effect of the lime is caused by the decomposition of the ferric sulphocyanide, whereby ferric hydrate is precipitated, and calcium sulphocyanides produced; but this effect does not take place until not only the whole of the free acid present is neutralised, but also almost the whole of the iron precipitated as hydrate. In practical analysis a solution of iron without free acid is never obtained; nor could the solution be neutralised by addition of alkali, because the point when the free acid is just saturated cannot be observed either by test-papers (as ferric salts have an acid reaction) or by the commencement of a precipitate, as ferric hydrate is soluble in solutions of ferric salts.

I have frequently had occasion to examine acid liquids containing metals in solution with a view to the determination, firstly, of the free acid, and secondly, of the total acid free and combined (as, for example, in the waste manganese chloride of the bleaching powder works). This second quantity is readily ascertainable by adding a standard alkaline solution to the acid liquid examined until an alkaline reaction is observed; but whenever ferric salts were present, the exact determination of the first quantity was found impossible from the solubility of the ferric hydrate; even Kiefer's very convenient process for estimating free acid in solutions containing heavy metals by means of an ammoniacal solution of a cupric salt always indicated a larger amount of free acid than was really present, from the circumstance that as soon as a turbidity was produced by the precipitation of cupric hydrate from the neutralisation of the ammonia (the terminal reaction), it disappeared on agitation, the cupric hydrate apparently decomposing the ferric salt, and thereby being dissolved, whilst the newly-precipitated ferric hydrate also dissolved in the remaining ferric salt.

The destruction of the red ferric sulphocyanide by alkalies might be used as a terminal reaction in acidimetry; but on trial the sulphocyanide appears to be inferior to the ferrocyanide (Prussian blue) as an indicator; and this latter is not so convenient as litmus or cochineal tincture as usually employed.—I am, etc.,

Charles R. A. Wright, B.Sc.

Chemical Laboratory, St. Mary's Hospital, W., Jan. 13, 1868.

Water Analysis.

To the Editor of the Chemical News.

Sir,—As is mentioned in your report of the proceedings at the last meeting of the Chemical Society (*Am. Reprint, March*, 1868, *p.* 143), I asked Dr. Frankland whether the comparative experiments on our method of water analysis as contrasted with his own method had been carried out on natural or on artificial waters. Your report, however, omits to give his reply.

It was, that *natural*—not artificial waters, have been used. Inasmuch, therefore, as the amount of nitrogenous organic matter present in these natural waters is an unknown quantity, the fact that Dr. Frankland's numbers are not parallel with our own, leaves the question of correctess entirely untouched.

Had artificial waters (*i.e.*, waters into which known quantities of organic matter had been put) been taken, the contrast would have borne a different construction.—I am, &c.,

J. Alfred Wanklyn.

London Institution, January 25, 1868.

To the Editor of the Chemical News.

Sir,—In reference to the report of the last meeting of the Chemical Society, which appeared in the Chemical News of the 24th instant (*Am. Reprint, March*, 1868, *p.* 143), I beg to state that I did not say that in employing Messrs. Wanklyn, Chapman, and Smith's method, I calculated the nitrogen as albumen, or used their process as a means of control.

What I did say was in effect that the process was found to give valuable information as to the character of waters, and that the results were in accordance with their known history.—I am, &c.,

William Thorp, Jun.

The late Lunar Eclipse.

To the Editor of the Chemical News.

Sir,—In a recent number of your valuable journal you inserted a notice of the *Proceedings* of the Manchester Philosophical Society. Among other matters, this contained a paper by Mr. A. Brothers, on the late lunar eclipse. In his paper Mr. Brothers expresses his surprise that I had stated in a letter I sent to the *Astronomical Register* that I saw no colour on the darkened part of the limb of the moon, while he himself distinctly saw colour with a refracting telescope.

In a paper I have read at the Royal Astronomical Society I have furnished an explanation of these apparently contradictory observations.

As this explanation can scarcely fail to be of interest to many of your readers, I should feel greatly obliged by your inserting the paper in your journal.*—I am, &c.,

John Browning.

Upper Holloway, January 1, 1868.

Ozone.

To the Editor of the Chemical News.

Sir,—The following is an account of the development of atmospheric ozone in October, November, and December:—

In October there were large amounts of ozone on the 2nd, aft. of 16th, and morn. of 17th; considerable amounts on 29th and 30th; very little on 5th, 9th, 11th, morn. of 12th, 19th, and morn. of 28th; no ozone on aft. of 1st, aft. of 10th, aft. of 12th, 13th, aft. of 15th, aft of 18th, 20th, 22nd, morn. of 23rd, and aft. of 25th.

In November, large amounts on 16th, 17th, and 18th; considerable quantities on 13th and 14th; very little on aft. of 1st, morn. of 9th, 11th, 15th, 21st, 23rd, 24th, 26th, and 27th. No ozone on aft. 2nd, 3rd, aft. of 7th, 8th, aft. of 9th, 19th, 20th, 22nd, 25th, and 28th.

In December, large quantities on 15th and 17th; considerable amounts on 3rd and 9th; very little on 5th, 6th, 11th, 12th 14th, 23d, 25th, 30th, and 31st. No ozone on 4th, 8th, 10th, 20th, 26th—29th.

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* They appear to have the relation to each other that masses of ice bear to the delicate crystals of frost.

* See page 55.—(*Am. Reprint, March*, 1868, *p.* 113).

During November and December the development of ozone has been very scanty.

During the past year the greatest development of ozone in January occurred on the 5th, 6th, 7th, 20th, 22nd, 23rd, and 29th; in February, on the 5th, 6th, 16th, and 26th; in March, on the 25th, morn. of 27th, and aft. of 30th; in April, on the 4th, aft. of 8th, morn. of 9th, aft. of 10th, and morn. of 11th; in May, on the morn. of 14th, 15th, aft. of 21st and 25th; in June, on 5th, 6th, 7th, aft. of 8th, 24th, aft. of 25th, 26th, aft. of 27th, and aft. of 28th; in July, on aft of 3rd, 14th, 15th, and morn. of 16th; in August, on aft. of 5th, morn. of 6th, morn of 7th, 12th, 13th, 17th, morn. of 18th, and aft. of 20th; in September, on aft. of 2nd, 4th, 5th, 6th, morn. of 7th, aft. of 14th, aft. of 17th and 18th; in October, on 2nd, aft. of 7th, aft. of 16th, morn. of 17th, and 29th; in November, on the 13th, 14th, 16th, 17th, and 18th; in December, on 3rd, 9th, 15th, and 17th.

I should state, that in almost every instance the test-papers were freshly prepared immediately before exposing them to the atmosphere.—I am, etc.,

R. C. C. LIPPINCOTT.

Bournemouth.

Antiseptics.

To the Editor of the CHEMICAL NEWS.

SIR,—I have for some time been a reader of your journal, and frequently find therein articles which interest me as a manufacturer. There is one subject upon which I am anxious to obtain practical information, in which I am directly interested, and which is seldom spoken of in works relating to organic chemistry, at least in any works which it has been my fortune to consult. I allude to the subject of antiseptics. Can you point, for instance, to any rule, by an acknowledged authority, which defines the quantity of any chloride, say of mercury, zinc, etc., which, when in solution, will, with certainty, protect from mould or other change, whether produced by a change of climate, a moist atmosphere, warmth or cold, another given quantity of material which contains a large proportion of nitrogenous matter, such as mucilage, etc.; and is there any other antiseptic more active or certain in its preservative properties than the substance named, which is equally economical, and which can be used advantageously in an extensive business where considerable quantities would be required? If you can afford me practical information on this subject, or direct me where to find it, you will confer a favour, and much oblige,

A CONSTANT READER.

Philadelphia, Penn., U.S., Dec. 28, 1867.

[Carbolic acid or (if the odour be an objection) oil of cloves will produce the desired effect. About one part in 1,000 will, in ordinary cases, be sufficient.—ED. C. N.]

Hydrous, not Hydrated.

To the Editor of the CHEMICAL NEWS.

SIR,—Most writers on modern chemistry regard hydrates as altogether distinct from what they call hydrated substances, yet retain the respective names, to the great risk of confusion; a hydrate is stated to be a body containing hydroxyl (HO), a hydrated substance one containing water (H_2O). Why not let the word "hydrated" follow its parent, and relate to hydrates only? Salts without water are appropriately spoken of as anhydrous, salts with water may surely be termed hydrous. I should, for instance, consider the body having the formula Mg_2HO as a hydrate, that having the formula $MgCO_3, 3H_2O$ as a hydrous carbonate, not a hydrated carbonate.—I am, etc.,

JOHN ATTFIELD.

Detection of Ozone.

To the Editor of the CHEMICAL NEWS.

SIR,—During the past year I have on several occasions exposed moistened silver-leaf to the atmosphere, with a view to determine how far it might be used as a trustworthy test for ascertaining the presence or absence of ozone.

Whenever the ordinary test (potassic iodide and starch) indicated that ozone was present, some part of the silver-leaf was oxidised, and the greater the amount of ozone, the quicker was the oxidation of the silver effected.

The silver-leaf was exposed freely to the atmosphere, and was kept moistened by causing distilled water to pass over it, the water being conducted from an adjacent vessel by two or three threads of common darning cotton.

By observing the time which elapses before the silver-leaf is oxidised, an idea may be formed of the relative amount of ozone present on any given day.—I am, etc.

R. C. C. LIPPINCOTT.

Bournemouth, Jan. 13.

MISCELLANEOUS.

Liebig's Extract of Meat.—The Government have contracted with Liebig's Extract of Meat Company (Limited) for the supply of the Company's extract to the troops of the Abyssinian expedition. The extract is packed in small jars which a soldier can easily carry with him, being enabled thereby to dispense with fresh meat for a number of days, and to cook a palatable soup in fifteen or twenty minutes at any halting place where hot water can be procured. The Government were no doubt guided in this decision by the experience gained in the last German war, it having been acknowledged by many officers and men that they owed to the use of this extract of meat the preservation of excellent health. In many cases, fresh meat distributed to the troops in the morning was spoiled by the effect of heat at the time it was wanted; the extract in all such cases proved an efficient substitute for meat.

Average Composition and Quality of the Metropolitan Waters during the Year 1867.—The following are the Returns of the Metropolitan Association of Medical Officers of Health:—

Names of Water Companies.	Total solid matter per gallon.	Loss by ignition.*	Oxidisable organic matter.†	Hardness.		Organic and other ammonia.
				Before Boiling.	After Boiling.	
Thames Water Companies.	Grains.	Grains.	Grains.	Degs.	Degs.	Grains.
Grand Junction	20·29	1·07	0·700	13·0	4·2	0·003
West Middlesex	19·34	1·05	0·747	12·5	4·1	0·003
Southwark and Vauxhall...	19·47	0·99	0·816	12·9	4·1	0·003
Chelsea.......	20·20	1·19	0·794	12·7	4·1	0·003
Lambeth......	19·92	1·18	0·817	12·9	4·0	0·003
Other Companies.						
Kent	27·32	0·76	0·184	16·6	7·7	0·002
New River....	18·45	0·86	0·403	12·5	4·0	0·002
East London...	20·15	1·06	0·624	12·7	4·4	0·003

The fluctuation in the amounts of the several constituents have not been considerable, but the proportions are always a little above the average during the early months of the year when there is much rain, and they are below the average in the dry summer months. The Kent water is always remarkable for its beautiful blue colour when seen in large volume, on account of the nearly total absence of organic matter, it being derived from deep chalk wells.

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* The loss by ignition represents a variety of volatile matters, as well as organic matter, as ammoniacal salts, moisture, and the volatile constituents of nitrates and nitrites.

† The oxidisable organic matter is determined by a standard solution of permanganate of potash—the available oxygen of which is to the organic matter as 1 is to 8; and the results are controlled by the examination of the colour of the water when seen through a glass tube two feet in length and two inches in diameter.

Quality of the Gas supplied to the City of London.—Dr. Letheby reports that in the course of the quarter which expired on the 30th November last, 669 examinations were made of the illuminating power of the gas supplied to the city; each of the examinations was the mean of ten observations, and they were made in accordance with the instructions of the Act of Parliament. The following are the results:—

Illuminating Power in Standard Sperm Candles:

	Great Central Gas.	Chartered Gas.	City Comp. Gas.
Maximum	16·52	16·36	15·69
Minimum	12·02	12·57	12·00
Average	13·96	14·31	13·75

It thus appears that the illuminating power has been constantly above the requirements of the Act of Parliament, and to the extent of about two candles. In the corresponding quarter of last year, the average illuminating power of the Great Central Gas was 13·89 candles; of the Chartered Gas, 14·19 candles; and of the City Company's Gas, 14·17 candles, all of which numbers are close to the averages of the present quarter. The chemical quality of the gas, as regards the amount of sulphur contained in it, has not been so satisfactory, for, excepting the Chartered Gas, the quantity of sulphur has generally been excessive. This will be seen from the following table:—

Grains of Sulphur per 100 *cubic feet of Gas.*

	Great Central Gas.	Chartered Gas.	City Comp. Gas.
Maximum	34·61	26·30	30·58
Minimum	12·74	11·35	14·19
Average	21·87	11·73	22·97

In fact, of the 61 observations made during the quarter of the City Company's Gas, 42 were found to be in excess of the quantity sanctioned by Parliament. Of the 60 observations of the Great Central Gas, 41 were in excess; and of the 61 of the Chartered Gas, only 21 were in excess. The gas of each of the Companies has been at all times free from sulphuretted hydrogen, and, with the exception of the Chartered Gas, it has also been free from ammonia. The pressure at which the gas has been delivered to the laboratory has been rarely under an inch of water.

The Value of Milk as an Article of Food.—Mr. Horsley, analyst to the county of Gloucester, in a paper on this subject, says that a milk may be of high density and yet give but comparatively little animal matter, such as cream and casein, whilst the amount of lactine retained in solution in the whey may be greater than usual; on the other hand, a sample of milk may be of lower density and yet yield far more animal matter than ordinary, though each may be perfectly genuine; the difference in the relative value of the constituents depending much on the time of year, the mode of keeping and feeding the cow, etc. He found only one degree of difference between a sample purchased at Cheltenham and a sample supplied to the workhouse, but an analysis of the two specimens shows not only a vast difference in the amount of solid matter, but also that very little reliance can be placed in any of the instruments usually employed in determining the value of milk; for the fatty matter of the milk, unlike any other aqueous solution, helps to keep up the instrument, and gives no idea of the actual density of the sample, nor of its value.

The Velocity of Light, according to a calculation recently published by Professor Chase, of Boston, is nearly the same as would be acquired in one year by a falling body under the influence of an accelerating force equivalent to the force of gravitation at the earth's surface, viz., 32 1-6 × 86,400 × 365¼ ÷ 5,280 = 192,254 miles per second.

Nitroglycerine.—While the explosive nature of this compound is still exciting attention, it will be interesting to give the evidence with regard to the cause of an explosion in the United States, resulting in fatal consequences, deposed to by Professor Doremus, one of the most distinguished of American chemists. The circumstances under which the accident took place are briefly these. The Central Railroad Company are making a deep cutting near South Bergen, N. J., and in effecting this they use nitroglycerine. A can containing about sixty pounds of this oil, having partially solidified, was carried by a workman to the blacksmith's shed, where it was placed in water, red-hot bars of iron being plunged into the latter. Suddenly an explosion occurred, killing nine men. At the inquest Professor Doremus said:—"On Dec. 2 I received from the coroner two bottles of nitroglycerine, with a request to report upon its properties; I have subjected it to ultimate chemical analysis, and find it is well-made nitroglycerine; the substance freezes at about 46°; it is made to decompose in a very peculiar way; on moistening paper with it, it burns with rapidity; it does not explode when red-hot copper is placed in it; we tried it with the most intense heat we can produce with a galvanic battery, with two hundred cells holding a gallon and a-half each; some nitroglycerine was placed in a cup and connected with one of the poles of the battery; through a pencil of gas-carbon the other poles of the battery were connected with the glycerine; no explosion ensued; but when the point touched the Britannia vessel the nitroglycerine took fire, a portion burning and the rest scattering about; this is as severe a test as we can submit it to in the way of heat under the pressure of the air; we, therefore, would conclude that nitroglycerine carried about exposed cannot explode, even if you drop a coal of fire into it; if the liquid is confined, or is under pressure, then an explosion will ensue; if paper be moistened with it and put on an anvil and a smart blow given with a hammer, a sharp detonation ensues; if gunpowder or the fulminates of mercury, silver, or gun-cotton be ignited in a vacuum by a galvanic battery, none of them will explode; if any gas be introduced so as to produce a gentle pressure during the decomposition, then a rapid evolution of gases will result; the results of decomposition in a vacuum differ from those under atmospheric pressure or when they are burnt in a pistol, musket, a cannon, or in a mine; where we have little or no pressure it is difficult to get these substances to burn rapidly; nitroglycerine is more difficult to explode than powder; in many respects it resembles gun-cotton, which is made in a similar way. If gun-cotton be immersed in protochloride of iron it turns into common cotton; the same experiment was tried with nitroglycerine by mixing it with protochloride of iron, and it reverted into common glycerine. There are four well-known varieties of gun-cotton made by employing acids of different strengths; they differ in chemical composition and properties, as well as in their explosive qualities; the late Minister of War in Austria, in 1862, stated to me that he had ordered 400 cannon for gun-cotton, and six months after he stated that he had ordered all the cannon to be changed and adapted to powder in consequence of spontaneons combustion; much less is known of nitroglycerine than of gun-cotton, and probably several varieties of this article may be formed, as of gun-cotton; this would explain cases of spontaneous explosion; if the nitroglycerine is not carefully washed to get rid of the acid, a gradual decomposition will ensue, producing gases which, if the vessel be closed, will explode. My opinion is that nitroglycerine should be used in the most careful hands; I do not think I would put it in the hands of a common labourer for blasting purposes; it is less dangerous in a frozen than a liquid state; I think concussion would explode frozen nitroglycerine. . . . . Since leaving I have learned that the can of nitroglycerine, the explosion of which proved so fatal, was full of frozen or solid nitroglycerine. Now, although the cork might have been removed, it is possible that the red-hot irons melted and decomposed the material at the bottom of the can, leaving a quantity of solid nitroglycerine above, which, by preventing the escape of the gases, produced the pressure required for an explosion. A red-hot iron, or the more intense heat of a powerful galvanic battery, will not explode the nitroglycerine unless it is under pressure. This stopper of frozen nitroglycerine might have closed the orifice of the can sufficiently to produce the required pressure." Amongst other evidence was the following:—Otto Burstenbinden said: I am now in the blasting business, and reside in the City of New York; nitroglycerine is composed of nitric acid, sulphuric acid, and glycerine oil; it is a liquid of light yellow color, and about six-tenths heavier than water; in exploding it expands 10.400 times, and powder expands about 800 times; this compound called nitroglycerine will not explode by simple contact with fire; it requires about 360 degrees of heat to make it explode; it will not easily explode by concussion or friction when fluid, but it changes its qualities entirely when frozen; if frozen it will explode very easily by a stroke or slight concussion.—Wm. H. White said: I reside in Syracuse, and for the past two years have turned my attention to the use of nitroglycerine; I know that it is no more explosive when frozen than when in a liquid state; frozen, it is less liable to explode; it is very hard to explode a cartridge when frozen; a very heavy weight coming on nitroglycerine either in a frozen or liquid state would explode; I have seen a man let a can of frozen nitroglycerine fall from his shoulder on a rock without exploding it; I consider nitroglycerine twenty-five times safer than powder for blasting purposes; I have seen a chunk of nitroglycerine as big as my hat cut open with an axe.—The jury brought in the following verdict:—"We find that the deceased came to their deaths on the 25th day of November last by the explosion of a can of nitroglycerine, consequent upon the careless handling of the same by Thomas Burns, one of the deceased, and we censure the contractor, Colonel Schafner, for not being more careful in the selection of a man to use the article of nitroglycerine, and recommend that in future men should be employed in its use who understand its explosive qualities, and we request the Council of the town of Bergen to order that there shall not be any more than 100 pounds of nitroglycerine stored up or kept in the town of Bergen, the same to be stored in a fire-proof vault when not required for use."

Dietetic Salt.—One of the great evils that owes its origin to the scientific enterprise of the present age is that any promising scientific scheme, after being brought into prominent notice, becomes for the time being quite the fashion, and is then entirely forgotten, often, too, from mere caprice. We hope that this fate may still be averted from Dr. Lankester's ingenious scheme of supplying necessary but frequently overlooked articles of diet, by means of his dietetic salt. This compound is a proposed substitute for ordinary table salt, chloride of sodium being a notable constituent; but, in addition to this, which is far from being the sole or even most important inorganic constituent of our food, we have phosphate of lime, chloride of potassium, sulphates of potash and soda, with smaller quantities of magnesium and iron salts. The argument for their use is very strong. Leaving out the large proportion of epidemics, almost all the common diseases are directly traceable by modern physicians to dietetic errors; and those that certainly are due in part to deficiency of inorganic food form by no means a contemptible list. Scurvy is known to arise from a deficiency of the salts of potash. Scrofula and consumption, rickets, and softening of the bones, occur when the phosphates of lime and other bases are deficient. Anæmia, chlorosis, and a variety of nervous disorders, are the result of an absence of iron, and are at once cured by the use of this agent as a remedy. In such cases, the medical man is in the habit of prescribing medicines containing these agents; and there can, therefore, be no doubt that the habitual use of these substances in the food, in the same way as common salt is employed, would be a means of preventing the occurrence of a large number of diseases. The quantities of the saline ingredients employed, in addition to common salt, are so calculated that they shall be supplied in the same proportion by its use, as they exist in the human blood, and are got rid of in the body. Dietetic salt is one of those simple but useful applications of science of which the value is at once perceived; it deserves to hold a prominent place in the rank of articles of food, and it is to be hoped that it will not be lost in the crowd of similar inventions.

New Scientific Club.—We have received information of the establishment of a scientific club in Dublin. This club is not intended as a channel for the promulgation of original matter, but is simply designed to give to the members that early information of advances made by others in science, which in London springs from the unorganised conversation of the learned societies. It is proposed to deal with one group of sciences—the group of physical sciences, including physics, chemistry and the applications of mathematics to physics as in astronomy, mechanics, electricity, etc.; but excluding pure mathematics on the one hand, and physiology, geology, and the other natural sciences on the other. The number of ordinary members is limited to twenty-five, but visitors are to be freely admitted by members' introduction The annual payment for an ordinary member is £1. The scientific men in our provincial towns may take a hint from this Dublin Scientific Club. It will be remembered that a meeting in Glasgow was recently convened to forward the formation of a similar institution there.

Amber.—Recently the local correspondent of a Geelong journal announced the discovery of a supposed amber mine at Rokewood. The *Age* pooh-poohed the matter, and suggested that the substance was simply a description of gum found in lignite deposits. The author of the statement, as to the substance being amber, has replied, and says:—"I, for one, will confess that I never heard of this sort of gum referred to, except close to the surface of the roots of trees, and certainly not at a depth of 70 feet; but for the benefit of the curious I will give you a *verbatim* copy of the written opinion of a professional mineralogist, who resides at Ballarat, and whose ability and experience in such matters is, I am told, well known there and elsewhere. 'The resinous substance left with me for examination is undoubtedly amber, and has not previously, to my knowledge, been found in this colony; making therefore another addition to our colonial minerals. The colour of the said substance is brown, streaked yellowish white, transparent, conchoidal fracture, lustre waxy. Specific gravity 1·1. Acquires resinous electricity by friction; contains empyreumatic oil and succinic acid, and corresponds in all other respects with the brown amber of Europe. (Signed) A. T. Abel.' I have also been shown the substance obtained from the mine by Messrs. M'Keeman, draper, Ballarat, and Blair, miner, Break o' Day, either of which gentlemen I have no doubt would be most happy to show it to any gentleman interested in the development of our mineral resources. This mine, at which men are now employed, is situated at Grassy Gully, about eight miles from Rokewood, in the direction of the Mount Misery Ranges."—*Ballarat Evening Post, July* 4, 1867.

Petroleum for Steamship Boilers in the United States Navy.—After careful and long-continued trials, the Secretary of the United States Navy finally reports against the employment of petroleum as a fuel in steamships. He says—"The act approved April 17, 1866, appropriated five thousand dollars for testing the use of petroleum as a fuel for marine boilers. An elaborate series of experiments has been made at the New York and Boston Navy Yards. The conclusion arrived at is that convenience, comfort, health, and safety are against the use of petroleum in steam vessels, and that the only advantage thus far shown is a not very important reduction in bulk and weight of fuel carried."

Simple Method for the Extraction of Phosphoric Acid from Glass.—From a consideration of the nature of either the basic or acid* materials used in the manufacture of glass, the presence of phosphoric acid in this substance would naturally be suspected, but as far as I know, this acid does not enter for certainty into any of the analyses of glass hitherto published. I am therefore induced to

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[*] See in a former paper entitled, "On the Property of Silicic and Tungstic Acids to retain Phosphoric Acid," &c., Chem. News, Vol. xvi. p. 187 (*Amer. Repr., Dec.*, 1867, page 289).

describe the following simple process for its extraction. Pound the glass to be examined extremely fine, and shake it up with twice its volume of weak ammonia, and allow it to rest until the supernatant liquid is clear, which will be in about 24 hours afterwards; the clear liquid is then ready for the molybdenum test. By this process I have already detected phosphoric acid in German glass test tubes, in Bohemian combustion tubes, and in several other glasses, into the manufacture of which lead does not enter.—*William Skey, New Zealand.*

Lead Floating on Molten Iron.—Some experiments have been made in Germany which seem to show that molten lead when dropped upon liquid iron remains floating on the surface of the latter. As the specific gravity of lead (11·5) is more than one-half greater than that of cast-iron (7), there arose some discussion on this subject, which has been recently closed in a very satisfactory manner by the researches of Professor Karmarsch, of Hanover. An ironmaster in the vicinity of that town had sent to the Professor some samples of such drops of lead lying imbedded in the surface of a cast-iron block, and which had been produced in the manner above described. Professor Karmarsch found, upon close examination, that these drops of lead, instead of being solid globules, as was supposed at first sight, were all found to be hollow, forming bubbles composed of metallic skin, and apparently empty in the centre, so far as his observation was carried. He explains the whole by supposing that the molten lead, at the temperature to which it is raised by the contact with the liquid iron, forms an incipient vapour of lead, which is prevented from escaping by the skin of solidifying metal which forms on the top. The lead vapour, according to this explanation, keeps the lead resting upon the surface of the iron. It seems that in large quantities the result is different, since it is known that lead is occasionally tapped from the bottom of the blast furnaces which smelt certain classes of ores containing lead, and in these cases the lead is found below the liquid iron according to its greater specific gravity.

Tyrian Purple.—The Tyrians were probably the only people of antiquity who made dyeing their chief occupation, and the staple of their commerce. The opulence of Tyre seems to have proceeded, in a great measure, from the sale of its rich and durable purple. It is unanimously asserted by all writers that a Tyrian was the inventor of the purple dye, about 1,500 years B.C., and that the King of Phœnicia was so captivated with the colour, that he made purple one of his principal ornaments, and that, for many centuries after, Tyrian purple became a badge of royalty. So highly prized was this colour, that in the time of Augustus, a pound of wool dyed with it, cost at Rome a sum nearly equal to thirty pounds sterling. The Tyrian purple is now generally believed to have been derived from two different kinds of shell-fish, described by Pliny under the names *purpura* and *buccinum*, and was extracted from a small vessel or sac in their throats to the amount of one drop from each animal; but an inferior substance was obtained by crushing the whole substance of the *buccinum*. At first it is a colourless liquid, but by exposure to air and light it assumes successively a citron yellow, green, azure, red, and, in the course of forty-eight hours, a brilliant purple hue. If the liquid be evaporated to dryness soon after being collected, the residue does not become tinged in this manner. These circumstances correspond with the minute description of the manner of catching the purple dye fish given in the work of an eye-witness, Eudocia Macrembolitissa, daughter of the Emperor Constantine the Eighth, who lived in the eleventh century. The colour is remarkable for its durability. Plutarch observes, in his life of Alexander, that, at the taking of Susa, the Greeks found, in the Royal treasury of Darius, a quantity of purple cloth, of the value of five thousand talents, which still retained its beauty, though it had lain there one hundred and ninety years. This colour resists the action even of alkalies, and most acids. Pliny states that the Tyrians gave the first ground of their purple dye by the unprepared liquor of the *purpura*, and then improved or heightened it by the liquor of the *buccinum*. In this manner they prepared their double-dyed purple—*purpura dibapha*—which was so called, either because it was immersed in two different liquors, or because it was first dyed in the wool and then in the yarn.—*Prof. Dussauce.*

Explosive Powder for Blasting Rocks, etc.—Experiments have recently been made, in cutting for a tunnel at Milford, between Nobel's nitroglycerine and the explosive powder which is manufactured, without danger of decomposition or spontaneous explosion, by Mr. Horsley. The preference was given to the powder, not only for its power when exploded in a particular kind of cylinder, but also for its economy, and its greater safety in use and storage. It requires a temperature of 475° to ignite. It has been tried and approved by the Admiralty, and is much liked by miners, who say they are afraid of blasting oil, and think gun-cotton useless.

CONTEMPORARY SCIENTIFIC PRESS.

[Under this heading it is intended to give the titles of all the chemical papers which are published in the principal scientific periodicals of the Continent. Articles which are merely reprints or abstracts of papers already noticed will be omitted. Abstracts of the more important papers here announced will appear in future numbers of the Chemical News.]

Bulletin de la Société Chimique de Paris. August.

Brisse: "*A new Method of Manufacturing Soda.*"—Schiffman: "*A Method of Manufacturing Hyposulphite of Soda from the Waste Products of Soda Making.*"—Clemandot: "*A new Silica Glass for Pottery Ware.*"—J. Fuchs: "*On Preparing Metals in Fine Powder by Amalgamation.*"—Evrard: "*On the Manufacture of Aluminium Bronze.*"—W. Fuller: "*A Method of Separating Gold and Silver from their Ores by means of Lead.*"—Esquiron and Gobin: "*On a Method of Revivifying Peroxide of Manganese.*"—Tilghman: "*A Process for Manufacturing Paper Pulp from Wood.*"—J. Rousseau: "*An Improvement in the Manufacture of Beet-root Sugar.*"—Junemann, Du Rieux, and Bottger: "*On the Use of Strontia in the Manufacture of Sugar.*"—Du Rieux and Bottger: "*A new Method of Treating Beet-root Pulp and Juice with Lime.*"—Bourne: "*A Method of Deodorising Vulcanised India Rubber.*"—Fortier: "*On a Process for rendering Fabrics Waterproof.*"

Annales de Chimie et de Physique. August, 1867.

A. Cornu: "*Researches on Crystalline Reflection.*"—P. Vigier: "*On the Metallic Phosphides.*"—W. D. Khanikof and V. Louguinine: "*Some Experimental Researches on Henry and Dalton's Theory of the Absorption of Gases by Liquids at a Constant Temperature and under variable Pressures.*"—J. Boussingault: "*On the Fermentation of Stone Fruits.*"—V. Longuinine: "*On the Density and Dilatation of Benzene, Toluene, Xylene, and Cymene.*" Perret: "*Note on the Use of Pyrites instead of Sulphur in the Manufacture of Sulphuric Acid.*"

Bulletin de la Société Industrielle de Mulhouse. July, 1867.

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Dingler's Polytechnisches Journal. August, 1867.

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C. Aubel: "*A new Method of Estimating Dioxide of Copper in Refined Copper.*"—A. E. Nordenskjold: "*On Selenium and Thal* *lium Ores (Crookesite, Eucairite, and Berzelianite) from the Skrikerum Mine."*—*"On the Use of Creosote for protecting Timber against the Teredo."*

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A. W. Hofmann: "*On a new Series of Homologues of Hydrocyanic Acid.*"—Niepce de Saint-Victor: "*On some newly discovered Chemical Effects of Light.*"—Berthelot: "*On the Hydrocarbons of Coal Tar: Acenaphthene and Anthracene.*"—Prat: "*On the Chemical Constitution of Fluorine Compounds.*"

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Bulletin de l'Academie Royale de Belgique (*Classe des Sciences.*)

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Schwann: "*Report on E. Husson's Chemical and Physiological Researches respecting the Action of Alkaline Silicates on the Animal Economy.*"—Gluge: "*Report on the same Memoir.*"—Melsens: "*Report on the same Memoir.*"—Kekulé: "*Report on W. Körner's Paper on the Synthesis of Anisic Acid, of Methyloxybenzoic Acid, of a new Cresylic Acid, and on Paraiodobenzoic Acid.*"—"*Report on Glaser and Radziszewsky's Memoir on some Transformations of Formobenzoic Acid.*"—Stas: "*Report on the same Memoir.*"—Kekulé: "*Report on Körner's Contributions to the Determination of Chemical Position in the Aromatic Series.*"—Stas: "*Report on the above Memoir.*"—Kekulé: "*Report on H. Ronday's Memoir on Homotartaric Acid.*"—Stas: "*Report on H. Ronday's Memoir on Itamalic Acid.*"—A. Kekulé: "*On the Sulphophenic Acids.*"—E. Husson: "*Researches on the Action of Alkaline Silicates on the Animal Economy.*"—W. Körner: "*Note on the Synthesis of Anisic Acid, of Methyloxybenzoic Acid, of a new Cresylic Acid, and on Paraiodobenzoic Acid.*"—Glaser and Radziszewsky: "*On some Transformations of Formobenzoic Acid.*"—W. Korner: "*Contributions to the Determination of Chemical Position in the Aromatic Series.*"—H. Ronday: "*Note on some Salts of Itamalic Acid.*"—"*Preliminary Note on Homotartaric Acid.*"

Journal für Praktische Chemie. September 24, 1867.

F. Stolba: "*On Silicofluoride of Rubidium.*"—*On Crystallised Silicofluoride of Copper.*"—F. Rochleder: "*On Aescigenine and on two allied Substances—Caincine and Chinovine.*"—A. Claus and C. Keese: "*On Neurine and Sincaline.*"—R. Otto and H. Ostrop: "*On the Action of Chlorine on Sulphobenzide.*"—H. Vohl: "*On some hitherto unknown Properties of pure Naphthalin.*" "*On the Detection of Naphthalin.*"—"*On the Preparation of Sulphide of Copper and Ammonium.*"—C. Winkler: "*A Method of Preparing Hydriodic Acid.*"—K. Haushofer: "*On some Malacolite from Gefrees, Bavaria.*"—"*On some Glauconite from the Cenomanien at Havre.*"—F. Reindel: "*On Prussian Blue.*"—"*On some Compounds of Ferrocyanides and of Ferricyanides.*"—A. Froehde: "*On the Part which Nitrite of Ammonia plays in Nature.*"

Bulletin de la Société d'Encouragement. August, 1867.

G. de Claubry: "*Report on P. Havrez's Improved Arrangement of Lixiviating Apparatus in which a Single Distributing Cock is employed.*"—Salvetat: "*Report on Brianchon's Pearl Glass for Glass and Porcelain.*'—Asmus: "*On the Manufacture of Caramel.*"—J. Stinde: "*On the Manufacture of Formic Ether.*"—J. Feuquieres: *On the Electrolytic Deposition of Tin on Lead which will bear Rolling.*" "*On the Electrolytic Deposition of Iron of Great Hardness.*"—Troost: "*On Obtaining Steel from Cast Iron by the Action of a Current of Oxygen.*"

Journal des Fabricants de Papier. September 1, 1867.

E. Bourdilliat: "*On Testing the Chemical Products used in Paper-making.* (*Continuation.*) *Bichromate of Potash. Acetate of Lead. Litharge. Chlorides of Tin.*" "*On the Use of Sulphite of Lime as an Antichlore.*

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E. Bourdilliat: "*On Testing the Chemical and other Products used in Paper-making.* (*Continuation.*) *Animal and Vegetable Fibres.*"

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J. Thomas: "*On a new Colouring Matter derived from Sericographis mohitli.*

Comptes Rendus. October 14, 1867.

Chasles: "*Answer to Faugère's Letter on the Authenticity of the Newton and Pascal Correspondence.*"—Morin: "*Observations on some Correspondence between James II. and Louis XIV., recently brought under the notice of the Academy, apropos of the Newton and Pascal Controversy.*"—Le Verrier: "*On the Authenticity of the Newton and Pascal Correspondence.*"—Sir D. Brewster: "*On the Question of the Application of Dioptric Lenses to Lighthouses.*"—J. Fournet: "*On the Path of Thunder Storms over various Parts of Switzerland.*"—J. Lemaire: "*On the Presence of Bacteriums and other Organisms in Emanations from the Human Body.*"—Faugère: "*Letter on the Authenticity of the Newton and Pascal Correspondence.*"—G. Kirchhoff: "*On the Theory of Sun Spots.*"—Fordos and Gélis: "*Remarks on Riche's and Kolb's Memoirs on the Nature of the Chlorides used for Bleaching.*

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E. Schwarze: "*On the Use of Picric Acid and other Substances for preparing Microscopic Objects in Two or more Colours.*" "*Report on the Prize of* 1000 *Florins for an Essay on the Progress of Mineralogy during the Years* 1862-65."

Poggendorff's Annalen der Physik. August 7, 1867.

E. Edlund: "*On the Property possessed by a Voltaic Current of causing Solids to expand independently of the Heat produced by the Passage of such Current.*"—C. Rammelsberg: "*On the Phosphites.*" E. Schone: "*On the Compounds of Sulphur with the Metals of the Alkalies.*"—H. W. Schroder van der Kolk: "*On the Mechanical Energy of Chemical Combination: Combustion.*" "*Reply to H. Deville's Remarks on the Author's Paper on the Theory of Dissociation.*" F. Melde: "*On a peculiar Mode of Formation of Sound Pulses, and on a Method of Counting the same.*"—R. Weber: "*On some Compounds of Chloride of Platinum and of Chloride of Gold.*"—F. Goppelsroder: "*On a Fluorescent Substance obtained from Fustic.*"—K. L. Bauer: "*On the Refraction of Light, and on the Angle of Minimum Deviation in Prisms.*"—H. Gerlach: "*Contributions to the Mechanical Theory of the Voltaic Current.*"—Hoh: "*On some Remarkable Effects of Lightning.*"—J. C. Poggendorff: "*On the Mutual Reaction of Two Induction Machines.*"

Annalen der Chemie und Pharmacie. September, 1867.

O. Loew: "*On Sulphonaphthalic Acid.*"—G. Wischin: "*On Phenylenediethylacetone and Ethylenediethylacetone.*"—F. Gauhe: "*On Sulphurous Cyanide and other Decomposition Products of Sulphurous Chloride.*"—O. Rembold: "*On Quino-tannic and Quinova-tannic Acids.*"—A. Grabowski: "*On Rhatania-tannic Acid.*"—G. Malin: "*On Filico-tannic Acid.*"—A. Grabowski: "*On Filicic Acid.*"—O. Rembold: "*On the Tannic Acid of the Root Bark of the Pomegranate Tree.*"—H. Hlasiwetz: "*On the Relations between the Tannic Acids, Glucosides, Phlobaphenes and Resins.*"—L. Carius: "*On Chlorous Anhydride and Benzol*"—C. Glaser: "*Researches on some new Derivatives of Cinnamic Acid.*"—E. Linnemann: "*On the Transformation of the Bromides of the Hydrocarbons belonging to the Series* C_nH_{2n} *into Acetones of the Fatty Acids containing the same Quantity of Carbon.*"—W. Heintz: "*On Phosphate of Zinc and Phosphate of Zinc and Ammonia.*"—C. O. Cech: "*On a new Method of forming Viridic Acid.*"—F. Beilstein: "*On the Behaviour of Toluol towards Bromine.*"—A. Gescher: "*On Sulphide of Copper and Ammonium.*"

Bulletin de la Société Chimique de Paris. September, 1867.

E. Jungfleisch: "*On some Mutual Relations between the Melting Points, Boiling Points, Densities, and Specific Volumes of some Chlorinated Derivatives of Benzene.*"—C. Friedel and A. Ladenburg: "*On a Bromide of Propylene derived from Acetone.*"—D. Gernez: "*Resumé of the Author's Investigations on Supersaturated Solutions.*"—C. Marignac: "*On an Analysis of Æschynite.*"—"*On the Separation of Niobic from Titanic Acid.*"

Mémoires de la Société des Ingenieurs Civils de Paris.

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Limet: "*Analysis of a Specimen of Boiler scale.*"—Simonin: "*On the Coal Fields and Mineral Veins of the Old and New World.*"—E. Flachat: "*On the same subject.*"—E. Flachat: "*On the Speci-*

men of Boiler Scale exhibited by Limet."—**Fargot, Limet, Tresca**: "*On the same subject.*"—**Ribail**: "*On the Use of Caustic Soda for preventing the Formation of Boiler Scale in Locomotives.*"—**Foucou**: "*Note on the Deposits of and Modes of obtaining Petroleum in North America, together with some Account of the Theories which have been proposed to account for its Origin.*"—**W. Clarke**: "*On the Modes of obtaining Petroleum in America.*"—**A. Morin**: "*Report on Technical Education.*"—**Delonchant**: "*On the Use of Plates of Mica for indicating the various Planes in Models for teaching Descriptive Geometry.*"—**Arson**: "*Analysis of a Deposit from a Feed-water Heater.*"—**Flachat**: "*Note on some Spherical Concretions found in the Boilers of the Transatlantic Steamer "Ville de Paris."*

Mittheilungen des Gewerbe-Vereins für Hannover.

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J. Kolb: "*On the Absorption of Carbonic Acid by some Oxides.*"—**Jundt**: "*Report on Braun's Carbon Process for obtaining Photographic Facsimiles of Sketches and Cartoons.*"—**A. Dollfus**: "*On Cadmium and its Industrial Uses.*"—**Dollfus**: "*On the Use of Cadmium as a Substitute for Bismuth in the Metal Plates for printing Fabrics.*"—**G. Schaffer**: "*Report on Pernod's new Extract of Garancine.*"—**Kühlmann**: "*On some Methods of Fixing the Gases of Stables, and of using them as Manure.*"—**Saco**: "*On the Use of Benzine for Extracting the Colouring Matter of Garancine and other Dyes.*"—"*On the Use of German Silver for the Manufacture of Watch Cases in Switzerland.*"—**Kuhlmann**: "*On the Action of Water on Lead.*"

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Dupuy and **Turpin**: "*An Improved Retort for Distilling Resins, and for Preparing the Oil so obtained for Lubricating Purposes.*"

October.

D. Savalle and Co.: "*Apparatus for Distilling and Rectifying Alcohol.*"—**D. Lapparent**: "*Note on a New Process for Charring Timber.*"—**Weinberger** and **Laffourcade**: "*A Portable Apparatus for the Manufacture of Alcohol from Refuse Grapes.*"—**A. E. Rudberg**. "*An Improved Method of Manufacturing Nitroglycerine and of Exploding the same in Blasting Operations.*"—**Cholet**: "*A Solder for Aluminium Bronze.*"

Comptes Rendus. October 28, 1867.

Payen: "*On the Use of Osmose in the Manufacture of Sugar.*"—**E. Chevreul**: "*A Comparative Examination into the relative Facility with which French and Japanese Silks take the Dye.*"—**A. Poey**: "*Remarks on the Ozonoscopic Colourations obtained with the Jame Test, and on Berigny's Ozonometric Scale.*"—**Le Verrier** *on the same subject.*—**Chevreul**: "*Observations on the Discrimination of Colour apropos of Poëy's Paper.*"

Sitzungsberichte der Kaiserlichen Akademie der Wissenschaften zu Wien. (Mathematisch-Naturwissenschaftliche Classe.)

May, 1867.

A. F. Reibenschuh: "*On Crystallised Ankerite from Erzberg, Upper Styria.*"—**F. Ullik**: "*Researches on Molybdic Acid and its Salts.*"—**Gottlieb**: "*Analysis of the Emma Spring at Gleichenberg, Styria.*"—**W. F. Gintl**: "*On the Volumetric Estimation of Soluble Ferrocyanides and Ferricyanides by Means of Chamæleon Mineral.*"—**A. Brio**: "*Researches on the Optical Properties of Oxalate of Ammonia, Bitartrate of Soda, and of Formiate of Copper and Strontia.*"—**E. Brucke**: "*On the Behaviour of some Albuminoid Substances towards Boracic Acid.*"

June—July.

A. Lielegg: "*On the Spectrum of the Flame of Bessemer Converters.*"—**F. Rochleder**: "*On Æscigenine, and on some Allied Substances—Caincine and Chinovine.*"—**H. Allemann**: "*Chemical Analysis of the Waters of the Mineral Spring at Ebriach, Carinthia.*"—**J. Wolff**: "*Chemical Analysis of the Mineral Spring at Sztojka, Transylvania.*"—**M. Erofejeff**: "*Researches on the Optical Properties of Sulphate of Iron.*"—**S. Konya**: "*Chemical Analysis of the Spring at Baden, near Vienna.*"—**E. Brucke**: "*On the Structure of the Red Corpuscles of the Blood.*"—**F. Rochleder**: "*On Saponine.*"—**S. Mayer**: "*On the Quantity of Fibrine separated from Blood during Coagulation.*"—**L. Pfaundler**: "*On the Capacity for Heat of the Hydrates of Sulphuric Acid.*"—**F. Ullik**: "*On some Compounds of Tungstic Acid.*"—**H. Allemann**: "*On the Chemical Composition of Maize Oil.*"—**W. Baxt**: "*On the Physiological Action of some Alkaloids contained in Opium.*"—**E. Ludwig**: "*On the Presence of Triethylamine in Wine.*"

PATENTS.

Communicated by Mr. **Vaughan**, F.C.S., Patent Agent, 54, Chancery Lane, W.C.

GRANTS OF PROVISIONAL PROTECTION FOR SIX MONTHS.

3359. E. Belknap, Mortimer Street, Cavendish Square, Middlesex, "Improvements in the treatment of the solution of malt for brewing."—Petition recorded November 27, 1867.

3384. J. Baylis, Durdham Down, Bristol, "An improved chemical preparation or compound to be used in preparing mixed textile fabrics for dyeing or colouring."—November 28, 1867.

3388. T. Rose, Oxton, Cheshire, and R. E. Gibson, New Brighton, Cheshire, "An improved mode of treating cotton seed to obtain oil therefrom, and in machinery employed therein."

3389. O. Albisser, Mincing Lane, London, "Improvements in the preparation of sulphate of magnesia, applicable to the treatment of the crude potash, salts of Stassfurt, and the refuse from the manufacture of chloride of potassium."—A communication from J. Vorster, and H. Grüneberg, Cologne, Prussia.—November 29, 1867.

3405. W. R. Lake, Southampton Buildings, Chancery Lane, "An improved mode of and means for clarifying saccharine solutions."—A communication from J. E. Freund, New York, U.S.A.—November 30, 1867.

3440. J. Gjers, Middlesbrough, Yorkshire, "Certain improvements in the manufacture of cast steel and homogeneous iron."—December 3, 1867.

3463. S. Perkins, and W. Smellie, Gorton, near Manchester, "Improvements in the manufacture of malleable metal of a steely quality, partly from Bessemer 'scrap' or other Bessemer metal."

3469. P. G. L. G. Designolle, Rue de la Seine, and J. Casthelaz, Rue Ste. Croix de la Bretonnerie, France, "Improvements in the manufacture of explosive and fulminating powders."—December 5, 1867.

3473. J. Durrans, Thurlstone, near Penistone, Yorkshire, "An improved material or composition to be employed for covering or coating the interior surfaces of moulds, crucibles, or ducts, previous to their receiving the molten metal in the process of casting, and for other purposes."—December 6, 1867.

3483. R. B. Jones, Nelson Terrace, City Road, Middlesex, and W. Powell, Circus Place, Finsbury, Middlesex, "Improvements for the prevention of incrustation in steam boilers."—December 7, 1867.

3499. L. Rose, Leith, Scotland, "An improved mode of preserving vegetable juices."

3502. O. Martin, Chancery Lane, W. Barrett, and T. S. Webb, Norton, Durham, "Improvements in the treatment and reduction of titaniferous iron ores, and in the manufacture of iron, and in the construction of furnaces to be employed therein."—December 9, 1867.

3517. A. M. Clark, Chancery Lane, "An improved process for the reduction of tin, so as to render it applicable for coating metals and for other purposes."—A communication from E. Cornelis, Boulevart St. Martin, Paris.

NOTES AND QUERIES.

It has been represented to us that our column of Notes and Queries has occasionally been made the vehicle for the surreptitious disposal of trade secrets by subordinates in chemical works, unknown to their principals. This column has proved to be sufficiently useful to a large class of our readers for us to be reluctant to discontinue it for the sake of a few who abuse its privileges. Probably a more rigid supervision will enable us to obviate the difficulty. There will be no objection to a correspondent asking for information on trade subjects; but the answer must likewise be made public, and in such cases no name or address can be given, no private communications forwarded through us, and no offer of payment for information can be published.

Deodorising Petroleum.—I have been informed that a solution of "plumbate of soda" will deodorize petroleum. Can any of your correspondents courteously give me instructions in the mode of preparing and using this agent.—O. P. A.

Cleansing Fire-arms.—Can any of your readers kindly inform me as to the composition of the "spirit" sold for cleansing fire-arms. I imagine turpentine is one of the principal ingredients.—**Thomas Blair.**

Water for Steam Boilers.—E. S. T., in "Notes and Queries," will find all he requires in "Engineering," (published at 37 Bedford Street, Covent Garden.) for March 22d, 1867, page 280; for March 29th, 1867, page 304; and May 10th, 1867, page 484.—H. K. B.

Extraction of Oil—Dyeing Turkey Red.—In answer to correspondent E. (*Amer. Repr., Feb.* 1868, *page* 99), I can state that bisulphide of carbon is inapplicable to the extraction of olive oil from an aqueous solution of carbonate of potash, as bisulphide of carbon is decomposed in alkaline solutions forming sulpho-carbonate of the alkali, which still keeps the oil in solution. The process carried out in Turkey-red works for the recovery of oil is to neutralize the emulsion with sulphuric acid when the oil separates. Hypusulphite of alumina has been tried as a mordant in Turkey-red dyeing, but does not produce colours so brilliant as the mordants in use. To describe the Turkey-red process, as carried on in Lancashire, would occupy more than the space set apart for "Notes and Queries;" but I may state here that the Turkey-red dyers of England are three in number. Two of them adopt a process similar to that invented and carried on by F. Steiner, Esq., of Accrington, who is the third. This method cannot be applied to the dyeing of yarns. The process for yarn dyeing is a modification of the Scotch process for cloth.—F. F. F.

Mordant for Green on Cotton.—Could you kindly inform me through your Chemical News what is the best mordant for dyeing the new greens on cotton yarns?—W. T. C.

Bleaching Calico.—Can any one inform me, how far in bleaching calico, the bleaching powder solutions are exhausted? Can they be refreshed with new powder *ad infinitum*, or must they be thrown away when a certain quantity of fabrics has passed through them? If so, is there any practical rule for guiding the manufacturer, such as the observation of the specific gravity of the solution? It appears some bleachers exhaust their solutions much more than others.—G. L.

Deodorising Petroleum.—Your correspondent O. P. A. desires to receive information on the preparation of "plumbate of soda," its use and application in deodorising petroleum; when oxide of lead or litharge is added to a pretty concentrated solution of caustic soda, the oxide of lead is dissolved therein, and may be considered to play towards the soda the part of an acid; the clear solution may be used with advantage to deprive petroleum of some foul smelling compounds it may happen to contain, especially as sometimes occur organic sulphur compounds, by thoroughly shaking and mixing the petroleum with the plumbate of soda, and afterwards giving sufficient time for the liquids to separate in two layers; the upper layer being the petroleum, the latter will have to be washed with water to remove the adhering soda, and should then be deprived of moisture by applying lumps of caustic lime.—Dr. A. A.

Aniline Dyes.—Could you kindly inform me as to the best method of stripping off the aniline dyes of goods, and if any book is published on the manufacture and method of dyeing these colours?—W. P. B.

Mordant for Green on Cotton.—If W. T. C. will send his address, T. Charlesworth, jun., Leicester, will send him the required mordant.

Mordant for Green on Cotton.—Je viens de lire dans votre dernier numero, qu'un de vos correspondants cherche à avoir le procédé d'application du nouveau vert sur coton. Comme je suis aussi bien teinturier que fabricant de produits chimiques, veuillez dire à votre client de m'envoyer quelques livres de coton, que je teindrai par mon procédé en nouveau vert. Ci-inclus un échantillon de coton teint par mon procédé. Dans l'attente de vos nouvelles; veuillez reçevoir, Monsieur le Directeur, &c.—Pierre Clavel. Bale, le 13 Janv., 1868. [The specimen of dyed cotton mentioned by M. Clavel, can be seen at our office.]

Diphenylamine.—Could you inform me how I might make diphenylamine? There is a process mentioned in "Watts's Chem. Dict.," but so expensive that it is impossible to use it. There must be another plan of making it, I believe, for I find a process which speaks of commercial diphenylamine.—Ignoramus.

Extraction of Oil.—Dyeing Turkey Red.—Will you kindly inform your correspondent "E," of Notes and Queries of 20th of December last (*Am. Repr., Feb.*, 1868, *page* 99), that if he will correspond with me on the subject on which he inquires, I shall be happy to give him all information necessary.—Rudolph Schombург, Uccle, Belgium, 2 Jan., 1868.

Constant Galvanic Current.—The following observations may have occurred to others, but not having met with them published, they may be of value as tending to the perfection of our scientific instruments, by providing the source of a constant galvanic current, of large quantity and very great intensity. The bichromate of potash battery furnishes a current of great force, and its simplicity, economy, and convenience of management would make it preferable to the double fluid batteries, but for its want of constancy when a current of large quantity is required. Experimenting with it lately, I became satisfied of the cause of this defect. Although there may be a large reservoir of liquid, only the stratum between the plates is active, and as no gas being given off there is no circulation; this soon becomes exhausted, and, as it is renewed merely by diffusion, can only maintain a current equivalent to the fresh supply of liquid thus obtained. I therefore used a thin beaker as the containing vessel and placed it over a Bunsen's burner capable of maintaining a moderate circulation of the liquid, and, as I expected, the battery now gave its fullest force with absolute constancy until the complete exhaustion of the exciting fluid. Mechanical stirring of the liquid or motion of the plates will produce a similar result; and thus by any of the various modes which may be employed, this battery can be made to yield a current more powerful than any other known form, without giving off any noxious gases, and as absolutely constant as can be desired.—John T. Sprague.

Determination of free Sulphuric Acid.—I want to determine the free sulphuric acid in superphosphates, and do not quite know how to do it. Is it possible to do it by shaking the solution with oxide of lead and to determine afterwards the sulphate of lead formed? Can any of your correspondents tell me if this plan would do, or can they tell me of another way.—V. C.

To Prevent Water Freezing.—Can any of your readers oblige by answering the following questions: 1. What proportion of salt must be added to water to prevent it freezing, when exposed to the coldest weather known in this country? 2. What percentage of alcohol should water contain for the same purpose? 3. Is there any other cheap substance which would effect the same object, and which would not attack iron?—Volta.

Bleaching Calico.—Your correspondent G. L. desires to receive information relating to "Bleaching Powder Solutions." It is not absolutely necessary to throw away the solutions after you have passed a certain quantity of fabrics through them, unless in the case of very fine goods, or where there are any coloured ornaments in the cloth to preserve; the solution, if it has been kept too long, is apt to injure the fabric or decolorize the ornaments. The usual method for testing solutions of bleaching powder is by means of the sulphate of indigo test. The hydrometer being a very fallacious test, I would recommend your correspondent to make himself acquainted with the chlorimeter test introduced by the late Mr. Walter Crum, and which is so easily manipulated that a workman of ordinary intelligence can easily understand it. The chlorimeter test can, if I mistake not, be procured from Griffin, who I have no doubt will give every information regarding it.—A. G. S.

Detection of Magnesia in the presence of Manganese.—When a compound is perfectly free from magnesia, but contains manganese, together with phosphates insoluble in water, on adding ammonia and hydric disodic phosphate (Na_2HPO_4) a precipitate is almost invariably obtained; this precipitate has very much the appearance of triple phosphate, and might be readily mistaken for it; but analysis shows that it does not contain any magnesia. Its formation is due to MnO being carried down with Fe_2O_3 on boiling with potash; by boiling with chloride of ammonium the MnO is dissolved out; now on treating with Na_2HPO_4 and ammonia, the precipitate in question is obtained, which adheres to the sides of the vessel where it has been rubbed by the rod, in exactly the same manner as the magnesia precipitate. This reaction does not take place if the precipitate containing MnO be exposed to the air for some time, for then it absorbs oxygen, and forms MnO_2, which is insoluble in chloride of ammonium. Mg may be readily detected in the presence of Mn by dissolving the precipitate in HCl, neutralising with ammonia, and precipitating the Mn by means of ammonic sulphide and again adding Na_2HPO_4 and ammonia, when the Mg will be precipitated, and this may be known to be free from Mn by its not colouring a borax bead.—A. Liversidge.

Dyeing Black.—Could you kindly inform me of the best method for dyeing a good black for polishing, and the best means to dye the yarn through.—R. B. C.

Yellow Chromate of Lead.—Can any of your correspondents give me a process for preserving, with its original lemon tint, yellow chromate of lead in paste. The method of precipitating with excess of sulphate of lead is unsatisfactory.—A. C. B.

Oxychloride of Magnesia.—I am engaged in a business connected with the use and preparation of cements in the building trade, and would thank any of your numerous scientific friends to inform me where I might find cheaply a crude carbonate of magnesia suitable for making oxychloride of magnesia for siliceous cementation, and a few hints as to the best mode of its preparation and use.—J. E. Hamilton.

Free Sulphuric Acid.—A correspondent in your last issue (*Am. Reprint, March*, 1868, *page* 155) desires a process for the estimation of "Free Sulphuric Acid in Superphosphates." I have much pleasure in assisting him. The following is a method which I have frequently tried, and is satisfactory for the purpose. A water solution of the manure being made, evaporate slowly until a small quantity only is left: add about seven volumes of concentrated alcohol, and allow to settle in the cold for some hours. This precipitates all sulphates, and leaves in solution, besides phosphates, the free sulphuric acid. Filter, wash with alcohol, add a large amount of water to the solution, and carefully evaporate off the spirit, and estimate the acid by baric chloride, etc. The soluble phosphates do not in any way interfere.—R. Carter Moffat, Ph. D., Glasgow.

Weights and Measures.—Now that foreign weights and measures are so much used in scientific books, it would be a great advantage to those who like myself are not very conversant with the value of these figures, if one of your clever mathematical correspondents would calculate and publish in your columns some simple factors whereby kilogrammes could be converted into cwts. and tons, francs into shillings and pounds sterling, grammes into ounces, metres into feet and yards, etc., etc., by a simple process of multiplication.—Ignoramus.

ANSWERS TO CORRESPONDENTS.

NOTICE.—The American Publishers of The Chemical News *give notice that in accordance with a suggestion of* Mr. Crookes, *the Editor and Proprietor of the English publication, they will be pleased to receive and forward to him in London any scientific publications issued in America, for review—and also any Notes and Queries, Articles, Correspondence, etc., for publication or reply. Their facilities of communication with* Mr. Crookes *render this very desirable to all persons in the United States who wish to confer with him. Address,*

W. A. TOWNSEND & ADAMS,
434 *Broome Street, New York.*

W.—1. The oil-nut has been submitted to a high botanical authority for report upon. 2. We believe no one bought the business you speak of. You can get everything you require in the way of apparatus at Griffin & Sons, Garrick Street.

W. R.—Will our correspondent favour us with the full description of the dyeing process which he thinks would be too long for our columns?

X.—The explanation is very simple. Each acid will separate a portion of the other from its combinations. Our Publisher says that he can supply the periodicals if you will send particulars.

W. H. Vodrey.—We are obliged for our correspondent's calling attention to the article, and will endeavour to meet his wishes. It is very gratifying to find that the American Reprint of the Chemical News is so highly appreciated in all parts of the United States.

J. W. Young.—Received with thanks.

D. Waldie.—The remarks referred to in the Quarterly Journal were

[English Edition, Vol. XVII., No. 422, page 15; No. 423, page 25; No. 424, page 37; No. 426, page 49; No. 426, page 61; No. 422, page 14.]

umns.

Spectrum will find some excellent articles on the application of spectrum analysis to the microscope, by Mr. Sorby, in recent volumes of the Chemical News.

Enquirer.—The French metre is the forty-millionth part of the length of the earth's meridian, or the ten-millionth part of the distance from the equator to the pole.

A Mystery.—A correspondent forwards the following cutting from a contemporary, and asks for an explanation: "Succinic Acid.—M. J. Hooper, in a communication to the Chemical News, points out that the presence of active acid or a soluble acetate, partially or entirely suspends the ordinary reaction of succinic acid in solutions of ferric salts."

H. Bamber.—Coat the polished steel with a mixture of lime and sweet oil before putting it away. This will prevent it getting rusty.

Analyst.—A mixture of finely granulated zinc and iron filings, put into an alkaline solution of a nitrate, will cause all the nitrogen to be evolved in the form of ammonia. Harcourt's process for estimating nitrates is based on this principle.

Xpect.—You can filter the strong nitric acid solution through a tuft of gun cotton loosely pushed into the neck of a funnel.

E. K. B.—We strongly advise you not to attempt to prepare chloride of nitrogen. Nitroglycerine is innocence itself in comparison to it.

T. Sterry Hunt (*Montreal*).—Arrived too late for insertion in this number. It shall appear next week.

S. Maldon.—You must use the best charcoal iron. "Pig" is too impure.

Physicist.—The exact equivalent of heat is 772 foot-pounds, according to Joule's most recent researches. The probable error is considerably less than 1 lb.

W. Murray.—Dr. Phipson has detected the presence of xanthic oxide in guanos which contain no uric acid.

Querist.—The colour of green pickles may be greatly improved by previously boiling the vegetables in water containing a quarter of an ounce of liquid ammonia to the quart.

Carbo.—Permanganic acid is volatile but very unstable. You will not be able to make use of this property in separating manganese quantitatively from iron.

Exhibit.—The list of jury awards in class 2, section A, was given in the Chemical News, vol. vi., p. 62, *et seq.* (*Eng. Ed.*)

Dr. Wilhelmi.—The price of the German edition of "Geschichte der Chemie," by Dr. T. Gerding, is 9s.

W. Davies.—The best practical information which we know of on the subject of grinding lenses is given in a little work published by Secristan, of Paris, entitled "De la distance Focale des Systémes Optiques Convergents." The price is about 2s., 6d., or 3s.

W. Bird Herapath.—Received with thanks. A proof shall be forwarded shortly.

Moonstone.—Amongst mineralogists the name moonstone is applied to one of the varieties of felspar. Fine specimens possess a certain value, but they cannot legitimately be called gems. Adularia is another name for the same stone.

Physic.—Test the deposits for urate of soda. They are probably "chalk stones."

O. Reiman.—There are no less than 52 separate products of the destructive distillation of coal. It would occupy nearly the whole of this column to give the list. Dr. Frankland's Lectures on Coal Gas (reported in this journal in the spring of last year) will give you full information on this point.

H. D. S.—There are many reasons against the supposition that the chemistry of silicium and of boron would be as extensive as that of carbon if fully investigated. The gaseous character of the oxides of carbon, as contrasted with the fixed nature of the silicium and boron oxides, and the difference between the hydrogen compounds of one and the others, are much against the view which some chemists have adopted. At the same time it must not be forgotten that the number of organic compounds containing silicium is increasing daily.

Surprise.—We have the best reason to believe that the rumour of the "distinction," which it is said is about to be conferred, is unfounded. It would in fact be almost an insult to offer it to such a man.

Hafu.—The existence of native zinc in nature is considered somewhat problematical.

H. Hordlicska.—Your communication has been forwarded to the required address. You will must likely hear direct by post. If any difficulty occurs about sending letters we will communicate through this column.

N., Hoxton.—Write to the Secretary, Burlington House, W.

Enquirer asks for the best work treating on the combustion of coal in furnaces.

Student.—See Dr. Miller's lectures on spectrum analysis, reported two years ago in this journal. No special book on this subject has yet been published in England. A very good one in Dutch was written some years ago by M. Dibbits, and reviewed in the Chemical News.

J. T.—Mix the crude paraffin with paraffin oil, benzol, or coal naphtha.

Rusticus.—1. We believe Dr. Crum Brown first brought his system of graphic notation before the Royal Society of Edinburgh. 2. The researches are given in the Journal of the Chemical Society. They are not thought much of by chemists. 3. Not except through a member.

W. P. B.—A letter is waiting for you at our office. Please forward your address.

T. D., an Old Subscriber.—Toluidine is a regular article of commerce now; almost any aniline maker, or dealer in coal tar products

F.R.S.; M. Peterson; J. Hallett (with enclosure); Constantine, Count, Zabielo; L. Lloyd; F. O. Ward; Dr. Quarlach; F. H. Hill (with enclosure); Dr. Röhrig (with enclosure); J. Russell; W. Holyoak; F. Roberts; Dr. Letheby (with enclosure); A. Lavenant; Dr. R. Angus Smith, F.R.S.; Captain W. A. Ross (with enclosure); T. Reader (with enclosure); F. C. Samuels; G. F. Symons; Dr. Wilhelms; C. P. Babin; E. J. Quarles; W. Sutton (with enclosure); T. Blair; D. Waldie (with enclosure); J. Wallace Young (with enclosure); A. Smith; Vodrey and Bro.; J. Bray; M. A. Whichelo; W. Rodger (with enclosure); W. H. Dear; W. Salter (with enclosure); J. E. Thorpe; W. E. Walker; P. Jessop; G. Farrer; L. Honer; W. Wilson (with enclosure); W. Kelso; W. H. Hoadley (with enclosure); J. H. Johnson (with enclosure); Longmans and Co.; Spottiswoode and Co.; E. W. Eccles; J. Robarts Penrose; Dr. Adriani; A. C. Bowdler; Dr. Day; Mawson and Swan; H. Rastrick; J. Chalmers; A. Glendinning; J. A. Parkes; L. Wundt; C. E. Gorman; P. Darcy; Rev. A. Rigg; Liebig Extract Meat Company; Dr. Muspratt; J. C. Lee; H. R. Marsden; D. Dawson; Dr. Quesneville; S. Rowland; L. Wundt; Rev. J. T. Burt; J. L. Tgelström; Runcorn Soap and Alkali Company; F. C. Clayton; T. Charlesworth, jun.; Captain W. A. Ross; J. F. Sprague; J. How; J. Walker (with enclosure); Dr. Andrews (with enclosure); V. Cruse; Dr. Herapath, F.R.S.; Dr. W. Kellner; J. Butterfield; R. Nicholson; F. C. Calvert & Co.; Rev. A. Rigg; Dr. Letheby (with enclosure); W. H. Exall (with enclosure); M. Murphy (with enclosure); L. Benzon; J. C. Bell; J. Sprague (with enclosure); E. M. Delf; J. Spiller; W. J. Day; Professor Heaton; C. R. C. Tichborne (with enclosure); F. C. Calvert & Co. (with enclosure); W. Sugg; E. G. Tosh (with enclosure); Magnesium Metal Co.; E. Cetti; T. H. Rowney (with enclosure); Dr. R. Oxland; J. Heywood; Dr. A. Wuth (with enclosure); J. Slessor (with enclosure); R. Talling; R. Rumney (with enclosure); E. D. Day; W. Wilkinson (with enclosure); J. H. Kiel; W. Smith; W. Bailey and Son (with enclosure); J. Hill (with enclosure); E. W. Bartlett; J. Scrivener; H. Hankey (with enclosure); Professor Heaton; J. How; W. Gordon; E. M. Delf; J. Sprague (with enclosure); E. Goodchild (with enclosure); J. Emerson Reynolds (with enclosures); J. Browning (with enclosure); D. Forbes, F.R.S. (with enclosure); Rev. B. W. Gibsone (with enclosure); W. Wood (with enclosure); Archd. Liversedge (with enclosure); C. J. Woodward (with enclosure); Dr. Day; Dr. H. Debus; Dr. Attfield (with enclosure); Dr. Frankland, F.R.S. (with enclosure); J. Horsley (with enclosure); M. Janssen; Dr. E. Rohrig (with enclosure); Messrs. Townsend & Adams, New York (with enclosures); W. Ercot Smith; F. A. Aramayo; O. Coke; J. Dalmeira; H. P. Dobson (with enclosure); Messrs. Johnson and Matthey; J. E. Hamilton; A. Hochstetter; E. Jones; J. Cliff; T. R. Fraser, M.D.; Dr. Sansom; T. W. Lovibond; H. Lowe; Dr. H. Sprengel; K. Rumball (with enclosure); Karl Hoffman; W. Wyatt (with enclosure); Captain W. A. Ross; Dr. F. C. Calvert, F.R.S.; D. Marples (with enclosure); W. Hall (with enclosure); J. A. Wanklyn; A. P. Price; W. Therp, junior; W. E. Drysdale; Dr. Watts (with enclosure); W. J. Day; J. Muspratt & Sons (with enclosure); F. W. Hart; N. Earle; Professor Tyndall, F.R.S.; J. E. Thorpe; H. Bower; Dr. R. C. Moffat; G. Gore, F.R.S. (with enclosure); Rev. B. W. Gibsone, M.A. (with enclosure); J. Heywood; A. Stark; L. Power; F. C. Calvert & Co. (with enclosure); Townsend & Adams; E. A. Parnell; Prior of the Monastery of St. Joseph; G. Lunge; Kingsbury & Co.; Dr. Adriani; C. J. Woodward (with enclosure); P. J. Worsley; Runcorn Soap and Alkali Co. Lim.; S. Rowland (with enclosure); M. A. Gage (with enclosure); Capt. W. A. Ross, R.A. (with enclosure); A. W. Wilson; A. M. Scott (with enclosure); W. Briggs (with enclosure); Dr. Quesneville; S. Scott; Nelson Bros. (with enclosure); Montgomerie & Greenhorne; T. Hill (with enclosure); H. R. Marsden (with enclosure); R. C. C. Lippincott; F. C. Clayton (with enclosure); T. Fisher (with enclosure); Professor Frankland, F.R.S.; Professor W. A. Miller, V.P.R.S. (with enclosure); J. Stubbins; Asher & Co.; T. Sterry Hunt, F.R.S. (with enclosure); J. Walker; W. H. Exall; C. R. A. Wright; W. Exmore; C. Richter; Professor Weltzein (with enclosure); Davenport & Co.; E. C. Hogg (with enclosure); E. P. H. Vaughan; Mottershead & Co.; J. C. Bell (with enclosure); Rudolf Schomberg (with enclosure).

Books Received.—"Philosophical Magazine" for Jan. 1868; "Popular Science Review;" "Hardwicke's Science Gossip;" "First Principles of Modern Chemistry," by U. J. Kay Shuttleworth, London, Churchill; "Braithwaite's Retrospect of Medicine," London, Simpkin & Co.; "American Journal of Mining;" "Scientific American;" "American Gaslight Journal;" "Pharmaceutical Journal;" "A Dictionary of Chemistry," by Henry Watts, B.A., F.R.S., part xiii., London, Longmans and Co.; "The Journal of the Quekett Microscopical Club, London," Robert Hardwicke; "Street Tramways for London," by Charles Mackay, LL.D. London: P. S. King; "First Step in Chemistry," by R. Galloway, F.C.S., fourth edition. London: Churchill & Sons; "Inorganic Chemistry," by Charles W. Eliot and Frank H. Storer. Second edition, revised. London: John Van Voorst; "Le Moniteur Scientifique;" "Journal of Materia Medica;" "American Gas Light Journal;" "Scientific American;" "Etude Descriptive, Théorique et Experimentale sur les Météorites." Par M. Stanislas Meunier. Paris: Aux Bureaux du Cosmos, 7 Rue Perronet près la rue des Saints-pères;" "A Treatise on Frictional Electricity in Theory and Practice," by Sir William Snow Harris, F.R.S. Edited by Charles Tomlinson, F.R.S. London: Virtue & Co., 1867. "Boiler Deposits," by Dr. T. L. Phipson, F.C.S.; "Bulletin de la Société d'Encouragement;" "American Artisan;" "The Oriental Mail and War Office Gazette;" "American Journal of Mining;" "Action of Sunlight on Glass," by Thomas Gaffield; "Scientific American."

[English Edition, Vol. XVII., No. 422, page 14; No. 423, page 25; No. 424, page 37; No. 426, page 61; No. 422, page 14; No. 424, page 37; No. 425, [illegible]; No. 426, page 61; No. 423, page 25; No. 422, page 14; No. 424, page 37; No. 425, page 49; No. 423, page 25.]

THE CHEMICAL NEWS.
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CRYSTALLOGRAPHY AND THE BLOWPIPE.

II.

BY CAPTAIN W. A. ROSS, R.A.

[For I., see *Amer. Repr.* Chem. News, Feb. '68, page 74.]

As Professor Richter and several other operators have found some difficulty at first in blowing the vesicles described by me, I ask your permission to commence this paper with an explanation of their formation.

Method of Blowing Vesicles of Borax, Soda, or Phosphorus Salt.

a. **Borax.**—This being the most cohesive and least deliquescent of the three fluxes, requires no addition to enable a strong vesicle to be blown, which will last for weeks or months. The platinum wire should be twisted into a ring over one of the legs of a pair of the *round* pliers used by bird-cage makers; the ring is then nearly perfect, and should have the diameter of a largish pin's head. The other end is then placed in a holder, the wire heated, and a bead of borax taken up, which should be *perfectly clear* on cooling. This bead is then heated again, and charged with the substance. If a silicate, the bead will be observed to become much less fluid, and to move heavily round under the influence of the O.F.[*] like a thick jelly. After a little practice the operator will find it the best way to hold the ring of the platinum wire nearly horizontal to the table, so that the greater part of the fluid bead hangs downward, because by blowing *upward* through this there is not only less chance of bursting the vesicle, but the colouring matter (if any) will accumulate better round the ring, where the borax is generally thickest. I always now use the *geblase*, or caoutchouc bellows,[†] with the aid of which, in heating the beads, I can easily blow thirty vesicles in a couple of hours, and could make them in one if the minerals or oxides were ready and powdered. The bead should be held in a strong O.F. or R.F., according to the condition in which the substance is required. It should be allowed to cool down to red heat, and then the jet of the blowpipe applied close to, but not touching it, and square to the ring of the wire. Siliceous vesicles (which are, in fact, glass) are easily made, but they require to be annealed by being held near the flame for a short time after, for if suddenly withdrawn, large pieces will crack out of them. As the strength of the blast from a mouth blowpipe does not vary much, the *size* of the vesicle is not under the operator's control, and can only be partially regulated by the quantity and density of matter in the bead. Two beads of apparently the same size and density will, however, sometimes give vesicles of different dimensions, in which case the smaller will always be found to have a greater quantity of the flux round the ring of the wire.

The bead may be charged with the substance until perfectly opaque; for however saturated it may be, the vesicle will always be blown out clear. Even cobalt and manganese only give faint coloured lines of blue and pink over the *vesicle*, however much the *bead* may have been charged, but the exposition of undissolved matter is so delicate in the former that what may have seemed merely a thick opaque solution in the latter, appears in the vesicle as a number of spots or particles of extraneous matter, some of which look formidably large under the microscope. The merest particle of reduced metal is so discernible in this way, that I have amused myself by holding a green bead of the oxide of copper in the R.F. until it was apparently quite clear, and then, blowing it into a vesicle, I invariably found, with a microscope, a particle of metallic copper. I have now a vesicle of chromate of iron two inches long by one wide, made from an "opaque" bead, covered in this way with spots of the undissolved ore.

The vesicles, as made, should be placed in a tray on cotton, and a record *immediately* written of each of them, numbering from the right. If this is omitted, or a vesicle is misplaced, its contents are forgotten, and the only resource is to shake off the re-heated bead, and make a new vesicle.

b. **Soda and Phosphorus Salt** vesicles are made in exactly the same way as those of borax; but a small proportion of silicic acid must be added, without which the soda vesicle cannot be blown at all, and, even then, both of them deliquescing, will not last more than a short time.

I now proceed to record a few observations on the borax vesicles, which, I think, will be found to be based on certain *fixed* principles.

1. A vesicle clouding over with an unctuous-looking white film within an hour or so of being made, and showing (under the microscope) small deliquescent drops outside, may be set down as containing an alkali in considerable proportion, combined with little or no silicic acid. (N. B. I hope to be able soon to distinguish between soda and potash, the former appearing to crystallise in flowers or leaflets, the latter in stars.)

2. A vesicle clouding over with a *dry* white film after a few hours, and not deliquescing at all, or not for several days, contains one of the alkaline earths. Of these *baryta* may be at once recognised by the peculiar blue-white appearance of the new film, which has much the colour of a solution of sulphate of quinine.

3. These films, however slight they may appear at first, are evidently due to the aggregation of minute crystals, which are, in the first instance, not distinguishable by the most powerful pocket lens.

4. After the first day or two, an immense number of similar crystals generally cover the surface of the vesicle, those apparently containing metallic acid salts, having a curious resemblance to the annular rings of a section of exogenous wood, but in no two differing vesicles are these crystals exactly alike. Over these, after the lapse of another day or two, a fresh kind of crystal sometimes appears, smaller and much fewer in number than the first. If I might venture a surmise regarding this phenomenon, it would be that the *first* are crystals of the double borates of the metallic oxides, the *second*, some combination of the latter with CO_2 derived from the atmosphere.

5. A vesicle of *boro-silicate of soda* remains quite clear for several days, and as far as I know yet, does not crystallise.[*] It is therefore the best vehicle I know

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[*] Oxidating flame.—Initials will be used to save space.

[†] This ingenious and portable bellows is the invention of an American student at Freiberg. It is described and figured in Richter's last edition of Plattner's work.—Leipsic, 1865.

[*] This vesicle, made of a mixture of borax with one-third of silicic acid, eventually crystallised after a period of three weeks. The crystallisation is interesting as a type of SiO_2. I apply the term *grammate* to it from its similarity to a series of lines or letters.

for the vesicular exhibition of crystals of substances contained in it.

6. Vesicles of *silicate of soda* deliquesce a few minutes after formation, and those of P. salt in a little longer time, but the most curious phenomena are those exhibited under the magnifying glass by vesicles of silicate of potash, which cloud over and deliquesce as soon as formed, the crystals, scarcely discernible, appearing like small white rings with a black centre; the deliquescent moisture at the edges shrivelling up the vesicle, and advancing on all sides *towards the centre of gravity* like a miniature wave. The crystals of soda silicate appear formed like small white flowers with four petals.

The above may, I think, be depended on as a groundwork for careful examination, but when I came to attempt to reduce my observation to system, making sketches of the crystals as I proceeded, I found, that independently of requiring the pencil of a Redgrave or Millais to copy the beautiful forms examined, I might as well begin to write a perfectly new work on crystallography, every second page of which would require to contain elaborate illustrations! The field is immense, and requires many and careful observers, for although the whole effects are evidently due to the operation of definite laws—

> "A mighty maze, but not without a plan,"

the clues cannot be followed successfully by one or even by few observers. Every metal with its salts appears like a kind of mineralogical kaleidoscope, throwing its crystallisations apparently at random into the most elegant shapes, each of which must be made to yield its atom of information as to the source of all. But when I proceeded to examine crystallised vesicles of the alkaline and earthy silicates, as *albite*, *adular*, *calcite*, *heavy spar*, etc., I could really, with little imaginative aid, fancy myself beholding scenes in fairy land. The beautiful snow crystals pictured in the Chemical News in illustration of Professor Tyndall's lecture, are tame compared to these. Given a candle, a powerful pocket lens, and these vesicles, and you have objects of exquisite beauty, before which the most brilliant gems in the fairest setting of silver or gold must "pale their ineffectual fires." Taking the platinum wire carefully in a pair of fixing pliers and holding the vesicle between your eye (applied to the magnifying glass) and the light, you behold the most delicate tracery of fronds, flowers, ferns, or winter trees, standing out in frosted silver against a flood of golden light. Sometimes the appearance is that of a Cashmere shawl elaborately worked in silver (*calcite*), but the mineral *cerite* seems to afford forms even more exquisitely beautiful than these. I cannot attempt to describe the appearance of the *cerite* crystals, unless sprigs of the maiden hair fern, elegantly posed together and covered with frosted silver on a ground of clear glass, can afford some idea of them.

It may be said these crystals are possibly pretty, but what is the use of them? I answer, that if friendly *collaborateurs* will assist me, I hope to turn them to a very distinct use. Already I can distinguish by means of them with tolerable certainty, an alkali or alkaline earth, isolating one—baryta—and this you will recollect is at present the weakest part of blowpipe analysis. If I had space, I should much like to inform you of some remarkable vesicular reactions afforded by *molybdenite*, which, with the result of ulterior experiments, would appear to place that metal in close relation to the "earths," but I must reserve such remarks for another paper. In the meantime, why should not balls of fluid glass, containing substances in solution, be blown into globes of sufficient tenuity to favour crystallisation, and thus form permanent and beautiful models in illustration of one of the queens of earthly science—crystallography?

Some new and apparently incontrovertible facts deducible from the vesicular reaction observed and recorded by me, would seem to be:—

1. *Every* inorganic substance, chemical or mineralogical, crystallises inevitably from its solution in borax.
2. These crystallisations are *not* isomorphous.
3. Those substances which crystallise soonest are most deliquescent.
4. Crystallisation *always* precedes deliquescence.
5. Alkaline are more crystallisable and more deliquescent than acid salts.
6. There seems to be *two distinct kinds* of crystallisation in nature, having widely differing forms. One, the primary kind, in which every element and every combination of elements has a crystalline form *peculiar to itself;* the other, or secondary, the aggregate of many primary forms, in which the crystals are for the most part isomorphous, as recorded by Mitscherlich. Supposing this hypothesis correct, snow and ice are familiar examples in which the difference of the snow crystals would correspond to some difference in the composition of drops of water of which they are formed.

Whatever may be the value of these deductions, there can be no doubt that in order to describe vesicular crystallisations, a very different nomenclature must be employed from that used in crystallographical works.

I soon found also, from the extraordinary resemblance of most of these new crystalline forms to those of the vegetable world, that the only glossology applicable to them would be one derived from that used in botany. I therefore propose, with great deference, the adoption of the following terms to enable observers to record their observations until some more complete or better system may be enunciated:—

A. *With reference to the Vesicle.*

1. *Diaphanous*, quite transparent (D.)
2. *Diaphanebulous*, slightly clouded, but partially transparent; (D.N.)
3. *Diaphachromous*, clear but coloured; (D.C.)
4. *Diaphanunctuous*, clear, but having a moist or oily look; (D.U.)
5. *Nebulous*, clouded over thickly, crystals not distinguishable; (N.)
6. *Nebulunctuous*, clouded, and having a moist or oily look; (N.U.)
7. *Lumenebulous*, clouded, but crystals distinguishable by transmitted light; (L.N.)
8. *Chromenebulous*, in which the vesicle is at first *Diaphanous*, but colouring matter appears in the crystals afterwards; (C.N.)

B. *With reference to the crystals.*

1. *Zonate*, having rings, like exogenous wood; (Z.) *Ex.* Some of the metals of Group 3 in chemistry.
2. *Areolate*, an aggregation of Zonate crystals, like a tesselated pavement; (A.) *Ex.* Magnesium.
3. *Acrogenate*, fern-shaped; (Ac.) *Ex.* Cerite, etc.
4. *Aciculate*, like needles; (Aci.) *Ex.* Antimony.
5. *Campenulate*, like the flower *convolvulus*; (C.) *Ex.* Soda.
6. *Ciliate*, having fine hairs or fringe at the margins; (Ci.) *Ex.* Lead.

7. *Discoidate*, like a disc of polished steel; (D.) *Ex.* Tungstic acid—in Wolfram.
8. *Dendroidate*, like winter trees; (De.) *Ex.* Silicates of the alkalies.
9. *Discoistellate*, discs having star in the centre; (D.S.) *Ex.* Potash.
10. *Digitate*, fingered; (Di.) *Ex.* Apophyllite.
11. *Filiformate*, thread-like; (F.) *Ex.* Silicic acid.
12. *Florate*, flower-like; (Fl.)
13. *Guttate*, small irregular rings; (G.) *Ex.* Chloride of platinum.
14. *Grammate*, like hieroglyphical letters; (Gr.) *Ex.* Arsenic.
15. *Hypocrate*, salver-shaped; (H)
16. *Macculate*, large irregular rings; (M.)
17. *Palmate*, like the leaves of a palm tree; (P.)
18. *Plumosate*, feather-like; (Pl.)
19. *Reticulate*, a net work of veins; (R.)
20. *Stellate*, like a star; (S) *Ex.* Silver.

It will be seen that among the examples given I have left some deficient. This has occurred precisely from the want I am now endeavouring to supply—a *systematic* arrangement of observation and record. I wrote every one of the above terms (taken chiefly from "Lindley's Botany") with reference to some particular vesicle, but in some cases I did not apply my new arrangement to the vesicle described, and have now therefore forgotten to which vesicle the example refers; in others the crystals were not sufficiently developed, and have altered slightly, as in the case of silica, or I have now placed the crystal under another denomination found afterwards, which I consider more expressive of its appearance. Silica at first appeared like a number of short marks or hieroglyphics scattered over the vesicle at hazard, these shortly after grew into elegantly grouped filaments or threads, having something like the appearance of minute branches or twigs of a tree, and I am therefore half inclined to change the term for silica again to "dendroidate."

It is obvious therefore that it is only by the agreement of different observers, *pursuing the same system of observation and using a similar glossology*, that the real value of these crystallisations as analytical agents will be obtainable. I am all but convinced now, not only that these primary crystallisations are isomorphous, but that the slightest change in any of the constituents of a salt will exhibit a corresponding difference in some part of the primary crystallisation. Thus *caustic* potash combined with silica only, clouds over (*i. e.*, crystallises), deliquesces, and indeed vanishes almost immediately after it is formed. In borax it clouds over unctuously, and shows discoistellate crystals. *Carbonate* of potash clouds over, and deliquesces in a very considerably longer time, and the crystals are also distinctly referable to the term *discoistellate*. *Chloride* of potassium clouds over very shortly also, but there is apparently no deliquescence, and no crystals are distinguishable. The first, second, and third of these vesicles I therefore call *nebulunctuous*, the fourth *nebulous*. Again, the only substance the formation of whose crystals I have yet found to colour the previously diaphanous vesicle is ferrocyanide of potassium. A chemical gentleman and myself imagining that the cyanogen could not possibly have been retained under the heat of the blowpipe flame in charging the borax bead, I blew a vesicle containing carbonate of potash, and a saturation of oxide of iron, but the result was a *diaphachromous* instead of a *chromenebulous* vesicle, and the crystals were different, although both exhibited the discoistellate forms of potash. Thus, if these crystals, as I believe, respond to the very slightest change made in the composition of their component parts, but remain isomorphous *as long as these component parts are inviolably maintained*, it is evident that nothing could be more invaluable to the chemical analyst than reactions so delicate as these, made by the hand of nature herself.

I have now to record a circumstance so remarkable that I almost hesitate to put it on paper, and yet I believe it to be a fact, namely, that most, if not all the metals under Group III. of the chemical arrangement, not only produce, as I stated in my last paper, zonate discs like sections of exogenous wood, but that these rings *actually grow, each in a certain time, exactly as they do in wood, substituting weeks or days for years, so that the age of one of these crystals can be ascertained like that of a tree.*

I will now, with your permission, make a few remarks regarding *molybdenum*. The powdered ore *molybdenite* yielding on several occasions a *nebulous* vesicle in as short, if not a shorter time than the alkaline earths, I was led to examine this metal more closely than I would otherwise have done. Professor Bloxam says in his valuable work on chemistry (page 393), "The bisulphide of molybdenum (*molybdenite*) is roasted in air *at a dull red heat*, when sulphurous acid is evolved, and *molybdic acid* (MoO_3) mixed with oxide of iron is *left*," but both myself and Mr. Charles, chemical assistant at the R. A. Institution at Woolwich, failed to drive off completely the SO_3 from this ore after ten minutes or quarter of an hour of the strongest roasting in a fierce blowpipe flame. I however obtained in a pipe-clay crucible, from this process, crystals of the so-called "molybdic *acid*." On rubbing these crystals with moistened test-papers, they gave neither an acid nor alkaline reaction. I found a similar account of molybdic acid in Miller's Chemistry to that above given, and going further back (to Brande), that Scheele, the discoverer of the metal, had employed the bisulphide (*molybdenite*) in his experiments. It then struck me that the SO_3—supposed to be eliminated—had perhaps combined with ammonia and oxide of molybdenum to form the "molybdenate" of that alkali, and Mr. Charles and I tested the salt so called in the laboratory here, which we found to give a strong sulphur reaction on silver foil, and with barium. I think therefore it would be worth while to reinvestigate the properties of this metal.

The following vesicles last made by me, I have laid in a tray according to their chemical arrangement, recorded in the following manner after my system, and I quote them here that they may serve as a guide to intending observers, the results of whose labours I should be glad to see published in the CHEMICAL NEWS.

Vesicles made Feb. 8, 1868, *in Borax.*

GROUP I.

1. *Ammonium*, Chl., 9th,[*] L.N. (C. 2nd S.)[†] The two forms appeared almost simultaneously.
2. *Potassium*, caustic, 8th, N.U. (D.S.)
3. *Sodium*, carb., 8th, N.U. (C)

GROUP II.

4. *Barium*, cl., 9th, D.N. (   ). Crystals not yet distinguishable by pocket lens.

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[*] Date of crystallisation.

[†] When there are two distinct forms of crystallisation, the *second* in time will be shown by a "2nd" prefixed. When the form gradually assumes a second shape, the ulterior will be placed *under* the first as in mercury.

5. *Calcium*, oxalate, 9th, N. ( ). Crystals not yet distinguishable by pocket lens.
6. *Magnesium*, carb. Vesicle not blown till 15th.
7. *Strontia*, carb. Vesicle not blown till 15th.

GROUP III.

8. *Alumina*, corundium. Vesicle not blown till 15th.
9. *Chromium*, oxide, 12th, D. (S.Fl.)
10. *Cobalt*, oxide, 10th, D.C., (D. Fl.) Crystals not distinguishable by *transmitted* light.
11. *Iron*, oxide, 10th, (D.Z. 2nd S.) A peculiar blue colour by *reflected* light.
12. *Manganese*, oxide, 10th, D.C. (D.Z.) A peculiar blue colour by *reflected* light.
13. *Nickel*, oxide, 10th, L.N. (D.) Appears to deliquesce slightly.
14. *Zinc*, carb., D. (D.S.)

GROUP IV.

15. *Arsenic*, oxide, 12th, D. (Gr.) Appear to deliquesce slightly.
16. *Antimony*, oxide. Vesicle not blown till 15th.
17. *Bismuth*, oxide, 10th, L.N. (D. Fl., 2nd S.)
18. *Cadmium*, oxide, 11th, D. (Fl.)
19. *Copper*, oxide, 10th, L. N. (Fl., 2nd D. Z. C_2).* The first form of crystals transparent—a very unusual thing.
20. *Gold*. No vesicle formed.
21. *Lead*, oxide, 10th, D. (D.Z.Ci.) *
22. *Mercury*, oxide, 10th, D. $\left(\frac{\text{D.S.}}{\text{Fl.}}\right)$
23. *Platinum*, chlo., 10th, D. $\left(\frac{\text{G.}}{\text{Fl.}}\right)$
24. *Silver*, oxide, 12th, D.N. (S.) The crystals in myriads a little larger than those of barium, are distinguishable as the small stars of astronomical nebulæ. The colouring matter round the wire ring is opaline with pink transmitted like the noble opal.
25. *Tin*, oxide, 12th, D. (S.) Crystal stars very few and far between up to date.

Non-Metals.

26. *Silicon*, SiO_2, crystallised after three weeks, D. (De.)
27. *Boron*, sassoline, D.U. $\left(\frac{\text{G.}}{\text{S.}}\right)$ Both forms of crystals transparent.
28. *Sulphur*. Vesicle not blown till 15th.

Although the crystallisation of these vesicles has naturally first and most occupied my attention, I have not by any means given up the idea (of examining their optical peculiarities) with which they were originally made. Although I have not an apparatus for observing them in polarised light, without which it is impossible to draw many valuable conclusions from their diffractive phenomena, I could easily see in forming the vesicles enumerated above, that the light was refracted much more by some than by others, and indeed Dollond's discovery of achromatism was based upon this principle. I found that *tin* and *silver* were almost the only two oxides which yielded a perfectly diaphanous non-refracting glass. *Lead*, *arsenic*, and *bismuth* were highly refractive, and *sulphur* even more so than these. *Chromium* also appeared to give a clear glass where blown out, but its colouring qualities would of course prevent any useful application of it to glass in this way.

Wishing to ascertain, as I had before observed the development of electricity in vesicles, if metals in this condition assume the electro-positive or negative state, and if this state affects their crystallisation, I blew a vesicle of *zinc* at one end of a piece of platinum wire, and one of *copper* at the other. To the ends of another piece of wire I affixed similarly a vesicle of *iron* and one of *tin*. In the first, the *zinc* crystals appeared within a few hours of the *campanulate* order, very perfect, and very considerably larger than those of the single *zinc* vesicle; while the connected copper vesicle had comparatively few crystals, and so small that I cannot distinguish their shape. Similarly, in the *tin* and *iron* series, the *tin* crystals appeared disproportionately enlarged at the expense of the *iron* ones, and neither of them bore much resemblance to the typical crystals of single vesicles. If, then, the voltaic current is actually induced between couples of vesicles under crystallisation, it would appear to be in the reverse direction to that passing between the electrified metals, so that copper in this case becomes electro-positive to zinc. Iron to tin.

I have omitted to mention that different metals produce primary crystals of different magnitudes, to express which the Greek letters α, β, etc., might be used.

Woolwich, 15th February, 1868.

[Since the above was written, I have received (on the 17th February) what I cannot but consider as a confirmation of the hypothesis here advanced. I argued that crystallisation formed from *liquids*, as acid solutions, etc., are of the isomorphous (or secondary) form, because they occupy more time in formation, *i.e.*, aggregation of the primary forms, whereas the germs of crystals formed from fusion by fire, must be produced almost instantaneously on cooling, their growth afterwards being the only matter in which *time* is concerned. To prove this, therefore, I plunged a hot borax vesicle (in which unfortunately I had taken up some oxide of *antimony*) into a tumbler of cold water. There was no apparent change at first, and I laid the vesicle on cotton in the usual way. The same evening, I was delighted to find, on examining it with the microscope, indubitable forms of isomorphous crystals —tetrahedra with bevelled edges, hexagonal planes, and a most remarkable combination of the two crystallographical systems in the shape of a flower like a convolvulus, whose petals were formed of the ends of *prisms*. The *rationale* of the experiment seems to be as follows:—The pyrogenous crystallisation is checked by the plunge into cold water, and the borax being soluble, a liquid crystallisation is commenced. Snow crystals being (although formed from a liquid) of the primary form, would appear to be due to the rapidity with which they are frozen.]

THE MICROSCOPE IN GEOLOGY.

BY DAVID FORBES, F.R.S.

AN interesting paper on this subject, by Mr. David Forbes, F.R.S., appeared in the *Popular Science Review* for October last; from it we condense the following. The original article is illustrated with numerous coloured diagrams of rock sections, as seen under the microscope.

The more searching and exact method of investigation now demanded by the advancing state of geological inquiry, necessitates that the student of that science shall in his researches avail himself of all possible means which the collateral sciences place at his disposal; and, amongst others, of those which can enable him to extend his powers of observation beyond the limits to which his unassisted eyesight can convey him.

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* *Discoisocillate*. Compound terms of this kind can be used conveniently.

[English Edition, Vol. XVII., No. 429, pages 88, 89; No. 427, page 64.]

exception of Sorby's invaluable memoirs on some special points of inquiry, literally nothing has as yet been made public which could even serve as an introductory guide to the geologist who might wish to commence the study of the subject.

In the present communication it is intended, as far as the space at disposal will allow, to attempt a short sketch of some of the results already obtained, in order thereby to illustrate the use of the microscope in similar inquiries.

When applying the microscope to the examination of rock structure and composition, it is necessary to prepare the specimens previously, in order to be enabled to make full use of transmitted light in their investigation.

When in sufficiently thin splinters or laminæ, by far the larger proportion of mineral compounds allow light to pass through them with more or less facility, and amongst these, most silicates, chlorides, fluorides, carbonates, sulphates, borates, and other salts; as well as many oxides, and some few sulphides, sulph-arsenides, etc. On the other hand, all native metals, alloys, and most of their combinations with sulphur, arsenic, antimony, etc., along with some few oxides, and other compounds, are opaque, even when in the thinnest laminæ, and consequently when present, as they often are, in minute quantity in rocks, although sometimes recognisable by their external crystalline form, are not to be distinguished by their optical properties, as in the case of those bodies which, as before-mentioned, are translucent.

When a mineral or rock under examination is entirely in the vitreous state, as, for example, obsidian, it appears when viewed under the microscope, merely as a more or less transparent or coloured glass, presenting, if perfectly in the vitreous condition, no evidence of crystalline or other structure, except, perhaps, traces of the striæ of viscid fusion. It is usually found on inspection, however, that some part of the mass is sufficiently devitrified to allow of its structure and mineral composition being recognised. In some cases, when the glassy appearance presented to the eye would discourage any hopes of structure being discovered, the microscope proved the reverse most conclusively.

In many cases, however, where the specimens are so perfectly in the vitreous state as to show no trace of structure whatsoever, this may be developed by carefully acting upon the surface by gaseous or liquid hydrofluoric acid.

The rock sections may be prepared for the microscope as follows:—A fragment from one-quarter to three-quarters of an inch square, and of convenient thickness, is chipped off the rock specimen in the direction of the required section, and ground down upon an iron or pewter plate in a lapidary's lathe with emery until a perfectly flat surface is obtained. This surface is then worked down still finer by hand on a slab of black marble, with less coarse emery, then upon a Water of Ayr stone with water alone, and, lastly, finished by hand with water on a slab of black marble. This side of the rock is now cemented by Canada balsam on to a small piece of plate glass about 1¼ inch square and ⅛ thick, which serves as a handle when grinding the other side on the emery plate as before. This grinding is continued until the section is so thin as to be in danger of breaking up from the roughness of the Water of Ayr stone with water, and afterwards upon black marble, as before described. The section is now removed from the plate-glass, and mounted in Canada balsam on a slide, covering its upper surface with a thin glass as usual.

The thickness to which such sections need be reduced is, of course, entirely dependent upon the transparency of the rock constituents, and is commonly from 1-100 to 1-1,000 of an inch.

Thin splinters of rocks and powdered fragments, mounted in Canada balsam, may also be examined with advantage, but cannot replace the above-described sections.

The examination of such a rock section enables a mineralogical analysis to be made, even of the most compact and apparently homogeneous rock, and generally leads to the discovery of other mineral constituents previously unsuspected, from their being invisible to the eye, and also, as Sorby has observed, allows those minerals, formed at the time of solidification of the rock, to be distinguished from such as are the products of subsequent alteration.

Arranging rock species according to their structure, it will be found that most rocks fall naturally into one or other of two great classes—

I. PRIMARY OR ERUPTIVE ROCKS.
II. SECONDARY OR SEDIMENTARY ROCKS.

And it will be seen that the microscope is of special value when applied in cases where the external appearance renders it doubtful as to which of these classes a rock may pertain.

The terms *primary* and *secondary* are here used quite independently of geological chronology.

I. PRIMARY OR ERUPTIVE ROCKS.

This class includes rocks which have made their appearance in many, if not in all epochs, from the most ancient to the most recent, from the old granitic outbursts to the eruptions of the now active volcanoes; and if, as is now generally admitted, the earth be regarded as having been once a molten sphere, the consolidated original crust of the globe would pertain to this class of rocks.

Mineralogically they consist of crystallised silicates, with or without free quartz, and usually containing many other minerals in minor quantities, especially metallic compounds, as magnetite, titanoferrite, iron pyrites, etc., which last are frequently present in so minute a quantity as only to be detected by the microscope.

Whatever be their geological age, or from whatever part of the earth's surface they be taken, the microscopical inspection of such rocks shows immediately that they possess certain general and definite structural characters, distinguishing them at once from all other rocks.

The mineral constituents of such rocks are seen to be developed as more or less perfect crystals, at all angles to one another, thereby indicating that the entire mass must have been at one time in a state of liquidity or solution (aqueous or igneous), sufficient to allow of that freedom of motion absolutely essential to such an arrangement of the particles.

The microscopic examination already made of many hundred sections of eruptive rocks, differing widely in geological age and geographical distribution, shows that in all rocks of this class, whether of the most compact, hard, and homogeneous appearance, or occurring in the softest and finest powder, like the ashes and dust frequently thrown out by volcanoes; a similar crystallised arrangement and structure is present and common to them all. Lavas, trachytes, dolerites, diorites, porphyrites, syenites, granites, etc., all possess the same general structural features, serving to distinguish the eruptive rocks as a class from all others.

In the examination and discrimination of the minerals which compose these rocks, especially when close-grained, the microscope is quite indispensable, since without it no such inquiry could be attempted. In these examinations the assistance of polarised light is most valuable; but the space, unfortunately, only allows of a mere mention of its application. In distinguishing dolerites from diorites, when fine-grained, as is often of considerable geological importance, the fibrous structure of the hornblende of the latter is generally so well developed, even when present in very minute quantity, as to distinguish it readily from the augite of the former, which possesses no such structure. Even in the case of Uralite, a mineral characteristic of certain porphyritic rocks, which has the external form of augite, although its chemical composition is that of hornblende, the fibrous structure characteristic of hornblende is distinctly visible. The microscopic structure of some minerals, however, varies with their origin. Thus Sorby has shown that the structure of augite, and some other minerals in meteorites, is quite distinct from that of the same minerals occurring in eruptive rocks, and demonstrates, in a very striking manner, how the study of such peculiarities is likely to clear up the mystery in which the origin of these bodies is involved.

When, as is often the case, especially with translucent, colourless minerals like quartz, leucite, calcite, felspar, etc., the appearance presented under the microscope is alike, their optical properties and the use of polarised light afford the means of distinguishing between them with certainty; as, also, in the event of one substance being present under two forms, as calcite from aragonite, monoclinic from triclinic felspars, etc. In a similar manner, the structure, whether crystalline or vitreous, is determined and valuable information obtained, elucidating the mode of formation and origin of the rocks themselves.

The alterations produced in eruptive rocks, subsequent to their solidification, by the action of water, atmospheric, or other agencies, are studied with advantage under the microscope.

Before proceeding to the next class of rocks, the discovery by Sorby of the numerous minute fluid cavities in the quartz of granites should be alluded to, as proving the great value of the microscope in the study of these rocks. The result of this gentleman's researches[*] proves that granites have solidified at a heat far below the fusing points of their constituent minerals, and at such a pressure as to enable it to entangle and retain a small amount (½ to ⅓ per cent.) of aqueous vapour, which naturally must have been present during its liquefaction. The presence of these fluid cavities in the quartz of granite was immediately blazoned forth as proof positive of the non-igneous origin of granite; whereas if Mr. Sorby's memoir had actually been read, it would have been seen that he had found fluid cavities, perfectly identical with those in granite, not only in the quartz of volcanic rocks, but also in the felspar and nepheline ejected from the crater of Vesuvius; and that the presence of fluid, vapour, gas, and stone cavities, are common both to the volcanic quartz-trachytes and to the oldest granites; and the inference drawn by Mr. Sorby from the results of his researches is that both these rocks were formed by identical agencies. He, therefore, classes them together under one head as rocks of similar origin.[*]

II. SECONDARY OR SEDIMENTARY ROCKS.

The rocks pertaining to this class are all, directly or indirectly, formed from the breaking up or débris, of previously existing rocks. When found in the normal state of sedimentary deposition, they may be conveniently subdivided into:—

1. Rocks formed of the immediate products of the breaking-up of eruptive rocks.

2. Rocks built up of the more or less rounded or angular débris of previously existing sedimentary or eruptive rocks.

3. Rocks composed of mineral substances extracted from aqueous solution by crystallisation, precipitation, or the action of organic life.

1. *Rocks composed of the immediate products of the breaking up of eruptive rocks.*—The little attention paid by geologists in general to the study of Rocks of this class has introduced the elements of confusion into many of their inquiries, and frequently has led to very erroneous opinions being formed as to the nature and origin of certain rocks, which could never have been entertained had microscopic investigation gone hand in hand with field observation.

Rocks of this class may either be of subærial or subaqueous origin; in the former case for example, volcanic ashes may have been deposited as beds on the surface of the land, and afterwards been covered by lava streams poured out over them; or, from having been depressed below the sea level, may have had sedimentary beds of aqueous origin subsequently superposed on them.

When of subaqueous origin, as is by far the most common case, subærial or subaqueous outbursts may force into the sea eruptive rocks, which, being at once broken up into a state of division, more or less fine, in proportion to the greater or lesser cooling power of the water mass in immediate contact, may be spread out into beds by the action of the waves; the texture of these rocks may vary from that of the coarsest breccia down to the finest mud, and, as is usually the case, such deposits may present themselves as alternating beds of coarse and fine character. Upon the consolidation of such formations, rocks are formed, identical in chemical and mineralogical composition with the original eruptive rock from which they were derived, and which, particularly when close-grained, often present an external appearance so like the original rocks as to be frequently undistinguishable from them by the naked eye; in such deposits it is often easy to pick out specimens having all gradations in appearance from the above described down to such as would

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[*] Quart. Jour. Geol. Soc. vol. xiv. pp. 453—500.

[*] These researches tend to confirm the theory of the igneous origin of granite and eruptive rocks in general. It must not be forgotten that by *igneous action*, as used by the Plutonist, was always understood *the action of heat as developed in volcanoes* (the study of which was the basis of the theory itself), in which the agency of water was always recognised. Nearly half a century ago, Scrope not only insisted on the important part played by water in volcanic action, but specially pointed out the difference between such volcanic fusion and ordinary melting. The term hydro-igneous action might not be inappropriate for such, but hydro-thermalism does not at all express what is intended. The idea of a true dry fusion in nature exists only in the brain of the ultra-Neptunist or lukewarm hydrothermalist.

[English Edition, Vol. XVII., No. 427, page 65; No. 428, pages 75, 76.]

No wonder, therefore, if the field geologist finds himself bewildered under such circumstances, and inclined to settle down in the comfortable belief of the transmutation or transition of sedimentary rocks into eruptive, etc., and even the chemist feels puzzled, when he finds that a rock taken out of apparently normal stratified deposits has the same chemical composition with one of undoubtedly intrusive nature. The microscopic examination, however, soon shows that, however similar the external appearance of two such rocks might be, their internal structure is totally different; showing in the primary rock the crystallised structure and arrangement previously described, whilst the secondary rock is resolved into a mere agglomeration of more or less broken fragments of the same minerals constituting the former. In beds formed from the consolidation of volcanic ashes, the microscopic examination occasionally affords evidence as to whether such ashes had been deposited on land or had fallen into water.

2. *Rocks built up of the more or less rounded or angular débris of previously existing sedimentary or eruptive rocks.*—Where sufficiently coarse-grained, these rocks constitute ordinary conglomerates, breccias, grits, sandstones, etc., and are easily analysed by the eye; but if fine, as shales, slates, etc., the microscope must be appealed to, in order to resolve them into their constituent mineral or rock particles, and by this means it will be seen that even the most compact and homogeneous specimens are a mere aggregate of more or less rounded and water-worn grains of quartz; weathered felspar, mica, chlorite, soft and hard clays, clay slate, oxide of iron, iron pyrites, carbonate of lime, fragments of fossil organisms, etc., arranged without any trace of decided structure or crystallisation, even when the highest powers of the microscope are employed in their examination. The physical structure and optical properties of the mineral components enable them, however, to be recognised with great certainty, even when in grains of less than 1-1000th of an inch in diameter.

3. *Rocks composed of mineral substances extracted from aqueous solution by crystallisation, precipitation, or the action of organic life.*—Under this class are included most beds of gypsum, rock salt, and other saline bodies, as well as travertine, siliceous sinter, flint, infusorial slates and earths, limestones, etc., many of which have been as yet but very superficially examined.

In the microscopic investigation of such rocks as owe their origin to the development of organic life, very considerable progress has been made, with corresponding important and interesting results.

As early as 1836, Ehrenberg proved that large rock masses were built up of the carapaces of minute siliceous infusoriæ; and, more lately, Sorby has done good service by his investigation of limestones; these he has proved not to have originally possessed any crystalline structure whatsoever, but to have been deposited as mere mechanical aggregates (aptly termed by him, organic sands or clays) formed of the débris of calcareous organisms, which admit frequently, not only of being recognised, but of having their relative proportions determined. The comparison of the microscopic structure of the organisms in chalk, with those now forming in the depths of the Northern Atlantic Ocean, indicates that there is an immense de-
taceous period; and the same able observer has shown that the reason which certain calcareous organisms are found so well preserved, whilst others had disappeared or become entirely disintegrated, was from the carbonate of lime in the first being in the form of the stable calcite, whilst in the latter it was present as instable Arragonite.

When a calcareous rock has undergone cleavage, the microscope shows a distortion of its particles and organisms, just as in a cleaved slate, though in much less degree; the measurement of such distortion serves as a basis for estimating the amount of compression undergone.

With the exception of having briefly referred to the alterations in igneous rocks, subsequent to their solidification, and the cleavage of sedimentary beds, all the classes of rocks treated of have been considered in their normal or unaltered condition; it remains now to direct attention to the use of the microscope in the study of subsequent alteration or metamorphism of rocks.

Many sedimentary beds become more or less indurated, at points where they are cut through by eruptive dykes; thus the coal-shales and clays of Staffordshire are found altered into a hard rock with conchoidal fracture, or even into porcellanite, when in immediate contact with basaltic dykes. An examination shows no change in mineral or chemical composition beyond the expulsion of the water always contained in such beds, and sections of such rocks are often seen to be quite identical in structure with those of common stoneware made from the same clays, the only difference being that the latter is usually more porous from not having been submitted to the pressure which rocks baked *in situ* would experience.

The alteration of rocks produced by infiltration may or may not be accompanied by chemical changes; thus a section of calcareous grit often shows that the calcite filling up the interstices between the grains of sand has been merely deposited from a solution of carbonate of lime which has percolated through it, and in otherwise unaltered limestones it is common to find microscopic veins of calcspar, due to minute cracks or fissures, filled up in a similar manner. Frequently, however, such infiltration is accompanied by an entire change in the chemical composition of the rock itself; thus the beds of Cleveland ironstone have been proved by Sorby's microscopical researches to have been originally shell limestones converted into carbonate of iron by the action of ferruginous solutions, the fragments of the original shells being still distinguishable in all stages of conversion. In the same manner he has proved the magnesian limestones of the Carboniferous and Devonian ages, as well as the Permian dolomites, to have been originally common limestones, or aggregations of organic débris, the particles of which, by the use of the microscope, can be traced back to their original unaltered state from which they have been changed by the action of magnesian solutions.

The metamorphism of rocks produced by gasolytic action, as, for example, carbonate into sulphate of lime, etc., has, as yet, not been made the subject of microscopical enquiry.

The foliated schists, quartzites, etc., form by themselves a distinct and well-defined class of metamorphic rocks, characterised by structural peculiarities differing from all previously treated of.

This appears to be due to their crystalline development having originated in a solid body, and not from liquefaction; the minerals composing them differ greatly in structure from the same minerals when found in eruptive rocks. Instead of, as in the latter case, presenting themselves in more or less defined crystals, occurring in all positions and at all angles to one another, in the foliated rocks they are developed only in one general direction, not characterised by well-defined bounding planes, but forming a string of drawn-out and irregularly bounded crystalline aggregations.

The microscopic examination of these rocks proves their original sedimentary origin, often showing the contours of the originals and grains, and, as Sorby has pointed out, the existence of ripple-drift and wave structure, peculiar to sedimentary rocks alone. These rocks appear to have been micaceous and argillaceous sandstones, the constituents of which have been recrystallised *in sitû*, owing to molecular action developed in the solid rock.

The quartz of these schists frequently contains numerous fluid cavities, indicating that they have been exposed to a pressure under which the water, always present in more or less quantity in sedimentary rocks, has been entangled and retained during the recrystallisation of the quartz.

The direction of the lines of foliation or crystalline development is that of the lines of least resistance in the rock, which commonly will be the lines of stratification, but in cleaved rocks will doubtless be those of cleavage. Sorby has alluded to this fact by the names of "stratification foliation" and "cleavage foliation."

In conclusion, the author hopes that it may be the means of attracting attention to the subject, and thereby of causing a hitherto almost unexplored field of microscopic enquiry to be more cultivated; and leaves it to his readers to form a correct estimate of the justness of the sneering assertion that "mountains should not be looked at through microscopes."

NOTE ON THE

DETECTION OF GASEOUS IMPURITIES IN OIL OF VITRIOL.

BY ROBERT WARINGTON.

It is essential for some purposes that oil of vitriol should contain neither sulphurous acid nor any of the lower oxides of nitrogen; both of these impurities are met with in some commercial samples of oil of vitriol. After trying several methods for the detection of sulphurous acid, when present in very small quantity, I at last resorted to the following plan, which also admits of testing at the same time for the presence of the nitric oxides.

About two pounds of the oil of vitriol are placed in a bottle, which the liquid half fills; the bottle is then stoppered, and violently shaken for a minute or two. The gases contained in the oil of vitriol are thus washed out by the atmospheric air contained in the bottle. Sulphurous acid is then tested for by introducing into the air space of the bottle a slip of paper coloured blue by iodine and starch; the paper is conveniently held in the bottle by means of a wire and a cork. The bleaching of the paper gives evidence of the presence of sulphurous acid.

The test-paper is best prepared from Swedish filter paper; this is first passed through a well-made solution of starch, and then dried. A slip of this paper is next placed in a weak aqueous solution of iodine, where it remains till it has acquired a distinct blue colour. It is then removed, pressed between blotting-paper, and is now ready for use.

The paper thus prepared gradually loses its colour by exposure to air; it should therefore be used as soon as made. For the same reason its exposure to the gas in the bottle should not exceed two or three minutes; no perceptible change of colour will occur in this time if no sulphurous acid be present. The colour of the paper is also at once destroyed by heat; it cannot therefore be used for testing the gases given off by hot liquids.

The nitric oxides are detected by substituting for the first test-paper one imbued with iodide of potassium and starch. As NO forms NO_2 on contact with air, and N_2O_3 produces the same compound on contact with air and moisture, the presence of either of these three oxides will suffice to liberate iodine on the moist test-paper, and colour the starch. Since sulphurous acids destroys the blue iodide of starch, the presence of an excess of this gas will prevent the detection of the nitric oxides. The nitric oxides are, on the other hand, without effect on the test paper employed for the sulphurous acid. If therefore the sulphurous acid is not in excess, it is quite possible to obtain the reactions of both gases from the same sample of oil of vitriol, and this is no uncommon occurrence with oil of vitriol which has been imperfectly boiled.

After employing for some time the method above described for detecting sulphurous acid, I made experiments with the reaction adopted by the chemists who lately examined the atmosphere of the Metropolitan Railway tunnels.[*] They employed a test-paper containing iodic acid and starch, on which sulphurous acid *produces* a blue colour by the reduction of the iodic acid. This test they mention as capable of detecting in air the presence of 1-100,000th part of sulphurous acid; a smaller quantity than this was perceptible by its smell, but had no effect upon the paper. On examining by this test several samples of oil of vitriol, I found that I could obtain no reaction; although the same samples, when treated as above described, had distinctly bleached the blue paper previously employed. On proceeding to other samples, in which the smell of sulphurous acid was quite perceptible, the iodic-acid-paper gave a distinct reaction. Thus the blue iodine paper was bleached when sulphurous acid could not be recognised by its smell;[†] the iodic acid produced a reaction only when the odour of sulphurous acid was distinct. The following equations, representing the reactions of these two tests, will throw some further light on their relative delicacy—

$$SO_2 + I_2 + 2(H_2O) = H_2SO_4 + 2(HI)$$
$$5(SO_2) + 2(HIO_3) + 4(H_2O) = 5(H_2SO_4) + I_2$$

From these equations it appears that to deoxidise iodic acids so as to liberate a molecule of iodine, requires the presence of five times as much sulphurous acid as is needed for the conversion of a molecule of iodine into hydriodic acid; or, in other words, the bleaching of the blue iodine paper will be effected

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[*] Chemical News, November 8, 1867, p. 239. (*Am. Repr., Jan.* '68, *pp.* 9, 10).

[†] The purest oil of vitriol I have met with, gives some slight odour to the air with which it is shaken; but this odour is not recognisable as sulphurous acid.

second.

In using either of the reactions here described for the purposes of general testing, it is to be remembered that sulphuretted hydrogen produces with each the same effect as sulphurous acid.

ON THE

ESTIMATION OF SULPHUR IN COAL GAS.

BY WM. VALENTIN, ESQ.,

ASSISTANT IN THE ROYAL COLLEGE OF CHEMISTRY.

SULPHUR is known to exist in coal gas in several forms of combination, the principal of which is bisulphide of carbon (CS_2). By the combustion of gas for domestic purposes a certain amount of sulphurous acid is formed, which diffuses itself into the atmosphere of our rooms, together with the steam which is simultaneously generated, and becomes rapidly converted into sulphuric acid by the absorption of oxygen from the air.

Advantage has been taken of this oxidation of sulphur into sulphur acids by combustion, and methods of quantitative analysis have been based upon it. The best known, and I believe the most generally employed method for the quantitative estimation of sulphur in coal gas is that devised by Dr. Letheby, and which was described in the CHEMICAL NEWS of Feb. 14, 1863.—(*Eng. Ed.*)

In my capacity of gas examiner to one of the London gas companies I have had frequent occasion to observe that this method of Dr. Letheby's, which recommends itself, at first sight, by its great simplicity and facility of execution, does not comply with the requirements of a *quantitative* test for sulphur in coal gas, for the simple reason that it never tells, even approximately, *how much* sulphur there really is in gas.

This arises from two causes—viz., imperfect combustion, and consequently imperfect oxidation of the sulphur compounds contained in coal gas; and, secondly, imperfect condensation of the sulphur products of combustion.

Whilst endeavouring to overcome these defects in Dr. Letheby's sulphur test, I first tried various burners that promised to consume the gas and to oxidise the sulphur compounds more perfectly; but I found that I made but little progress, and sometimes obtained even a lesser percentage of sulphur than that given by Dr. Letheby's apparatus. I was more successful in preventing a loss of sulphur arising from incomplete condensation. This loss was, however, not so great as to account for the deficiency in the sulphur indicated by Dr. Letheby's test and that given by more perfect methods, such as the soda-lime process.

Professor Anderson, of Birmingham, who bestowed much attention upon the method now generally employed for estimating sulphur in coal gas,* and who, it would appear, directed his efforts principally towards a more complete oxidation and absorption of the sulphurous products that pass off with the large amount of non-condensable gaseous products of combustion, sums up his results as follows:—

"1. That a single (fishtail) burner, consuming the gas at the rate of 2½ cubic feet in five to six hours,

"2. That in no case can the sulphurous products of the combustion be wholly recovered where condensing receivers open to the external atmosphere are employed. The best arrangement of apparatus set up on this principle loses 40 per cent. of sulphur, and the arrangement given by Dr. Letheby, I find, from the same cause, always entails a loss varying from three-fourths to four-fifths of the bisulphide sulphur in the gas."

At page 49, referring to the corroborative quantitative results obtained by M. Ellisen,* of the Paris Gas-Works, and by Mr. Evans, the engineer of the Chartered Gas-Works, Professor Anderson again states "that by the employment of the 'Leslie' jet and open receivers not more than from one-fourth to one-fifth of the bisulphide sulphur of coal gas can be estimated."

After failing in various attempts to convert the sulphur compounds entirely into sulphuretted hydrogen, by passing the gas together with steam over heated copper, etc., my endeavours were mainly directed to secure complete combustion of the gas, so as to obtain all the sulphur impurities in the form of sulphuric acid, and not as sulphurous acid, as I had observed how difficult, or rather how impossible, it is completely to oxidise a small quantity of sulphurous acid diffused throughout a large amount of gaseous products of combustion, and to absorb it by passing these products through various oxidising solutions.

There is, as is well understood, a definite amount of air required to completely burn coal gas, and to convert it into its two principal ultimate products of combustion—carbonic acid and water (steam). Sulphur compounds are oxidised readily into sulphurous acid; complete oxidation to sulphuric acid, however, is effected with much difficulty only, as will appear hereafter. It appeared to me, then, that a mode of combustion which supplied to the gas that amount of atmospheric oxygen which is requisite to cause complete combustion (or a slight excess of atmospheric air even), and which brought the gaseous particles into the most intimate contact with the oxygen of the air, at a high temperature, during their passage over a highly porous material, such as spongy platinum, known to possess that power in the highest degree, would most effectually accomplish the object in view.

On testing this theory by experiment, I found that complete combustion was effected by causing the gas to pass, together with an adequate amount of air, through a porcelain tube strongly heated in a small Hofmann's gas combustion furnace. Gas and air are mixed just before they enter the tube, and are made to pass slowly over ignited spongy platinum, loosely packed in a platinum cage made of a sheet of fine platinum gauze, which completely fills the tube. With an insufficient amount of air—about three parts of air to one of gas—carbonic oxide is mainly formed, and the sulphur in the gas is converted chiefly into sulphurous acid, part of which resolves itself into sulphuretted hydrogen on passing along with the steam over the ignited spongy platinum—

$$SO_2 + OH_2 = SH_2 + O_2$$

yielding, in fact, oxygen to aid in the combustion of the carbon compounds of the gas.

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* "On Defects in the Apparatus generally used for the Determination of Bisulphide of Carbon in Coal Gas." A reprint from the *Journal of Gas Lighting*, p. 53.

* "Sulphur in Coal Gas." Report by Thomas G. Barlow, C.E., and Albert Ellisen, Chief of the Experimental Works of the Paris Gas Company.

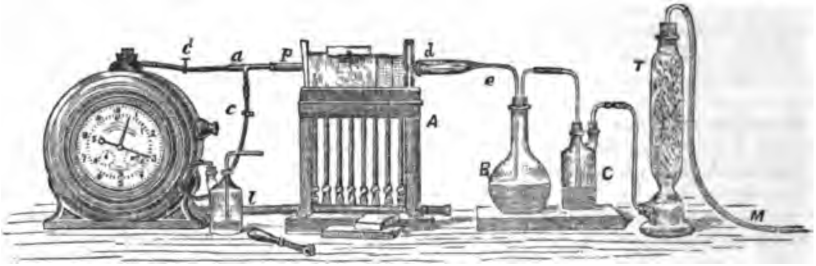
[English Edition, Vol. XVII., No. 428, page 75; No. 429, page 89.]

A vivid combustion takes place in the anterior portion of the tube, just where the mixture of gas and air first comes into contact with the spongy platinum. On passing the gaseous products of combustion through several Woolfe's bottles and towers containing powerful oxidising solutions, such as chlorate of potash and hydrochloric acid, and lastly through a solution of pure soda and through distilled water, I satisfied myself that it is extremely difficult to completely oxidise and absorb the sulphur product of the combustion, since I almost invariably found traces of sulphurous acid in the last tower containing distilled water only. It was evident that no reliance could be placed upon the oxidising and absorbing power of the various solutions. Gaseous sulphurous acid is not oxidised nor retained so readily, when mixed and diffused throughout an overwhelming amount of other gases, as is generally supposed, and it became, therefore, necessary to modify the mode of analysis at first adopted. This is a fact of great importance, since it throws light upon the discrepancies observed by various chemists in the results obtainable by Dr. Letheby's apparatus.

After various alterations I fixed at last upon the apparatus represented in the following figure.

Over the posterior end of the porcelain tube is fitted an adapter-tube, *d*, drawn out and joined on to a narrow glass tube, *e*. The narrow end of the tube is bent at right angles, and fits tightly into a perforated cork, so as to deliver the gaseous products of the combustion into a solution of pure caustic soda, made entirely free from sulphuric acid, by burning sodium under water, such as is obtained now in commerce from the Magnesium Company, Manchester. From 10 to 15 grammes are a convenient quantity to be employed. This solution of caustic soda may be placed into a two-necked Woolfe's bottle, or, as shown in the drawing, into a small flask, B, capable of holding about a pint of liquid, fitted with a doubly perforated cork. If sufficient caustic soda is present in the flask, the whole of the sulphuric acid—for such only is obtained when the combustion is properly conducted—is retained in the first liquid. The greater portion of the sulphuric acid is even found to condense in the adapter-tube, *d*, and may be washed out, and estimated separately.

Fig. 1.



A is a small gas combustion furnace, containing three perforated clay burners in a row or line. Eight or ten rows of such burners suffice. On the low burners of the middle row rests a Berlin-ware porcelain tube, *p*, capable of resisting a high degree of heat and rapid changes of temperature. This porcelain tube, *c*, is 12 inches long, and has a diameter of half an inch. It is best embedded in thin layers of asbestos, spread out in a tinned iron trough, to prevent the direct action of the gas-flames upon it. A platinum tube, made of fine platinum gauze, is made to fit tightly into the porcelain tube. It need not be longer than from 5 to 6 inches. One end is closed by causing the platinum gauze to overlap, and the tube can then be filled with spongy platinum, and when closed at the other end and fastened together at short distances with thin platinum wire, is ready to be introduced into the porcelain tube. A tight fitting cork fixes a narrow glass tube, *a*, drawn out to a point, into the anterior part of the porcelain tube. The latter reaches far enough out of the furnace, and the flow of cold gas and air mixed keeps this part of the tube sufficiently cool to render slight explosions in the anterior part of the porcelain tube and in the glass tube of rare occurrence. Although harmless enough in themselves, they may be entirely avoided by admitting a slight excess of air over that required to completely burn the gas. The gas is supplied through one leg of the short bifurcated tube, and the air through the other. Both gas and atmospheric air are measured by being passed through meters of sufficient capacity to register from 5 to 10 cubic feet of gas per hour. The air may be passed through a solution of acetate of lead, contained in a small two-necked Woolfe's bottle, *l*, to deprive it of any trace of sulphuretted hydrogen before it enters the meter. Compression cocks, *c c'*, regulate the flow of the gas and air.

From the flask containing the caustic soda the gaseous products may be passed through a second flask or through a two-necked Woolfe's bottle, containing a few grammes of chlorate of potash, and moderately dilute hydrochloric acid; and from this into a third, containing a little pure carbonate or caustic soda. Any trace of sulphurous acid is thus oxidised into sulphuric acid, and is retained in the various solutions. And, lastly, the gases are passed through a tubulated cylinder containing a column of a few inches of distilled water, and in its upper part large pieces of broken glass, offering a large moist surface to the gases, and from the top cork, through a bent tube, towards the aspirator M.

I find, however, that when the mixture of gas and air is properly adjusted, *the whole of the sulphuric acid is retained in the first flask*, B, and that the Woolfe's bottles containing the oxidising solutions and the alkali may be entirely dispensed with, retaining for precaution's sake merely one Woolfe's bottle, C, containing a few grammes of pure soda solution, and the condensing-tower, T, as shown in the drawing.

It is, of course, out of the question to drive gases through a series of solutions offering a resistance of a couple of inches of water pressure, without the aid of an aspirator. When a plentiful water supply can be obtained, an aspirator may be used, constructed on the principle of the Catalonian water-blast, with a fall of water of from 8 to 10 feet. Another convenient aspirator, which, moreover, strongly recommends itself on account of its economical use of water (bulk for bulk of air, or nearly so), was devised, some short time ago, by my colleague, Mr. M'Leod, and is described in the *Journal* *of the Chemical Society*, March number, 1867, page 164.

Perhaps the most simple and convenient means of aspirating air consists in using a 5 or 10 cubic feet gas-holder, employed exhaustively. When full the combustion may be temporarily interrupted, till the products of combustion have been discharged from it. Such gas-holders are generally found in large gas-works, and are used for testing gas meters. Thus any loss of water will be altogether obviated.

The flow of the gases is best regulated by means of a gas-tap, connected with the india-rubber tube leading to the aspirator from the condensing-tower.

The combustion must at all times be so regulated as to cause the products of combustion to pass off without showing a peculiar white smoke or cloudiness within the condensing-flasks. This is effected by having about ten times as much air as coal gas. It is quite possible to burn from 0·5 to 0·6 of a cubic foot of gas per hour, and, as only 2 or 3 cubic feet of gas have to be burnt to obtain a sufficient amount of sulphuric acid for a correct estimation, one is enabled to conduct and finish an estimation during a time of the day when the chief consumption of gas takes place—viz., in the evening.

The following tables contain the results of a series of experiments, conducted at the Laboratory of the College, upon ordinary coal gas supplied by the Chartered Gas Company:—

Table No. I.

Date of Experiment.	Cubic Feet of Gas burned during the Experiment.	Amount of Gas burned per Hour.	Sulphide of Barium obtained, in Grammes.	Sulphur per 100 Cubic Feet of Gas, in Grains.
April 16	2·3	0·48	·2822	25·98
17	3·3	0·73	·4240	27·21
18	3·2	0·71	·4680	30·95
20	3·2	0·80	·418	27·92
23	5·	1·10	·627	26·55
24	3·2	0·96	·430	28·45
27	5·8	0·93	·8375	30·58
30	3·9	0·78	·632	34·33
May 15	3·7	0·85	·4623	26·58
17	4·	0·89	·7283	38·56
20	5·	0·79	·5967	25·27
21	5·	0·94	·5806	24·59
	*4·5	1·00	·260	12·23
22	5·	0·95	·5595	24·03
	*6·	0·90	·3902	13·78
23	5·1	1·18	·6285	26·07
	*6·5	0·93	·501	16·32
24	6·5	1·24	·683	24·19
	*7·75	0·97	·6545	17·91
25	7·2	1·07	·875	25·75
28	8·8	1·34	·975	23·48
	*6·5	0·93	·543	17·70
29	4·1	0·75	·5365	28·73
June 27	2·75	0·42	·280	21·60
July 1	3·5	0·64	·5778	34·87
2	3·5	0·58	·347	20·98
3	4·5	0·66	·462	21·62
4	3·25	0·65	·382	24·89
	*4·25	0·70	·2741	13·6

* This mark indicates the analysis made simultaneously by the Letheby apparatus.

Table No. II.

Date of Experiment.	Cubic Feet of Gas burned during the Experiment.	Amount of Gas burned per hour.	Sulphate of Barium obtained in Grammes.	Sulphur in 100 Cubic ft. of Gas in Grains.
July 5	2·75	0·51	·2705	20·00
	†2·85	0·88	·3814	28·35
6	3·	0·64	·334	23·50
	†3·02	1·00	·3818	26·79
8	3·5	0·56	·4525	27·37
	†3·4	0·97	·5224	32·56
9	†4·6	0·84	·6971	32·14
10	3·	0·60	·3278	24·05
	†4·8	0·91	lost	—
11	3·	0·50	·3457	24·32
	†5·1	0·98	·7770	32·25
12	3·	0·53	·3585	25·32
	†5·	1·00	·7057	30·00
13	1·5	0·52	·2203	31·09
	†2·65	0·93	·4252	33·95
Sept. 18	†5·0	0·83	·7422	31·46
20	2·55	0·50	·353	29·34
	†4·5	0·90	·7167	33·74
21	1·3	0·40	·1815	29·58

† This mark indicates the analysis made simultaneously by the soda-lime process.

On half-a-dozen occasions an experiment was carried on simultaneously with the apparatus devised by Dr. Letheby, and it will be seen that the results are considerably below those which were obtained at the same time by the new method; by far not so low, however, as Professor Anderson states the loss to be.

There can be little doubt that the so-called *lime process* is by far the most perfect method of estimating sulphur in coal gas which can be found. It consists in passing the gas over lime (best soda-lime) loosely packed in a combustion tube of hard glass, and heated strongly from the outside in a gas combustion furnace.

Unless, however, *pure* lime and *pure* soda (free from sulphuric acid) are used, little reliance can be placed upon the process. The glass, moreover, is acted upon to a disagreeable extent, silica being dissolved out; and unless the gas be sent through the tube at a very slow rate, much carbon is deposited. The sulphur products also require oxidation after being dissolved out, and it needs no little experience in chemical manipulation to steer clear of all these drawbacks to an otherwise excellent method.

In order to check the above results by those obtained with the soda-lime process, I prepared some perfectly pure soda-lime by calcining marble, and slaking the pure caustic lime so obtained with a solution of pure caustic soda, and I thus succeeded in getting a soda-lime, which was perfectly free from sulphuric acid. In order to avoid the action of the alkalies upon the glass, I used a narrow-bore gun-barrel, coated over with fire-clay made into a stiff paste by means of starch solution, or solution of British gum, and dried, previous to being placed on the furnace. In this manner I succeeded in getting, to a great extent, over the above-described shortcomings of the method.

It will be seen from Table II. that the results obtained by the soda-lime process were invariably somewhat higher than those obtained by combustion of the gas over spongy platinum. I convinced myself that this arose from a slight loss of sulphuric acid, on account of its being retained by the spongy platinum, and condensed on the inside of that part of the porcelain tube nearest to the adapter-tube. It is, therefore, advisable to invariably wash out with distilled water both the porcelain tube and the cage of spongy platinum. The latter appears to retain the sulphuric acid with great pertinacity, and it requires repeated digestion with hot distilled water, slightly acidulated with hydrochloric acid, before the sulphuric acid can be dissolved out entirely. The cage of spongy platinum must be dried and ignited before it is put again into the dry porcelain tube.

If it were not for the difficulty and extreme tediousness with which the soda-lime process is attended, there can be little doubt that it would deserve the preference over any process known at present for estimating sulphur in coal gas.*

It has been my endeavour to provide the practical gas engineer with an apparatus for estimating the sulphur in gas which is easily manageable, and which requires but little supervision when once set going; also to obtain the sulphur at once in the shape of sulphuric acid without the aid of oxidising agents, such as nitric acid, bromine or chlorine water, chlorate of potash, and hydrochloric acid, solution of hypochlorites, etc. The soda solution contained in the flasks is simply rinsed out into a beaker. The adapter-tube, as well as the porcelain tube and spongy platinum, are carefully rinsed out with distilled water; the liquid is acidulated with hydrochloric acid, the whole heated to ebullition, and the sulphuric acid precipitated by means of chloride of barium as sulphate of barium. The precipitate is filtered off, washed, dried, and weighed in the usual manner.

Since writing these lines for publication in the *Journal of Gas Lighting*,† I have made further experiments with a view of combining the advantages of the combustion method by means of spongy platinum with those of the soda-lime process. By introducing a few grammes of pure soda-lime into a short platinum tube, about 4 inches in length, made of thin sheet platinum, and placing the tube so charged into the porcelain tube so as to cause the gas and air first to pass over the spongy platinum, and then over the ignited soda-lime, I succeeded in fixing the principal amount of the sulphuric acid produced by the combustion of the gas as sulphate of sodium and calcium. I have, as yet, not been able to fix and retain the whole of the sulphuric acid, within the porcelain tube, so as to dispense entirely with the solution of pure caustic soda in flask B., but have little doubt that by substituting a platinum tube for the porcelain tube, and by employing a somewhat larger amount of soda-lime packed directly into the platinum tube, so as not to give to the gaseous products of the combustion a chance of passing off between the porcelain tube and the small platinum tube, by means of which I now introduce the soda-lime into the porcelain tube, without being brought into contact with the alkaline absorbents, I shall succeed in retaining every trace of the sulphuric acid formed.

I subjoin a few results obtained by this modified process of combustion.

Date of Experiment.		Cubic feet of Gas burned during the Experiment.	Amount of Gas burned per Hour.	Sulphate of Barium obtained in Grammes.	Sulphur in 100 Cubic ft of Gas in Grains.
January	28th	2	·36	·352	37·10
	29th	3	·52	·474	33·42
	31st	2·8	·50	·503	38·05
February	3rd	3·3	·55	·522	33·52

In the last experiment the sulphuric acid was estimated separately in the portion of liquid derived from the soda-lime, and the washings of the spongy platinum; it amounts to 29·39 grains, whilst the sulphuric acid collected in flask B., by means of solution of pure caustic soda, amounted to 4·128 grains or 14 per cent. of the total sulphuric acid formed.

The advantage of merely having to dissolve out the alkali with dilute hydrochloric acid without having to remove the cage of spongy platinum, is quite obvious, and as the soda-lime is obtained perfectly free from carbon-particles or from sulphide of calcium or sodium, the solution can without any previous oxidation or filtration be precipitated directly with solution of chloride of barium.

I hope in a future communication to be able to give you the results of such modified combustion in a platinum tube.

It is obvious that the process of combustion over spongy platinum is applicable to other gaseous mixtures containing sulphur compounds, such as the volatile products, which escape during the process of incineration of various vegetable or animal matter, containing sulphur and phosphorus in combination with albuminoids, as well as in that of metallic sulphates and phosphates. There is, at present, no process known by which the *volatile* sulphur and phosphorus in albuminoid substances, can be ascertained with anything like satisfaction, *independently* from the sulphur and phosphorus, which is determined in the ash. I have, before this, tried slow combustion of such organic bodies, *ex gr.*, wheat, flour, coal, etc., etc., in a current of air or oxygen, passing the products of combustion into bromine water and pure alkali, and obtained results which lead me to think that our knowledge of the amounts of sulphur which is present in grains, for instance, is very imperfect, and that a reliable process for the estimation of the albuminoid sulphur and phosphorus, in contradistinction to the sulphur and phosphorus present as sulphates and phosphates, would be a great desideratum.

I hope shortly to be able to throw some light upon this important subject.

REMARKS ON THE VOLUMETRIC ESTIMATION OF PHOSPHORIC ACID.

BY CHARLES F. BURNARD, F.C.S.

Having been for a considerable period engaged in the analysis of phosphatic substances, raw and manufactured, I send for insertion in the pages of the Chemical News, some of my experiences in the pursuit of the volumetric process. I have confined myself almost entirely to the use of nitrate of uranium in the well known manner, which need not here be described; but in the following of which, according to the published methods, several precautions are necessary; while often with the greatest care as to uniformity of volume in samples tested, it is frequently doubtful when the point of colouration is obtained. For (say) in two determinations of the same sample made side by side, it is seldom that complete uniformity of results is obtained. Much depends on the size of the drop falling into the little pool of ferrocyanide, something in the manner in which the said drop falls, while in all cases *time* is an essential element in the question. If testing be continued, as is usually directed, until the brown colouration is evident, the result will be far too high. To prove this, let the operator in reaching the desired indication, cover over his slab (to prevent drying) and

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* The results obtained by the lime process by M. Albert Ellissen, of the Paris Gas-Works, and given on page 16 of the reports on the sulphur compounds present in coal gas, by Mr. Thomas G. Barlow, C.E., and M. A. Ellissen, differ so widely one from another that I am inclined to think there must be some error. I have always found the amount of sulphur obtainable by the soda-lime process, as well as by the combustion process described, to vary but little from day to day.

† January 7th, 1868.

[English Edition, Vol. XVII., No. 429, pages 91, 92; No. 430, page 99.]

leave it so some hours, say until next morning, when he will find the point to be five or six units below that indicated over night. But even now there is much uncertainty, for say he has (as he should always have) tried two side by side, he will frequently be perplexed in making his decision. I am happy, however, in being able to record a method by which all doubt is dissipated, and much greater accuracy obtained.

If the composition of the substance (say a manure) be quite unknown to me, then I make a preliminary examination, which soon shows the probable range of the per cent. of its phosphoric acid. But in general this is unnecessary. I use a plain porcelain slab, pits or indentations for the pools being objectionable, as hindering due access of light to the body of the pool. To prevent flowing about, a ring of cork, giving a clear space of ⅞ of an inch, pressed on some hard tallow and then on the slab, leaves a faint but effectual wall of grease.

My method may be best explained as follows, giving an actual determination by way of illustration. Three portions of the same solution, being each 100 septems, were taken and tried side by side on the same slab.

No. 1.—26	28	30	32	34	Septems.*
No. 2.—25	27	29	31	33	"
No. 3.—24	25	26	27	28	"

Now, at the conclusion of the actual testing, not one exhibited the slightest trace of brown colouration; they all appeared precisely alike. They were then covered over and left until the morning, when one only, viz. 34, showed the red brown colour, and that as an intense bright eye in the centre of the pool.

Now, following out my new plan, the slab was carefully put on a levelled stand before a fire, and the spots dried by radiant heat falling on their surfaces. Gently drying in this way being preferable to any other, for rising from below the heat disturbs the settlement of the precipitate. When dry, and the slab just warm, water was carefully dropped on each spot, so as to dissolve the dried-up ferrocyanide, when as if by magic, although on the dried slab there was not a trace of brown visible, the truth was revealed, and the reading became—

No. 1.—30 stood as the number.
No. 2.—29 do. do.
No. 3.—29 do. do.

At the time of testing there was no exhibition of colour; next morning 34 stood revealed; but on drying and redissolving, as explained, 29 was unquestionably the number. It is obvious that while a precipitate may be so slight as to render the colouration of a small pool difficult, yet by its settling down on the white slab it is immediately revealed on dissolving away the crust covering it.

Side by side with the above, has been tried another volumetric process, of my own devising, at least new, so far as I am aware of. It is exceedingly simple, and has afforded satisfactory results. Of course it is not suitable in all cases, but may be applied to the determination of the phosphoric acid, in the great majority of the so-called superphosphates; its value being increased, moreover, by the fact that it necessarily involves the estimation of the free acid in the manure. Suppose a superphosphate made in the usual manner; in such a manure the bone phosphate may be measured by the quantity of sulphuric acid employed in its solution. I extract all that is soluble in water from 100 grains of the manure, and divide it into two equal volumes of one thousand septems each, in beakers of the same dimensions. Into one I drop a standard solution of soda from a burette, when, as is well known, a precipitation of bone phosphate occurs; this, however, on gently moving with a stirrer is re-dissolved; and I continue to drop in until there is a faint trace of a permanent precipitate, which may be the better detected by comparison with the other volume in the second beaker. When sufficient soda has been added, then after duly noting the number of septems employed, an additional septem may be dropped in, when a decided milkiness and agitation will be manifest. The number of septems thus employed is the measure of the free acid existing in the manure. A little practice will enable the operator to very nicely determine the point of incipient precipitation. I now throw in a piece of litmus paper, if blue it instantly becomes red, and then continue the soda dropping until the red litmus becomes nearly blue. A few minutes' repose will allow sufficient time for the precipitate to somewhat settle down, leaving a clear space above; into this a drop of soda solution may be carefully let down, when, if further precipitation occurs, more soda may be added, the whole stirred, and allowed again to subside. In practice it is found that the litmus should be brought to a decided but not to a deep blue. Now, the further volume of the standard soda solution employed is the measure of the sulphuric acid economically employed in the manure, and is therefore the measure of the amount of the phosphoric acid in solution. In my own practice I have worked out a number, by which, on multiplying the number of septems of the standard solution of soda used, there is at once obtained the equivalent of the phosphoric acid in the shape of tribasic phosphate of lime. In comparative determinations, this process has given me results nearly constantly one half per cent. too low.

Compton Gifford, February 24th, 1868.

METHOD FOR THE

DETERMINATION OF SILICON IN IRON AND STEEL.*

BY V. EGGERTZ,

PROFESSOR AT THE SCHOOL OF MINES, FAHLEIN, SWEDEN.

Everybody who has been engaged in the analysis of iron and steel is well aware how very uncertain the determination of silicon becomes when the method hitherto used for its separation in the form of silica is followed, because not only cast iron, but also bar iron and steel, is never found absolutely free from slag intermingled with it. This slag is decomposed by the ordinary method of dissolving the iron in acids, and its silica then augments the amount of silica formed from the silicon contained in the iron or steel; accordingly, too much silicon is obtained when the ordinary method has been employed, as nearly the whole of the silica remains unacted upon, a very small portion only going into solution.

The same thing cannot be said of certain sorts of cast iron, but these sometimes contain blast-furnace slag. In the collections of the Mining Institution at Fahlein are some specimens of spiegeleisen which evi-

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* In the above each number is intended to represent a thin pool of ferrocyanide of potassium, of about ¼ inch diameter; and also in each case the number of septems of nitrate of uranium employed.

* From *Engineering*, July 24, 1868. Translated by C. P. Sanberg.

dently contain particles of slag; at the same time, pig iron containing slag may be considered as rare.

It ought also to be mentioned here, that, according to the *Berg- und Hüttenmannisch Jahrbuch*, vol. xi., page 289, crystallised silicon has been found in crystallised cast iron from Krain, in the form of small silvery plates, which were neither acted upon by boiling aqua regia nor by ignition in oxygen gas; but they were converted into silica by fusing with carbonates of potash and soda.

Crystallised silicon is insoluble in hot solutions of carbonate of soda, but soluble, with development of hydrogen, in hot solutions of caustic potash, and also in hot hydrofluoric acid. In working at the determination of silicon in cast iron at the Mining Institution, there has never been occasion to suspect the presence of crystallised silicon. Cast iron which had a thin white pulverulent coating of silica on its surface, has been sometimes observed.

After fruitless efforts to dissolve iron in highly diluted organic or inorganic acids, which should have no effect on the refinery slag, such a solvent was finally discovered in bromine, which, when mixed with water, dissolves the iron without the slightest action on the accompanying slag.

But as experimenting with bromine in large quantities is very disagreeable, trials were made to use iodine instead; and this, like bromine, has been proved to have no effect on the slag, nor on the oxide or proto-sesquioxide of iron, or proto-sesquioxide of manganese.

At the same time bromine dissolves iron quicker than iodine, and is, perhaps, more easily obtainable in the requisite state of purity.

Neutral chloride of copper may also be used as a means of solution, if copper is not precipitated.

Moreover, as continued experiments have shown that a solution of carbonate of soda can separate finery slag from the silica, which has been formed by the use of iodine or bromine on the silicon contained in the iron, the following method for the determination of silicon and slag in bar iron or steel has been used and considered successful; the same method may be employed for cast iron, because blast-furnace slag, when such is found, is not perceptibly changed by iodine or bromine, nor by solutions of carbonate of soda.

Three grammes of bar iron or steel which has passed through a sieve of 0·2 of a line at the most (the filing or boring must be made with great precaution, so that no scale nor the least trace of the file may get into the sample, and so affect the results); 15 grammes of iodine are added in small portions at a time to 15 c.c. of water in a beaker of 100 c.c. capacity. The water must be previously boiled to expel the air, which would otherwise oxidise the iron. The iodine is stirred in the water with a glass rod, in order to get rid of the air which has accompanied it, and the floating iodine and iron particles are allowed to sink.

The beaker with the iodine * and water, which is kept covered with a watch-glass, is cooled in ice water before the iron is put in, and during the solution it is kept at the temperature of 0° C. For the first few hours it must be well stirred every hour, or oftener, with a glass rod, but afterwards not so frequently.

By means of the low temperature and the careful admixture of the iron (by which heat is prevented), the solution may be performed without the least development of gas, and the iron has less inclination to become oxidised by the air at this low temperature.

By pressure, and by agitating with the glass rod, the solution of the iron particles which collect at the bottom of the beaker is much facilitated; but if no iron is visible, the beaker may be kept at an ordinary temperature, or, preferably in ice water. If some of the solution has risen, and dried up on the sides of the beaker or on the glass rod, it must be well moistened with the same solution before the water is added.

About 30 c.c. of water, which should be very cold in order to prevent the formation of basic salts, are added to the solution; it is then well stirred, left to settle, and the fluid with the lighter particles of graphite is poured into a filter of 2 in. diameter; the filtration is kept up without interruption until there remains only a somewhat heavy dark powder of slag, etc.; at the bottom of the beaker about 5 c.c. of water, with a few drops of hydrochloric acid, are now poured in and stirred with the glass rod; if hydrogen gas is given off, it is an indication that there is still some metallic iron undissolved.

The acidified water is quickly poured on the filter in order not to act on the slag. If a development of gas is perceived, a little iodine, with carbonate of soda and water, is added for the complete solution of the iron, and the residue is thrown on the filter and washed with cold water, until a drop of the filtrate gives no reaction with a solution of 0·2 per cent of ferrocyanide of potassium contained in a small porcelain crucible. Iron solutions containing 0·00001 gramme of oxide of iron per c.c. show in this way very distinct reactions, particularly if a drop of nitric or hydrochloric acid be added. The filtrate is evaporated to dryness, in which operation some of the iodine is sublimed away. Thirty c.c. of hydrochloric acid, 1·12 sp. gr., are then added, and it is again evaporated in order to obtain the silica which may be dissolved in it. When intending to estimate the amount of graphite, it must be washed only with water, because hydrochloric acid would dissolve the slag.

The filter, previously dried and weighed, is again dried and weighed when containing the precipitate. It is then ignited, and the residue weighed. After ignition, the residue is boiled in a solution of soda, in order to extract the silica, and weighed. It should be observed that some part of the silica which has been formed from the silicon in the iron may possibly unite with the slag during the drying and ignition. In consequence of this, it is difficult to extract it by means of a soda solution, whence this method is not to be recommended in exact determinations of silicon.

When using bromine as a solvent, there must be taken 6 c.c. to 3 grammes of finely powdered iron or steel with 60 c.c. of water, which has been previously boiled, and cooled to 0° C.; and this temperature preserved by placing the beaker in ice water until the solution is complete, which usually takes place in two or three hours; it is cautiously stirred once or twice with a glass rod; if stirred hastily, the solution proceeds too violently. The solution is placed on a table or in ice water, and stirred with the glass rod now and then. The further operations are conducted in the same manner as when using iodine. Bromine is preserved

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* The iodine should not leave any residue when exposed to a high temperature. Impure iodine may be purified by sublimation in the following manner: it is placed on a large watch-glass resting on a plain glass plate, and heated on a sand-bath to 107° (the melting point of iodine), a plainly polished beaker is inverted over the watch-glass, and on this the iodine is condensed. The purity of the iodine may be tested by dissolving in it 3 grammes of iron containing a small but accurately determined amount of slag; if these results agree, the iodine may be considered fit for use.

[English Edition, Vol. XVII, No. 430, pages 100, 101.]

under water, and is taken up by a pipette, which is introduced into the bottle, the upper end being kept closed by the finger.

When it is preferred to dissolve iron or steel in pieces, instead of in powder, it may be done; but in this case it is not necessary to place the beaker in ice water, as the metal is less violently acted upon in this form. Several days are required for the solution; the iron, and particularly the steel pieces, must be kept clear from the graphite which adheres to their surface.

In experiments using the solution at the temperature of 40° C. for iodine, and 25° C. or 30° C. for bromine, it occasionally happens that yellowish-brown basic salts have formed; therefore this temperature must not be used, but the solution of iron ought to be operated upon at a temperature of 0°.

In order to determine the silica (formed from the silicon in the iron) and slag, the filter, which contains graphite (in combination with iodine or bromine and water), silica, and slag, is unfolded, whilst it is still wet, on a watch-glass. The contents are washed away from the filter (which ought to rest only upon one-half of the filter whilst in the funnel) with a very fine jet from a wash-bottle (so as not to obtain too much water) into a platinum or silver crucible of the capacity of 30 c.c. The loosening of the mass may be facilitated by a fine paint-brush. The water in the crucible is evaporated to about 6 c.c., 3 c.c. saturated solution of carbonate of soda, free from silica, are added, and the crucible put in the copper ring in a water bath, the hole being large enough to allow the crucible to project ¼ in. above it. It is kept in the boiling water 1 hour, during which time the liquid is stirred two or three times, and the insoluble mass crushed with a platinum spatula. The liquid is carefully poured from the insoluble mass on to a small filter, and to the mass in the crucible is added 1 c.c. of saturated solution of carbonate of soda and 2 c.c. of water. When this has been boiled 1 hour * the whole contents of the crucible are thrown on the filter and washed. The solution of silica in soda is acidified by hydrochloric acid, and mixed with the iron solution, and the whole evaporated to dryness on a water bath. When the solution attains the thickness of ordinary syrup, it is stirred very often with the glass rod, until the mass becomes a dry powder, and heated until the smell of hydrochloric acid has nearly gone off; the beaker is then placed in boiling water for 6 hours, 15 c.c. of hydrochloric acid of 1·12 sp. gr. are then added, and the beaker left on the water bath 1 hour. When the red powder is entirely dissolved, 50 c.c. water are added; and when no crystals of chloride of iron are visible, the solution is thrown on a filter and washed with cold water, warm water forming basic iron salts, which make the silica appear red. The filter containing the silica is dried and ignited in a porcelain crucible, gradually increasing the temperature to a full red heat, and weighed; † if silica is coloured red by oxide of iron, a little hydrochloric acid, 1·19 sp. gr., must be poured into the crucible.

(To be continued.)

MR. RODWELL ON PHLOGISTON.*

The theory of Phlogiston is invariably regarded as a distinct development, unconnected with any previous theory, and uninfluenced by any prior mode of scientific thought. The object of Mr. Rodwell in the paper, of which we here give a short abstract, is to prove that "the theory of phlogiston was not the result of a sudden development; it did not owe its existence to an intellectual exploit, but it arose by a process of evolution, and by a gradual *modus* of development.

In Section 1. ("*Of the subtilis ignis of the Ancients*") the author shows that from the earliest times philosophers recognised a subtle fire innate in matter, and distinct from ordinary fire. The opinions of Zeno, Chrysippus, Lucretius, and others, are quoted to this effect. In the four element theory *fire* ("under which term was included light, the heat inherent in all bodies, flame, incandescent bodies, together with lightning, and all visible manifestations of electricity") was regarded as the *anima*, while air, water, and earth together constituted the *corpus*. Passing on to the Middle Ages (Section 2. "*Of old Chemical literature and of the significance of the terms sal, sulphur, mercurius, as employed by mediæval chemists*") we find the four element theory existing nearly the same in form, but somewhat modified in name, in the three chemical principles, Sal, Sulphur, Mercurius. These are *principia*, not (as is too often imagined) *corpora*; "they are αναλογα—representative bodies, types of classes, type of qualities." The fire of the four element theory was included in *sulphur*, the principle of combustibility. Into this section an account of some old typical chemical works is introduced, together with some remarks upon the rise and growth of the system of chemical symbolization.

Section 3 treats "*Of the supposed nature of fire prior to the rise of the theory of phlogiston; specially of Descartes' 'Materia Cœlestis,' and of Hooke's Theory of Combustion.*" It is here shown that from early times the idea of intestine material motion has been connected with heat. The Cartesian philosophy is discussed, and a connection traced between the *Materia Cœlestis*, and the *subtilis ignis* of earlier writers. The extended Cartesianism of Lemery is touched upon, and the following passage (which bears upon the history of thermo-dynamics) is quoted from his *Cours de Chimie*, published in 1675:—"But because there may be some difficulty in conceiving what is meant by little igneous particles ("*corpuscules ignées*"), I do understand by them a subtle matter, which, having been thrown into a very rapid motion, still retains the aptitude of moving with impetuosity, even when it is inclosed in grosser matters; and when it finds some bodies which by their texture or figure are apt to be put into motion, it drives them about so strongly that their parts rubbing violently against each other, heat is thereby produced."

Section 4 treats "*Of the ideas regarding the calcination of metals which prevailed prior to the rise of the theory of phlogiston.*" From this we learn that Glauber suggested that the increase of weight observed in some metals after calcination might arise from the coagulation

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* 0·1 gramme of ignited and pure silica obtained from analysis is dissolved in the above manner in 6 c.c. of a saturated soda solution and 12 c.c. of water. If any residue is observed after the second boiling, this arises from some impurity which has united in small quantities with the silica, rendering it insoluble. When strong hydrochloric acid is boiled with this insoluble silica, it may afterwards be dissolved. When the solution from the 1 gramme of silica is diluted with water to the volume of 50 c.c at the ordinary temperature, it has no tendency to come into the form of jelly. Quartz powder is dissolved by the preceding method, but very slightly, but ignited titanic acid and finery slag are not acted upon, and the tersilicate slag from blast furnaces but very little.

† When the silica is quickly exposed to a high temperature, a considerable loss may arise from the spirting of the water combined with the silica. Silica dried at 100° C. has been proved to obtain 1 equivalent of water to 3 equivalents of silica, that is, about 6 per cent. of water, which is lost by a strong ignition.

* "On the Theory of Phlogiston."—*Philosophical Magazine for January*, 1868.

signed the gain of weight to the absorption of "extinguished flame," or, as he otherwise expresses it, of "*igneous particles;*" and Lemery, to the assimilation of "*corpuscules de feu.*" These views were in all cases published before Stahl wrote on phlogiston.

In Section 5 we have an account "*Of Becher and Stahl, and of the rise and development of the theory of phlogiston.*" The writings of Becher are here discussed, and it is shown that he has used the word *φλογιστον* solely in its original adjective sense; while "Stahl converted Becher's *φλογιστον* into a substantive, and applied it to designate the *materia ignis*, so often spoken of in the works of former writers on chemistry; and at the same time he endued it with certain extended functions, many borrowed from Descartes, some added by himself.

Phlogiston was defined by Stahl as "*materia aut principium ignis, non ipse ignis,*" and was conceived to be "a very subtle matter, capable of penetrating the most dense substances; it neither burns, nor glows, nor is visible; it is agitated by an igneous motion (*igneo motu*), and it is capable of communicating its motion to material particles apt to receive it. The particles when endued with this rapid motion constitute visible fire . . . . . The igneous motion is '*gyratorius seu vorticillaris.*' . . . . Heat is an intestine motion of the particles of matter." As an almost invariable rule the expression '*loss of phlogiston,*' which so frequently occurs in the works of the Phlogistians, means in our language, *combination with oxygen;* while "*gain of phlogiston,*" or '*assimilation of phlogiston*' signifies *deoxidation.*

The sixth and last Section treats "*Of the Syncretistic nature of the theory of phlogiston,*" and in this the author summarises the matter of the preceding sections, portions of which summary we give below *verbatim.*

"Phlogiston was a new name for an old principle. We have seen that the idea of the existence of a subtle fire innate in matter has pervaded physical philosophy from the earliest times. *Phlogiston* was another name for the "*pure fire*" of Zoroaster; the *ατεχνεκον πυρ* of Zeno; the "*subtilis ignis*" of Lucretius; the "*elemental fire,*" "*astral fire,*" "*sulphur*" or "*sulphureous principle*" of the chemists; the "*calor cœlestis*" of Cardanus; the "*sideric sulphur*" of Paracelsus; the "*materia cœlestis*" of Descartes; the "*terra inflammabilis*" of Becher. The functions of this entity had been varied by different thinkers, almost as much as its name, until Descartes gave them accurate definition. The theory of phlogiston was the theory of the "Materia Cœlestis" extended in a chemical direction. Phlogistic chemistry was Cartesian chemistry. Descartes defined the physical functions of the *Materia Cœlestis*, Becher and Stahl defined its chemical functions, and applied them to the explanation of diverse chemical phenomena. Throughout the writings of Becher and Stahl we find a sprinkling of Cartesianism; they did not, however, adopt the system in its entirety; they appear to have discarded the second and third elements, and adopted the first as the parent of their own system. Enough, I think, has been said in the preceding section to show clearly that the dominant functions of the *Materia Cœlestis* were conferred upon its synonym "Phlogiston."

"The theory of Phlogiston was essentially and completely a syncretistic theory. It was built up of *idola theatri*, collected from various sources, and these were cemented together by the particular *idola specus* of Becher and Stahl. In this process of syncretism, generalisation. In that stage of chemistry, syncretism was inevitable; indeed all theories are more or less tinctured by it, with the exception of those which emanate from a new mode of experimenting, such, for example, as Kirchhoff's theory of the constitution of the sun. . . . . A theory proceeds by slow evolution until it dominates, or is destroyed. It was thus with the theory of phlogiston; arising under the most favourable conditions, it attained full development, became most cardinal, most sovereign, and fell. For twenty-eight years it was looming a half-formed thing through the mists of chemistry; for thirty-four years it was growing in strength and proclaiming its dynasty; for fifty-four years it was dominant, and it was fully ten years yielding up the ghost. There are men amongst us now who have listened to the echoes of its departing steps. Becher and Stahl were the prophets of a new mode of chemical thought, essentially classificatory, systematic, and syncretistic. In their day chemistry was at the commencement of a period of transition, and they bridged the gap which existed between empirical chemistry and modern chemistry. They did not collect the materials for the structure, they did not altogether construct it, but they designed it, and helped in the work of building. Albeit a bad bridge, and built upon shifting sands, yet it was a channel of escape from mystic science, and many passed over to take refuge on the other side." The author concludes as follows:—

"Of the influence of the theory of phlogiston, I need say but little. It was not the first chemical theory; it did not give the first explanation of combustion, and it was established in the face of facts which carried with them its refutation. When the first stage of its development was passed, facts were adapted to the theory, and phenomena were tortured and garbled so as to fit in with it, by which means the progress of chemical science was somewhat retarded. Even when Lavoisier had conclusively proved the fallacy of the theory, this blind adherence shut the eyes of the phlogistians to the merits of the new system, and to the utter falsity of their own. Nevertheless, the theory exercised influence for good; for by its means a certain amount of order was introduced among a vast chaotic mass of chemical facts, and phenomena were classed together, and reasoned upon together, and together submitted to similar processes of mental analysis, after the manner so strongly advocated by Francis Bacon."

"When Mde. Lavoisier, habited as a Greek priestess, burnt the writings of Stahl upon an altar dedicated to the new science, the downfall of the theory of phlogiston was not alone typified; for in that holocaust perished the vast system of empiricism which had pervaded chemistry from the time of its origin until then—relics of Egyptian and Chaldæan lore, of an age of fanaticism, of intellect perverted by a false enthusiasm. Phlogistic chemistry had arisen on the ruins of the older structure of mediæval chemistry, and from it arose modern chemistry. Let us be fain to remember that the mother died in giving birth to the child. The new science was, as Dionysius, born of the dying Semele; and while we worship the son, like the Ancients we have not forgotten to raise a statue to the mother."

LECTURES.

ON HEAT AND COLD; A COURSE OF SIX LECTURES* (ADAPTED TO A JUVENILE AUDITORY), DELIVERED AT THE ROYAL INSTITUTION OF GREAT BRITAIN (CHRISTMAS, 1867-8).

BY JOHN TYNDALL, ESQ., LL.D., F.R.S.

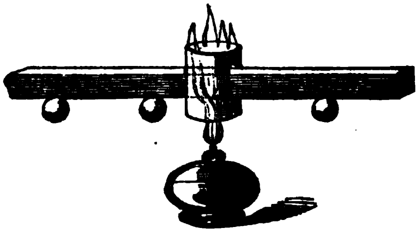
LECTURE IV.

(Continued from *Am. Repr., March*, 1868, page 134.)

Propagation of Heat.

I HAVE now to say a few words upon another subject—the propagation of this thing we call heat—this curious quivering motion of the atoms of bodies; and in order to make this evident to you, I will, first of all, make an experiment or two on liquid bodies, or on gases. I want you to understand the manner in which heat distributes itself in gases, and, for that purpose, I have here placed a little piece of platinum wire—that metal which we raised to a bright white heat in our first lecture. It is a refractory metal, and bears a very large amount of heat. Now, we will have the room made dark, and Mr. Chapman will excite our electric lamp, and I will ask you to look at the shadow caused by this little platinum wire on the screen. I trust that even the most distant young philosopher now sees that shadow. We will heat the platinum wire by an electric current, and you will observe two things. You see, first of all, that the platinum wire gets longer—swags, sinks down—when I heat it. Observe also the air rising up from the surface of the heated wire. That wave-like motion is due to currents of heated air rising from the wire. The air when heated, rises in that way. The same is true of liquids: I have here a glass cell containing cold water, which will enable you to see this. I will place it in front of the lamp, and cast an image of it upon the screen. There is a means of warming this spiral of platinum wire within the water, and I want you to observe that the same thing occurs in water as you saw taking place with the air just now. Mr. Cottrell will now make the circuit for the electric current to pass; and then the moment the circuit is made you will find that the water will be heated by this spiral of platinum wire, and the heated particles of water will rise to the surface of the liquid. There, on the screen, you see the action of the hot wire upon the water, causing he water to rise in these *striæ*. The water goes up from the eated surface, and in time the heated particles will distribute themselves through the entire mass of the water. I make this experiment in order to fix upon your minds the ifference between this action and another which resembles ; at first sight. The action which I have shown you re

FIG. 19.



ives the name of *convection*, which I should like the elder ys to remember, and I want you to distinguish between is and another process, which is a very different one, and hich is called *conduction*. In order to illustrate this subject conduction, I have placed here before you an iron bar, and opper bar (Fig. 19), and I want to ask them which concts heat best. Mr. Cottrell will now light a lamp, and ce it underneath the bars, so as to heat the ends of them at the same time; and as they become hot they will liberate these little balls, which are fixed on with wax; and I think you will find that the heat will travel along the copper better than along the iron. Here is a similar apparatus, with bits of tallow candle fixed to it. The greater the number of these pieces of candle that drop away from either bar, the farther and better the heat has travelled through that body. This is almost a better experiment than the more elaborate one, and it is one which you can make at home for yourselves. The copper will be able to melt away all its candles, while the iron will not be able to do so. The whole philosophy of the clothes you wear is, that they are bad conductors of heat. Your bodies are sources of heat. Through the burning up of the food you eat, within your bodies, warmth is produced; and the object of the woollen clothes which you wear at the present cold season of the year, is simply to prevent the passage of heat from the body to the air. For this reason we clothe the body with woollen cloth, that being one of the worst conductors of heat in nature. But the cloth has no warmth in itself. If I want to keep ice cool, as I did in a former lecture, I wrap my ice in flannel, which prevents the heat from without coming to the ice. Thus the woollen cloth simply prevents the transfer of heat in either direction, and hence the value of these non-conductors as articles of clothing.

The experiment with the pieces of candle sufficiently illustrates the fact that different materials differ in their power of conducting heat. I might also show you this in another way. If I warm this piece of iron by putting it into warm water, and then place it upon a cylinder of glass which stands on the face of the thermo-electric pile, that glass does not allow the heat to pass through to the pile, and the needle still remains on the side of cold. It would be a long time before the heat of this iron passed through the glass and reached the face of the pile. I will now remove the glass and place a cylinder of copper on the face of the pile, and then put the warm iron on the copper. I suppose that not more than two or three seconds will elapse before the heat will pass by the conduction of the copper to the face of the pile, and the moment it does so you will see that the needle will come to the other side of the middle line, showing heat. Now, in this case, instead of having the heat transferred, as in liquids or gases, by the passage of hot masses through the remaining bulk, we have a transmission of heat from atom to atom of the copper; and this process, as I have said, is called *conduction* of heat, in contradistinction to the other process, which is called *convection*.

And now I have to go on to another subject of a somewhat different character; but in passing I must say a word upon a very useful piece of apparatus, the safety lamp, which, unfortunately, is not always wisely used. I will state the problem which the inventor of this simple, but very wonderful apparatus, placed before him. You must know that in our coal mines the miners are prevented from using a candle to light them while at their work, in consequence of the quantity of gas which is in the air of the mines. In former times they used to employ a flint and steel, and work by the feeble light of the sparks The problem which Sir Humphry Davy, the inventor of the safety lamp, set before him was this:—"How can I give the miner light, and still preserve him from this explosive gas?" and he thought, "Can I put a light in any way within an apparatus, so that, although the light shall shine through the apparatus the gas outside will be prevented from exploding?" He found out that a flame could not pass through a piece of ordinary iron gauze. In fact, the flame is so much cooled by the wire gauze, in consequence of iron being a good conductor of heat and carrying the heat away from the flame, that the flame cannot get through. You see that when this iron gauze (Fig. 20) is placed over the flame, the flame is entirely cut off, and cannot pass through; and if we light the gas above the gauze it will burn there, but the flame is prevented from reaching the gas below the gauze. (See Fig. 21.) Now, Sir Humphry Davy, when he made the miner's safety lamp, surrounded the candle wick or the oil wick with a wire gauze; and, al-

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[*] Reported verbatim, by permission of the Author, for this journal.

though the light can pass through the meshes of the gauze, you might have an explosive mixture within and without the lamp, but the flame inside could not propagate itself to the gas outside, being unable to pass through the gauze.

FIG. 20.

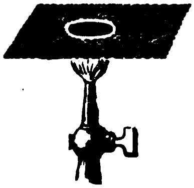
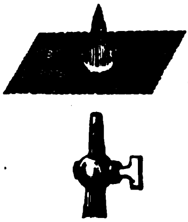


FIG. 21.



I come now to another subject, and a very interesting one. I will ask Mr. Cottrell to heat a silver crucible, or dish, almost to redness; and supposing I then pour water into it, what do you think will occur? You might at first say, "Well, the water will be converted into steam." That is not quite the case. You will find when I pour the water into the vessel that the heat of the vessel produces such an amount of vapour from the water, that the water is supported upon a spring or elastic cushion of its own vapour, and is thrown into the form of a sphere, and the water rolls about in its own vapour. In order to show you this effect, we will cause a beam of light to fall right into the silver basin, and that beam of light will illuminate the drop of water which we pour into the basin. The image of the interior will be then thrown upon the screen. We now blow in a little water.

Now you see represented on the screen the globules of water rolling about—rolling about upon a cushion of their own vapour. Sometimes in this experiment we get a most beautiful figure produced by the water. We get a rosette form of globule. The vapour breaks away from the water in a kind of musical way. We will see if we cannot get the rosette form—a crimping of the edge of the drop of water. [After a few seconds the rosette form occurred. See Fig. 22].

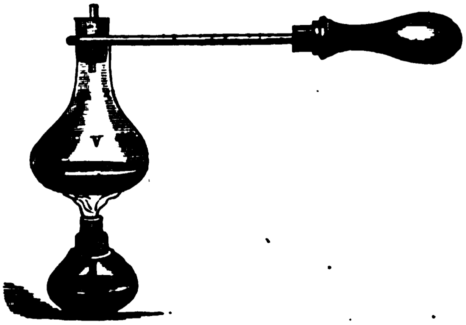
FIG. 22.

When the basin is not very hot, at first these little crimpings arise, and then, when the vapour is not sufficiently strong to lift the water out of contact with the basin, the water will come into contact with the basin, and will suddenly boil. There it is. [At this moment the spherical form ceased, and the water boiled up and immediately disappeared with a hissing sound.]

I must now send Mr. Cottrell down stairs to prepare something of very great interest and beauty; but as I do not know whether the experiment will succeed or not, I do not wish to raise your expectation. If, however, it succeeds, the experiment will be a very useful and a very important one.

In the meantime I want to show you what may occur in consequence of this spheroidal condition of water on a hot surface. I have here a little copper boiler (Fig. 23). I will

FIG. 23.



cork this boiler up, but I intend first of all to heat it very highly indeed, and then I will place a little drop of water into the boiler. I now heat the boiler, and Mr. Chapman will hand me some hot water, and when the boiler is heated I will pour a little into it, and that water will roll about as a spheroid. Vapour will be given off, but being small in amount, while the water is rolling about it will escape through a small hole in the cork. I will then withdraw the boiler from the source of heat, and the drop of water will then come into contact with the hot boiler; steam will be generated, and I think that that steam will be sufficient to expel the cork into the atmosphere. [The experiment was performed with the result anticipated.] There you see the steam drives out the cork the moment the water becomes changed into vapour by contact with the hot surface of the boiler. In this way we may have very serious explosions, but that is a subject into which I cannot go at present.

I want now to make an experiment or two which shall illustrate the character of a certain substance with which I am now going to operate. I have had occasion to mention gases several times in these lectures. Now, gases, and, in fact, the very air we breathe, are nothing more than the vapours of substances possessing very low boiling points. For instance, Mr. Faraday, to whom we are indebted for the very finest investigations upon this subject, succeeded in squeezing together the particles of the gas which is contained in this vessel, and forming it into a liquid; and there are other gases which have been liquefied by Mr. Faraday. One of them is a gas called carbonic acid, which we breathe out of our lungs. I want to generate a quantity of carbonic acid gas in this large round glass vessel. We have at the bottom of the vessel some bicarbonate of soda, and I have here an acid. If I pour the acid into the vessel it attacks the bicarbonate of soda, and we get this carbonic acid gas liberated. I dare say we shall presently have accumulated enough for our purpose. [After an interval]—Now let me see whether the gas which has been liberated has not the power of putting out a candle. This will show whether the gas exists in this vessel or not. [A lighted taper was lowered into the vessel, and was immediately extinguished by the carbonic acid gas therein contained.] Yes: there is the gas. You see it is incompetent to support the combustion of the candle. The vessel is very nearly full. Now I will show you that this gas is very much heavier than ordinary air. I might ladle it out or dip it out in a bucket, and if I did so in front of the screen you would see it fall like water from a vessel, although under ordinary circumstances it is quite invisible. But I want to show you its heaviness by means of a soap bubble. I will blow a bubble from this clay pipe, and allow that bubble to fall upon this invisible gas. You will find that the bubble will float about upon the surface of the gas as if it were floating upon the surface of a visible liquid. [Successive soap bubbles were then produced, and on being detached from the tobacco pipe, were gently dropped on the surface of the carbonic acid gas, and, while floating there, were illuminated with electric light.]

Let me now tell you what I have sent Mr. Cottrell to do. Down stairs in the laboratory we have two very strong iron bottles, and these two bottles are filled with this carbonic acid. The gas in those bottles has been liquefied, and at the present moment he is turning a cock and allowing the liquid carbonic acid to turn into gas. What I want you to understand is that when the liquid carbonic acid turns into vapour it generates enormous cold, just as our vapour of water did on its production, only the cold generated by the carbonic acid is far greater The consequence is, that when this liquid is turned into a gas and generates this cold, a portion of the vapour is turned into snow, and we thus obtain carbonic acid snow. I am almost afraid to speak to you about this matter, lest we should fail to get this wonderful substance. If I do get it I intend to put it into this vessel and make a few experiments with it which will both delight and surprise you. If we get the solid carbonic acid we shall be able to freeze water and produce ice in a crucible when it is actually heated to redness. First of all the carbonic acid snow is itself very cold, but in order to make it still colder I pour a little ether upon it. This turns it into a paste; and this mixture of carbonic acid and ether gives us nearly the greatest cold which has ever yet been produced. If we put that paste of carbonic acid and ether into the hot crucible, what occurs? The carbonic acid and the ether evaporate, and they so evaporate as to produce a protecting coating of vapour of carbonic acid between the red hot crucible and the pasty mass within it. In point of fact, the pasty mass does not touch the crucible at all. It remains intensely cold within the crucible. If we are successful in getting the solid carbonic acid, I shall dip this small brass sphere containing water into the mixture of ether and carbonic acid in the hot crucible; and I have no doubt that the water will freeze and will burst the brass sphere, and we shall then be able to take from the red hot crucible a sphere of solid ice. Mr. Cottrell is a long time bringing the solid carbonic acid. I am afraid he is not successful. Allow me simply to walk down stairs and see that the matter is going on rightly. [The lecturer then went in quest of the carbonic acid. On returning to the theatre he resumed as follows]—I am sorry to say that my worst anticipations have been realised. The experiment below has not succeeded. Here, however, is a little of this wonderful carbonic acid snow—solid carbonic acid. I will put a little in my mouth, and breathe against a candle. If I inhaled it I should kill myself; but I do not intend to inhale it. I intend simply to *exhale*. [The candle flame was then extinguished by the gas exhaled from the lecturer's mouth.]

LECTURE V.

Radiant Heat.—Reflection and Absorption of Radiant Heat.

YOU know that towards the end of the last lecture I failed in the experiment of freezing water in a red hot crucible by means of carbonic acid snow. As I do not like failures in experiments, I will try to make that good. I have here some of this beautiful carbonic acid snow, which I will now put in this red hot crucible. I will pour upon that a quantity of ether, and then I bring down into the middle of the mixture this hollow brass ball containing water. The ether is now boiling. I will put in some more of this carbonic acid snow. It burns my hand,—it is so enormously cold. This ball is very cold, and I have no doubt that already we have produced ice in it. The quantities of the substances are much smaller than I have been accustomed to work with, but I dare say we shall succeed notwithstanding all our difficulties. [After a short interval the water was found to be frozen.] There: look at that. The water in this spheroid is converted into ice, even in this red hot crucible!

I have here some mercury, and I will pour some of it into this basin. I dare say we shall be able to solidify this mercury by means of this beautiful carbonic acid snow. Now observe here what I think you have never seen before. You know the liquid metal mercury. You have it here made solid—frozen by the cold acid. This requires a far greater cold than will freeze water. I might beat this substance upon an anvil or cut it with a knife. It becomes liquid again in a moment. If I hold this solid frozen mercury in a vessel of water, the mercury will become liquid and fall, and each little drop of mercury which falls will produce a stalactite of ice. See, the frozen mercury is being melted by that water. This is really cold water, but it is hot to the frozen mercury, and a mass of ice is produced round about the mercury which has been cold enough to do that.

The best of men and the best of boys in the world, fall and fail; but when one falls the great question with him should be, "How long am I to remain down?" Every boy falls, but if he falls and fails, he ought to be up again and at it, doing his duty. And so, as we failed at the end of the last lecture in this experiment, five minutes had not elapsed before my assistant was down in the laboratory working the pump, determined to make good our failure, which we have here done.

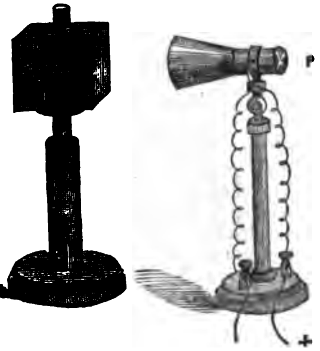
We have now to pass on to another and very different portion of our subject. I have endeavoured to give you a kind of image, more or less perfect, of this thing that we call heat. I have endeavoured to give you a picture, as it were, which your minds should realise.

If you take a hot body and place it in the air, you find that it gradually cools. It it be red hot the glow first of all sinks, and by and by you see nothing of it. The thing gets cooler and cooler, and at the end becomes as cool as the surrounding air. Now, this heat, in the first instance, was a motion of the particles of the hot body. When the body cools it is simply giving up its motion. Now, to what does it give up this motion when you place it in the air? Well, you might say, to the air. True: and when I held the heated piece of iron in front of the screen you saw the hot particles of air streaming up into the air above; so no doubt the motion which the hot body gives up is given up to the air. But if you put the hot body in the middle of a place where air did not exist it would still cool. Now, I want you to exercise your imagination as to the manner in which this motion is disposed of, lost, or given out, when a body cools. I believe most of you understand how it is that sound travels through the air,—at least, how it is that the sound of my voice propagates itself through the air and makes every word I say audible, I trust, to you all. I have often looked into persons' throats when they were speaking, and observed cords and tendons there which are thrown into a state of vibration when we speak or sing. They cause the air to shiver, and those tremors are propagated through it, just as motion is propagated by ripples over the surface of water when a stone is thrown into it. So if I draw this violin bow across a tuning-fork, you have this beautiful sound produced. I can actually see the fork vibrating, being thus near it, and you can hear it tapping against this card. The whole function of a tuning-fork is to throw the air into tremors, and these tremors, communicated to the air, are the cause of sound. The tuning-fork communicates its motion to the mass of air which surrounds it. The vibrations of this tuning-fork gradually become less intense, and the sound which it makes gets lower. Now, that is exactly analogous to the cooling of a hot body. It communicates its motion to what is called the "ether," by means of which bodies which are hot communicate their motion to the universe around. You all hear my voice. The human ear is one of the most wonderful organs in the universe. I often think that the human ear is still more wonderful than the human eye. It is by virtue of this wonderful organ that you hear with perfect distinctness every word I am uttering; but it does not tell that this communication of motion is going on. I want to show you something that will. Instead of the ear, I will take a flame, which I dare say will give me a very good result. Perhaps one of the boys will chirrup to that flame. Every vibration produced by the lips by the act of chirruping is communicated to that flame, and makes it dance in that peculiar way. The action of this flame is an illustration of the motion produced in the air by sound. This action of flames was discovered by Professor Leconte, in the United States; and it has been worked at in this country by Mr. Barret and myself. Something passes through the air and knocks the flame down when you chirrup. The vibrations communicated to the air make the flame behave in this peculiar way.

We now come to consider the cooling of a body. I say that the act of cooling must be figured in a similar way to the action of a body producing sound. The cooling body is communicating its motion, not to the air, but to this wonderful thing called the ether. The radiation passing through the air might be called the radiation of sound; but when motion is communicated to this wonderful ether it is called the radiation of heat. To illustrate this we must employ this beautiful instrument with which you are already acquainted—the thermo-electric pile. I shall now unite the ends of these wires with the pile, and we shall observe by means of our magnetic needle whether the pile is heated or chilled. I wish I could have a warm cheek here, for every one of you here present is a radiating body, not luminous, but radiating. [The lecturer then selected a boy from the audience, and led him to the lecture table.] I want to make my young friend here my radiating body. I will first chill the pile by turning it to the cool side of the room, and then bring the needle to rest by means of this magnet. The pile itself is now a radiating body, and hence you see the needle coming down. I will now try and extract heat from the cheek of my excellent friend here. He does not touch the pile. I will depend purely on the radiation of heat from his cheek, and I will venture to say that if his cheek is not chilled by the very cold weather, the needle will move up through an arc of 90 degrees. Observe, now, the needle goes up in virtue of the heat extracted from his cheek. We will now direct the face of the pile against this comparatively polar region of the room and allow it to waste its heat once more. Now the heat which has produced this effect on the pile is the radiant heat which I want to examine during the rest of the lecture.

I want to show you that various bodies possess the power of emitting this radiant heat in very different degrees. My friend's cheek was an admirable radiator of heat. There are various other bodies, however, much less admirable as radiators. To show this fact, I will take this cube. (Fig. 24). It is covered on three sides with velvet. One side has white velvet, one has scarlet velvet, and the other has black velvet, and this fourth side is a naked face of metal. I should like to make clear to you that these four sides of this cube possess the power of radiating heat in very different degrees; and for the purpose of showing you this I will fill the cube with boiling water. The sides of the cube will become equally heated by the hot water poured into the cube, and then I will allow them in succession to radiate against our thermo-electric pile. I dare say you will then see the distinction. I first bring the needle to zero by turning the face of the pile away from the audience; and now I place the cube of hot water on this little stand near the pile. I think you will agree with me that the outside of the metal side of the cube must be hotter than the velvet surfaces. You could feel this difference by placing your hand upon them. But still, I think the velvet will be able to produce a greater effect upon the pile than the metal surface. The metal side, you see, does not produce much effect upon the pile. Now I turn the velvet to the face of the pile, and you see that the needle goes up beyond the position it occupied when the metal side was there. I now turn the metal side back again, and the needle will go down. Now you see it going down; and when it has gone down a little more, I will turn the black velvet surface towards it, and you will see that the needle will go up again. Thus you see that the heat radiating from this velvet surface is much greater than the heat radiating from the metal; and we have from this fact a beautiful consequence which many boys would not think would occur. The consequence is this. If we filled with boiling water these two vessels, one of which is covered with a thick coating of flannel, and the other of which has naked sides of metal, and allowed them to rest here until the end of the lecture, and then put a thermometer in each to find out the temperature of the water, which vessel do you think would contain the coolest water?

FIG. 24.



Boys of the Audience: The metal one.

The Lecturer: You have not philosophised correctly upon the experiment I made with the cube. Your conclusion is the most natural one, but you saw that the quantity of heat sent away from the covered surface of the cube was greater than the quantity sent away from the uncovered surface. In the same way the quantity of heat from the radiating vessel coated with flannel would be greater than that radiating from the uncovered metal vessel, and therefore at the end of the lecture the water in the covered vessel would be three or four degrees cooler than the water in the other. In order that this difference should exist in favour of the covered vessel, it must be covered very closely; that is to say, the heat must communicate itself very freely from the surface of the metal to the flannel covering. If it were not covered closely the result would be different, and the heat would be preserved. This is the reason why ladies who wish to keep their tea-pots warm, put over them a kind of night-cap, which they call a "cozey." This cozey must, however, be loose about the tea-pot. If it were to fit very closely it would do more harm than good. However, if it does not fit tightly the heat radiates against the cozey, and the cozey prevents it from being radiated into space.

I have said that we find very great differences among substances in their power of radiating heat. Some are good radiators: some are bad radiators. The metals are all bad radiators. I now want to make plain to you another fact which goes hand in hand with this radiation I think you will understand the experiment by which I want to illustrate this point. Here you see I have a metal surface which is a bad radiator. If that metal surface formed the side of a vessel containing hot water, it would radiate far less heat away than this surface which is coated with lamp-black. A vessel coated as this surface is would cool the hot water in it far more rapidly than a vessel composed of naked tin. Now, observe that bodies have also different powers of absorbing or drinking in radiant heat, and as a universal rule the body that is a good radiator is a good absorber. Both actions are perfectly reciprocal the one to the other. I want to make this evident to you by means of a device; for in working in physical science we have incessantly to address questions, as it were, to nature, and we do that by means of experimental devices. And now I am going, in your presence, to ask nature the question which of these two surfaces, M N or O P device that I want to employ in this experiment will be evident to you after a little attention on your part. Nothing is learned or nothing is understood without an act of attention on the part of the student; and if you do not think of these lectures afterwards, and read about the subject afterwards—if you do not dwell upon what we say here, and work at the subject, and reflect upon it—these lectures will pass away from your memories, and make very little impression. In fact, these lectures are very little good except for the purpose of stirring you up, and giving you, as it were, the first taste of science. I really do not care much about lectures. I would rather have ten or a dozen boys working away with me in a room than be preaching to them as I am doing now. However, there is good to be done in this way if you will only think about the subject, and bring your own minds to bear upon it afterwards.

You see I have here two sheets of tin, M N and O P, one covered with lamp-black, and the other uncovered. I place them facing each other, and I put this stand exactly midway between them. Now, I have a litte device here—a tell-tale—which will inform me which of these plates is heated. Suppose I heat this plate. Observe what occurs at the magnetic needle. I simply warm that plate by putting my finger upon it. The red end of the needle moves towards me. I cannot explain the wonderful power which moves the needle. It is what we call an electric current, and is produced by the union of the two metals of the thermo-electric pile. When the plate is heated you see that a deflection of the needle is produced. The needle will return to zero when I withdraw my hand. I want you now to judge which of these two surfaces absorbs radiant heat most freely. The needle will not rest at zero unless these two plates are exactly at the same temperature. If one becomes warmer than the other the needle will deviate from zero. Thus we have it in our power to determine which plate absorbs heat most greedily. Now Mr. Cottrell will give me a ball of copper which is heated to redness. You observe it is radiating its heat as a luminous body radiates light. [The red hot copper ball was placed equidistant between the two plates of tin, one of which was coated with lamp-black. In a few seconds the needle of the pole began to travel from the zero.] Thus we prove that this surface coated with lamp-black, which is the best radiator, is also the best absorber. We might experiment with a sufficient to prevent the absorption of radiant heat. I have here an exceedingly instructive substance. It is a piece of paint given me by Mr. Hills, of the firm of Bell and Co. A portion of this paint is coated with gold leaf, and though the gold leaf is infinitesimally thin, it has been competent to protect the surface of the paint from the action of radiant heat to which the whole thing was exposed, while the other part of the surface, which was not covererd with gold leaf, has become blistered. Where the gold leaf was present it prevented the rapid absorption of the heat.

FIG. 25.

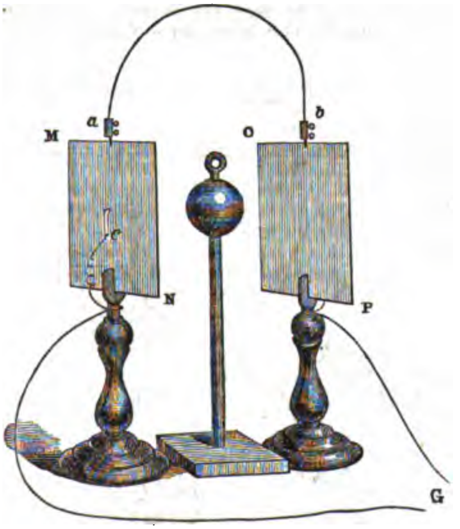
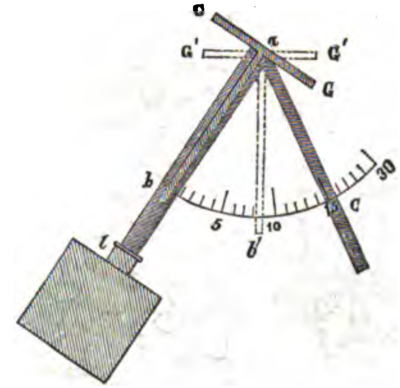


FIG 26.

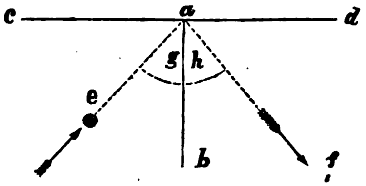
I have here a sheet of paper covered on one side with iodide of mercury, a substance which has its colour discharged by heat. On the other side of the paper there are certain figures represented by a thin coating of metal. I place the paper with the iodide of mercury side downward; and over the other side I will hold a hot spatula which will radiate heat to the surface of the paper. Where the thin coating of metal is, the heat will be rejected, but where the paper is not coated the heat will be absorbed, and then it will reach the iodide of mercury on the other side and destroy its colour. You will find that in this way we shall produce on the underside of the paper a perfect picture of the figures on the upper side, for you will find that the red colour of the iodide of mercury will remain underneath the metal coating, for that coating has the power of rejecting the heat as the gold leaf rejected the heat in the other case, and so protected the paint and prevented its blistering. [The experiment was performed with a successful result.]

FIG. 27.



The radiation of heat obeys the same laws as the radiation of light, and it obeys the law of reflection due to light. This we can illustrate by means of our beautiful thermo-electric pile; but I will first of all make a single experiment that shall impress upon our minds the law according to which light is reflected. It is a very simple experiment, but I trust it will be very effective as far as regards the proof of the law. Mr. Cottrell, who knows my requirements very well, is now placing there in front a little looking-glass, G G. I intend to send a beam of light, *a b*, from the electric lamp, *l*, towards the mirror G G. The beam will strike upon the mirror, and be reflected. How? So that the reflected beam will lie as much on the left of this index, *a b*, which is perpendicular to the mirror, as the direct beam lies upon the right side of it. There are two terms employed in connection with this subject which the elder boys ought to remember. This angle, *g*, made between the perpendicular, *a b* (Fig. 28), and the line,

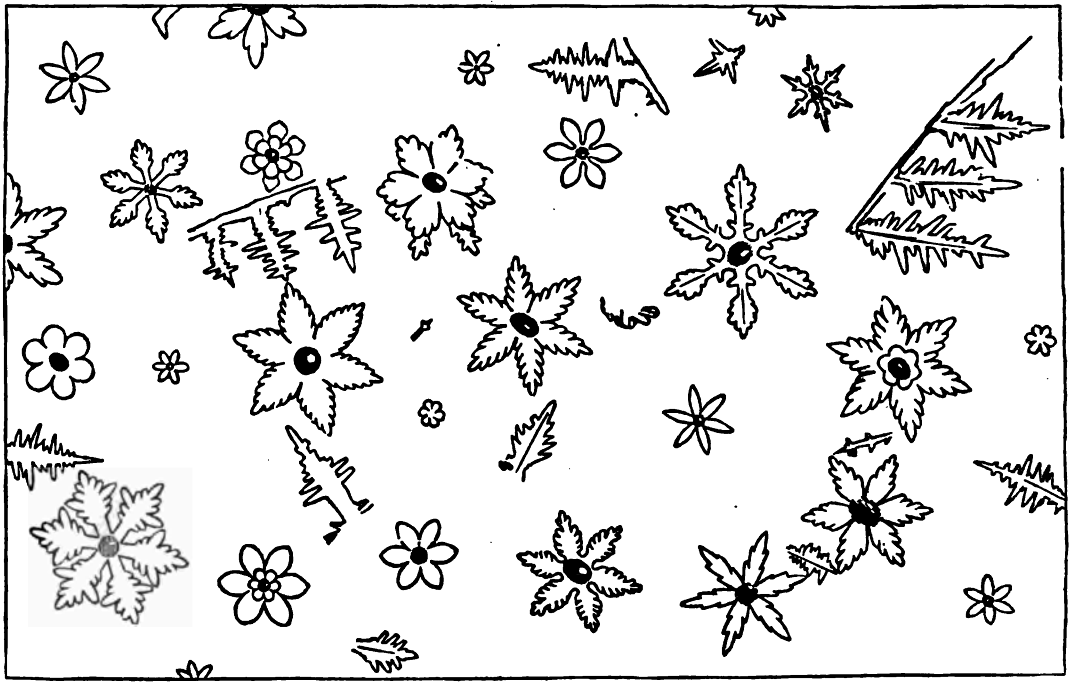
Fig. 28.



e a, along which the direct ray goes to the mirror, is called the *angle of incidence*. The angle, *h*, between the perpendicular *a b*, and the reflected ray, *a f*, is called the *angle of reflection*; and the law as regards both light and heat is this—that "the angle of incidence is equal to the angle of reflection." If I am right in what I have stated you will find the reflected beam as far from the perpendicular on one side as the direct beam is from the perpendicular on the other side. I want now to prove the same with regard to the radiant heat by a very rough experiment, and show you that it obeys the same law as light. I take this piece of tin, which will reflect heat, and hold it so that the radiant heat from this fire will fall upon it, and then be reflected, according to the law I have just mentioned, on to the face of the pile. I have no doubt that reflected heat will warm the face of the pile, and cause the needle to move towards me. We thus see that heat exhibits the same law in this respect as light.

I wanted to make one or two experiments more, and I wished to do so, as before, by means of our thermo-electric pile; but I find that the needle does not act freely although the pile does its duty. Hence I think I must tell you by my tongue what that needle, if it were in a proper condition, would have told you by its motion. I intended to make the needle my voice, but it has become dumb. I wanted to show you that this thing we call radiant heat passes in very different degrees through different bodies. I wanted first to compare the passage of heat through glass with its passage through other bodies. I have here a piece of rough glass, and I have also a beautiful substance—a very common one, but to me more precious than the diamond, though the diamond is a beautiful thing. This substance is rock salt. This would allow heat to pass through it with perfect freedom, while the glass would cut it off. So with different liquids. I have here a liquid called bisulphide of carbon, and here I have some of the well-known liquid called water. If I filled one cell with water and another with bisulphide of carbon, I should find that the bisulphide of carbon would transmit heat with great freedom, while the water would not transmit it at all. Water is, indeed, as regards heat, one of the most opaque bodies in nature to all but incandescent or luminous heat. It is a perfectly opaque body to all rays emitted, say from the surface of a boiling kettle, or from the heated cube, or from the cheek of the young philosopher who helped me in an experiment in the early part of this lecture. During the burning of Her Majesty's Theatre the heat struck upon the windows of a club house opposite, and as the glass would not allow the heat to pass through, the windows became hot, and thus the glass was broken. Had those windows been composed of rock salt the heat would have passed through them, and they would have remained perfectly cool, although there might have been an efflux of the most powerful radiant heat. If time allows, I will show you in the next lecture that we can boil water by radiant heat passing

Fig. 29.



through bisulphide of carbon, though the same heat does not boil the bisulphide of carbon through which it is transmitted, notwithstanding that bisulphide of carbon boils at a lower temperature than water.

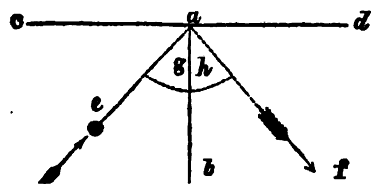
I have told you that different bodies, both solid and liquid, possess the power of transmitting heat in different degrees. Now, the body which absorbs the radiant heat, instead of transmitting it, becomes warm by the absorption. Ice is a body which is exceedingly opaque to the rays of heat, but allows light to pass through with freedom. I intend to place a piece of ice in the path of a beam from the electric lamp, and which will be a mixed ray of heat and light. The ice will stop by far the greater portion of the radiant heat, and the heat will be lodged within the ice. But the temperature of ice cannot be raised beyond 32° Fahrenheit without the ice beginning to melt, so that the portion of the beam arrested by the ice will occupy itself in liquefying the interior of the ice. It will liquefy the ice internally, and I want you to see the wonder and the beauty involved in this beautiful substance which you skate over every winter, but, perhaps, never think of. This beam of light and heat passing into the ice will dissect the ice and separate the crystals, and you will see the beautiful figures into which the ice resolves itself. The ice will break up internally into the most beautiful flowers consisting of six petals. In order to enable you to see these figures I must magnify them very much, and for that purpose I shall cause an image of them to be thrown on this large white screen. The lamp is placed in the gallery to increase the distance from the screen, and so make the figures appear larger. Mr. Cottrell has a lens there, and he will now take a piece of ice, and make the sufrace smooth by putting it on a warm body, and then place it in the path of the beam. The ice has been cut parallel to the plane of freezing from a block of the so-called Wenham Lake ice. It has been cut, I say, parallel to the surface along which the ice grows. [After a short time the image of the ice-flowers began to appear on the screen.] I do not know any experiment that I have ever made which is more delicate and beautiful than this. The flowers are growing larger and larger. First of all you see these leaves, and within you see a crimping. Those spaces which you see are spaces entirely devoid of air, for you know that the water occupies less space than the ice. The ice is larger than the water which formed it, and as the inner portions of this piece of ice melt, the water occupies less space than the ice, and a small vacuum is produced at that spot. This screen presents a glorious surface of ice-flowers. Every particle of ice is built up in this beautiful way. The ice has now become disintegrated, but I do not think your patience has been ill rewarded.

LECTURE VI.

Reflection, refraction, and absorption of radiant heat.—The heat of the sun.—Visible and invisible rays.—Extraction of light from the rays of heat.

In our last lecture I endeavoured to explain to you the law according to which radiant heat is reflected. I then made use of some terms which were, perhaps, rather difficult to remember. I explained to you that the angle of incidence was equal to the angle of reflection, so that if you suppose the surface of this table, *c d*, to be a reflecting surface, and this rod *a b*, a perpendicular to the surface, when a ray of light, *e a*, falls upon the surface, striking the bottom of that perpendicular, the ray is reflected so as to lie as far to the left of the perpendicular as the direct ray lies upon the opposite side of it. That is to say, the angle of incidence, *g*, on the one side is equal to the angle of reflection, *h*, which is on the other.

Fig. 30.



And now I have to draw your attention for a moment, not to the reflection of light or radiant heat from planes or flat surfaces, but to the reflection of radiant heat from curved surfaces. I have such a surface here. It is a large concave mirror, as it is called. It forms part of a large sphere of glass: it is, as it were, a slice cut from a large sphere of glass. Now, suppose a sunbeam to come in this direction, and fall plumb upon the mirror: you see that the edges of the mirror are bevelled or slanted off, and the consequence is that that sunbeam striking on it would be reflected in such a way that the reflected rays would converge and form a cone of convergent rays. I want to show you that when light is thus reflected from a concave mirror it is gathered up to a point which is called a focus. We will now throw a beam of light upon it. You cannot see light itself, but you can see bodies illuminated by the light; and in this room, and especially in London air, and, indeed, in all air, there is a considerable quantity of common dirt floating in the air, and these dirt particles will be illuminated by the beam of light; and I think this will enable you to see that after reflection the beam of light will be gathered up and brought to a focus. You see the beam is now reflected from the concave mirror and is gathered up in this wonderful way into that convergent cone. If we had time, we might prove that this must be the manner in which the rays would behave after reflection in accordance with the law that the angle of incidence is equal to the angle of reflection.

Now, having shown you this convergence of the rays of light, I want to show you the reflection of the rays of heat; and for that purpose I have not a single mirror, but two mirrors. They are called "conjugate mirrors," and one is suspended over the other. I have here the means of obtaining the beautiful electric light from a battery of fifty cells: if I now place this light in the focus of this mirror the rays will be reflected upwards, and if the mirror were perfectly true they would be reflected upwards in a parallel beam, or, so to say, a solid cylinder of light. Now remember what occurs. The rays of light will fall upon this lower mirror; they will be reflected upwards by it in a straight cylinder; that cylinder of light will strike upon the upper mirror, and will be converged, and reflected again from the upper mirror, and brought to a point in what is called the focus of the upper mirror. You will see these rays of light going upward through the dust of the room when the room is darkened. I intended to have a silver bead in the upper mirror; and if it were there you would see it shining with the brilliancy of the sun, owing to the convergence of these rays of light in the upper mirror. If I put the light in the upper mirror instead of the lower one, the rays would be brought to a focus in the lower mirror. I want to show you this with heat; and for that purpose I will take some boiling water. I lower the upper mirror and hang in its focus a flask of hot water; and now we will examine what occurs with the rays of heat. Having placed the flask in position, I draw the mirror up into its former place near the top of the house; and now the rays of heat are coming down from that hot water. Although you cannot see them, they are coming down as the rays of light which were given off from the electric light just now. The rays of heat are striking upon the surface of this mirror, and they are collected and brought to a focus here. I think that by means of our beautiful thermo-electric pile I shall be able to show that this is really the case. I now bring the face of the pile under the mirror, turning it downwards—not upwards, towards the hot water. You observe that the needle very soon moves, in virtue of the heat which is reflected by the lower mirror and collected to a focus in this way. I will now turn the face of the pile towards the cool region of the room, and allow its heat to waste itself; and now for the flask of hot water I will substitute a totally different body—a very cold one. I will, in fact, place a freezing mixture in the focus of the upper mirror, and then operate with the pile exactly as I did when the flask of hot water was there. You will now observe that the needle will move in the opposite direction. It will first come down to zero, and then move up on the opposite side. There will be a very sensible deflection, indeed, if I hit the right point. [The deflection took place as indicated] Now, I dare say many boys here present think that, as rays of heat issued from the vessel containing the hot water, so rays of cold issue from the vessel containing the freezing mixture. That, however, is not the case. In the case of the freezing mixture our thermo-electric pile is the warm body. It is hot compared with the freezing mixture, and that pile radiates its heat against this lower mirror; the heat is reflected above, is re-reflected against that mirror, and is then absorbed and drunk up utterly by the freezing mixture, so that the pile in this way wastes or loses its heat, and therefore gives that deflection of the needle due to cold. Instead of this freezing mixture or the bottle of hot water, I will now place in the focus of the mirror a body which I hope will be given to me in a bright cherry-red hot state. A copper ball has been placed in the fire in the next room; we will suspend that copper ball when it is red hot in the place which was occupied by the freezing mixture, and see whether we cannot get very visible evidence of its radiation. I do not like to use the thermo electric pile in this experiment; but I have here some black paper, and sometimes we are able to make paper smoke in the lower focus. I place this paper below in the focus, but I see the ball is not hot enough to burn it; there is no apparent action; but I can feel the heat very strongly indeed, through the reflection of the rays, so that my hand can not rest there. Some of this paper smoked freely yesterday when brought within the focus. If I place the face of the thermo-electric pile there for a single moment, you will find what I said to be true. The action of the needle proves that you have there the focus heat I have been endeavouring to describe.

Now we have to pass on to the still further consideration of these rays of heat: and I will first of all try to make plain to you wherein consists this wonderful light that we have been operating with so often. I will take a thin slice of this light and try to unravel it before you. The screen will be lowered in order to enable me to do this, and we will lower the roof so as to darken the room. You will see the beam of electric light making itself evident in the dust of the room; and this lens enables me to obtain a beautiful image on the screen. Now I want to twist that beam aside. That white mass of light which you see, is due to a mixture of lights of various colours. I will twist this beam aside by means of a prism, and separate these colours one from the other. First of all I will send the light through a single prism, thus, which gives this wonderful, rich display of colours upon the screen. Nothing can be more beautiful than this—so rich and lovely. And now I will try and make the band still bigger,—not richer: it is impossible to have it richer or more beautiful than that. For the purpose of increasing the size of this band of colours I will send the beam through another of these prisms, which will pull it aside still farther, and spread these colours still more. You now have the beam passing through a second prism, and when I bring the beam into the field you have this splendid band thrown on the screen. This is called a spectrum. This was the great discovery of Sir Isaac Newton. He found that white light was composed of all these colours; and if it were consistent with our present course of lectures, we could make these colours combine again and form white light. We will now turn up the gas, and you see how dead the spectrum becomes when the light falls upon it. I asked for the gas light in order to choose a boy "ruddy, and of a fair countenance." [The lecturer then selected a boy answering to this description, and led him to the screen. The room was then again darkened.] You will find what happens to the colour of his face when I lift him into the midst of this spectrum. Here [holding the boy's face in the red light] he is blooming like a rose. Now [transferring him to the yellow] he is like something very different from a rose.

Now I want to say a few words upon this wonderful spectrum. You see a great mass of light here, and you might suppose that that is all which comes out of that wonderful electric lamp; but that is, in reality, not at all the case. You have here a certain distance which is rendered visible to the eye by these splendid colours, but there are rays extending about as far on the outside of the extreme red, as the green colour is on the other side of it. The most powerful radiation emitted by the electric light does not fall on any part of the visible spectrum, but it falls as far on one side of the red as the green is from the other. And so also at the other end of the spectrum we have a vast body of rays stretching out beyond the visible portion; but all these ultra-violet rays and the ultra-red rays are perfectly incompetent to produce vision, although a great number of them reach the retina. I now want to make evident to you the prolongation of the spectrum in the direction of the violet, and for that purpose I must make use of a less expansive spectrum. We have produced this by means of prisms of liquid, but I must now make use of a prism of glass, or else have only one of the liquid prisms instead of two. I want to give you an idea of the comparative power of the luminous rays and those dark rays I have spoken of. I have now produced this present spectrum by means of one of the liquid prisms. We might, as Sir William Herschel did when he first discovered the dark rays of the sun, place a thermometer in this dark part beyond the red, and we should find that it would show an augmented temperature because of the heat falling on it from the electric light. Then if we travelled from this red end of the spectrum towards the other, we should find that the thermometer would gradually sink, and if we went back again it would rise gradually through the violet, through the blue, through the green, and the yellow, and the orange to the red, the red being the hottest part of the visible spectrum. But Sir William Herschel did not stop here, but made a further discovery. Far beyond the red he found very powerful rays falling upon the thermometer, and he represented the rise of the temperature by lines of certain length. He represented the least heated part by a short line, and the next by a longer one; the line representing the heat of the green is of a certain length; and the heat of the yellow was marked by a longer line still. The whole visible radiation from the sun was determined in this way by Sir William Herschel; but we have now far finer methods, and with the electric lamp which you now see before you, we went over these colours with a thermo-electric pile. The whole radiation of a visible portion of the spectrum is represented by this small coloured area that you see represented on the diagram; but over and above that, and beyond the red end of the spectrum, you have an amount of heat which is represented by this great mountainous peak. The invisible radiation is nearly eight times the visible; that is to say, only one-eighth part of the rays emitted by the electric light is competent to excite vision, all the rest are rays of heat, and not rays of light.

And now I want to show you the prolongation of the spectrum in the other direction; and for this purpose I will make use of a prism of flint-glass instead of this prism of bisulphide of carbon. I place the prism exactly as in the former case; the display of colours is not now quite so brilliant, but the glass is more transparent to the rays that I want to show you than the bisulphide of carbon is. I have here a certain substance called sulphate of quinine; and I have here also a screen of white paper which was wetted with this substance before the lecture. It was found by Professor Stokes that this substance has the extraordinary power of rendering visible these invisible rays of light beyond the violet. Now, observe this band of light which becomes visible beyond the violet, when I introduce the paper screen which has been spread with the sulphate of quinine. There is darkness when the screen is not there, but when it is held up you see this lovely band of colour produced. If I take the liquid itself and daub it upon a piece of paper, it will render the invisible rays visible. I have here also the means of changing the colour of rays by means of this beautiful violet glass, and rendering rays visible which were hardly visible before. Here is a piece of paper, on which are printed the words "A happy new year." As you look at it you see nothing upon it, by the ordinary light, but if we put up the violet glass observe how beautifully the letters come out.

So much, then, for the existence of rays beyond the red end of the spectrum, and also beyond the violet end, which are incompetent to excite vision. These are what are called invisible rays. Before I proceed farther I should like to show you an experiment by means of these powders. Professor Stokes has called that action which makes the sulphate of quinine visible, "fluorescence." The phenomenon called fluorescence has been known to philosophers a long time. It was observed that certain substances had the power, so to speak, of drinking in light, and then giving it out gradually. M. Edmond Becquerel, of Paris, has rendered himself exceedingly famous by his investigations on this subject, and the powders I have here were selected by him. I am indebted to Sir Charles Wheatstone for them. I will show you that if these powders are shone upon by the electric light, and then the lamp is extinguished, the powders will still retain their luminosity; they will still have the power of giving out light. They, as it were, drink in the light and then give it out slowly and by degrees. [The powders were exposed to the electric light for a short time, and the light was then extinguished.] There you see the powders are self-luminous, and emit this beautiful light. I have here a beautiful butterfly formed of these powders. It is painted upon glass. You see the surface of the glass is now perfectly dark. It emits no light; but if I allow the light of the sun or the light of the electric lamp to shine upon it for a short time, you will see that it has the power of drinking in that light, and emitting it gradually. The surface of glass on which the butterfly was painted with the fluorescent powders was exposed to the electric light. The light was then withdrawn and the butterfly was seen to have become luminous.] This beautiful butterfly is produced by means of these fluorescent powders selected by M. Edmond Becquerel.

(To be continued.)

FOREIGN SCIENCE.

PARIS, FEB. 4, 1868.

Ozone and the cholera.—The nature of the troïlite.—Method of distinguishing the protosulphide of iron from the magnetic sulphide.—A new material for hats.—Ulmic and humic acids.—Reaction by which phenic acid may be distinguished from creosote.—Important products extracted from the olive, and from the Australian myrtle.

URING the autumn of last year, when the cholera was elt severely in Turin, Father Denza studied the meteoroogical condition of the atmosphere; he studied especially he connection between the prevalonce of the disease and he absence of ozone. His observations were made at oncalieri, rather more than half a mile from the town: he electricity was measured as well as the ozone. During he days in August and September, when the cholera was at bout its height, the amount of ozone present was variable, ut considerable—perhaps about the average. The elecricity, however, during these days almost entirely disapeared; it is an interesting observation.

M. S. Meunier has recently published some facts concerng certain compounds occurring in meteorites, pyrrhotine e_7S_8, and troïlite. Troïlite has been considered by several ineralogists as a protosulphide of iron; the results M. Meunier has obtained in analysing many samples of troïlite, separated from meteoric iron, lead him to believe that the composition is much nearer that of magnetic pyrites. He indicates also a method of distinguishing these two substances so nearly alike in constitution, troïlite and pyrrhotine. The reaction consists in the precipitation of copper from its solutions by the one and not by the other. A number of experiments were made with artificial protosulphide and pyrrhotine; it was found that the protosulphide precipitated a solution of copper exactly like iron itself, while the magnetic sulphide gave place to no such reduction. The protosulphide obtained in the dry way exhibits the reaction even better than that obtained in the wet way, since the copper is not deposited in such fine particles. By melting iron and sulphur together, sulphides containing a little more sulphur than the protosulphide are obtained. In experimenting with these compounds as soon as the proportion of sulphur approached that of the magnetic sulphide, the precipitation ceased to be possible. Certain phosphides of iron, like the protosulphide, give rise to a precipitate. We may hope for further details.

Your correspondent hears, on good authority, that an entirely new kind of hat will be introduced in the summer. It will be made of paper in imitation of straw. The process of manufacture is curious, and probably quite new. A straw hat of the required size is covered with plumbago and electrotyped, the straw is burnt out of the mould, and manilla paper pulp pressed in. The invention is said to be that of an American. Many advantages, such as being waterproof and light, are claimed for the material.

M. Lefort has separated from among other substances contained in the trunks of old trees, an acid to which he gives the name xylic acid. This acid possesses the formula $C_{24}H_{14}O_{16}+HO$; it presents itself in the form of a vitreous black hard substance. Apparently this compound is the basis of all the compounds studied up to the present time, under the names of ulmic and humic acids.

M. Rust has made known a reaction by which phenic alcohol may be distinguished from the creosote separated from beech-wood tar. A mixture of 10 parts of collodion and 15 parts of phenic acid, forms a gelatinous mass, while the creosote from beech-wood tar mixed with collodion gives a clear solution.

M. de Luca, professor of chemistry to the Faculty of Science in the University of Naples, contributed at one of the meetings of the Société d'Encouragement, a memoir on some important products extracted from the olive and from the Australian myrtle. When the leaves of the olive are kept in strong alcohol they lose water, and at several points upon their surface radiated silky needle-shaped crystals make their appearance. If the leaves are treated with boiling alcohol, the liquid on cooling deposits the same crystalline matter; in this case, however, contaminated of course with all the other principles soluble in hot alcohol. The crystals have a faint sweet taste. The substance is not very soluble in alcohol, and its point of fusion is 164° to 165° C. Its composition is expressed by the formula $C_6H_7O_6$; the physical properties resemble those of mannite extracted from manna. The principle is present in the leaves during development, in small quantity, increasing with their growth; the amount diminishes at the flowering and when the leaves begin to lose their green tint. The process of extraction is easy; the leaves are macerated in water, and the liquid evaporated. The mannite does not undergo fermentation under the conditions, and is found in the residue. The flowers of the olive contain abundance of mannite: taken in the month of June and placed in alcohol, a solution is obtained, which, when the winter arrives (by the fall of 10 or 15 degrees) deposits mannite. The juice obtained from the fruit of the Australian myrtle, by simple expression, is of a fine violet red colour, its taste is slightly acid and very agreeable. This juice, which contains glucose, cream of tartar, and free tartaric acid, undergoes fermentation at the ordinary temperature with disengagement of carbonic acid and production of alcohol. The wine of the myrtle, that is to say, the fermented juice, acquires in time a particular ethereal odour, very agreeable, and which constitutes to some extent a bouquet. By a further exposure to the atmosphere, and the aid of porous bodies, vinegar is easily obtained. There are many analogies between the juice of the myrtle fruit and that of the grape. The myrtle flourishes admirably in Australia in the open air.

PARIS, FEB. 18, 1868.

Essays for the Prize of the Société de Pharmacie.—Detection of Kreatinine in Urine.—Manufacture of Pyrogallic Acid.

EVERY year the Société de Pharmacie offers a prize for the best essay upon some subject connected with pharmacy. This year the announcement of the examiners' decision was enhanced in interest by a speech from M. Coulier (reporter of the examining commission), in which he reviewed the work of the candidates. The detection of arsenic in cases of poisoning was the subject of an essay by one of the competitors, M. Aly-Read. The author had made experiments to determine exactly the temperature at which sulphide of arsenic is decomposed by sulphuric acid. Another competitor, M. Barret, chose for his subject a study of the preparations of opium described in the Codex of 1866. He sought to determine the causes which influence the proportions of morphine contained in different varieties of opium. Methods were given by which the amounts of morphine and narcotine might be estimated. The study of arsenic formed the basis of an essay from M. Dupuy. The first part related to the history of this element, taken first in a purely chemical aspect, then as a toxic agent. The second part contained the results of experiments upon the absorption and elimination of arsenical compounds. M. Dupuy states that an ordinary bath containing an amount of arseniate of soda up to 20 grammes will not affect a man. M. Eberlin sent an essay devoted to the chemical study of glycerine and its pharmaceutical application.

Officinal cantharides was the title of an essay in four chapters, by M. Fumouze, (1) Natural history of cantharides; (2) Chemical history of cantharides; (3) Causes which can alter or enfeeble its properties; (4) Insects and acarides met with in cantharides.

The resins employed in pharmacy was the subject of an essay by M. Guelliot. Finally, M. Guichard, a competitor, sent an essay on the alkaloids of the cinchonas. A resumé of the actual state of science regarding the constitution of artificial alkaloids, and of the genus of natural alkaloids, opens the subject. Then there is a chapter in which the question is treated historically, followed by six others upon those alkaloids which are obtained from the cinchonas besides quinine, and the chlorinated, brominated, iodinated derivatives. These chapters contain a complete history of the chemical properties of these substances. The salts of quinine, cinchonine, and quinidine are treated separately. After these the extraction of the alkaloids, and their commercial preparation, form the subject of consideration, and then the adulterations of quinine are taken. M. Guichard devotes a chapter to the special study of the red colouring matter which forms when cinchonine, quinine, and especially quinidine, are submitted to distillation; pure quinine he finds does not furnish these purple vapours, the presence of a glucoside is necessary. The commission unanimously awarded the prize to M. Guichard.

M. Roussin has proposed the use of bichloride of mercury for the detection of kreatinine in urine; kreatinine is precipitated from its solutions by the mercurial salt.

MM. de Luynes and Esperandieu have published a research on the preparation and some properties of pyrogallic acid. They remark in commencing, that the processes actually in use yield only about 25 per cent. of the weight of gallic acid employed. By the action of water at 200—210 degrees they are enabled to transform gallic acid into pyrogallic acid and carbonic acid. The process is conducted as follows:—Into a[*] brass cauldron with a tightly fitting cover, the gallic acid is introduced with two or three times its weight of water; the cauldron is heated to 200—210 degrees, and maintained at this temperature for an hour and a half to two hours.[*] At the end of this time the vessel contains a slightly coloured solution of pyrogallic acid. By boiling with animal black the colour is removed; the solution is filtered, and the water removed by evaporation. Upon cooling the pyrogallic acid solidifies in the form of a hard crystalline mass, slightly amber, and sometimes rose coloured. To obtain the product white, it suffices to distil in vacuo; an operation which goes on very rapidly, almost instantaneously. The yield of pyrogallic acid obtained by this process is equal to the amount theoretically obtainable. These are the properties of pyrogallic acid described. A solution of pyrogallic acid added to lime-water, gives rise to a magnificent violent colouration. Ethylamine causes the same colouration. A concentrated slightly acid solution of quinine, added to a concentrated aqueous solution of pyrogallic acid, produces a yellowish crystalline deposit, which contains the elements of sulphate of quinine and pyrogallic acid. If perfectly pure filtered solutions are mixed, no precipitate is formed until a little crystal of sulphate of quinine is added; in which case the solution becomes immediately a solid mass. Orcine and resorcine react just in the same way with sulphate of quinine, whence this reaction with sulphate of quinine would appear to be common to those substances designated as phenols.

REPORTS OF SOCIETIES.

ACADEMY OF SCIENCES.

JANUARY 27, 1868.

Respiration of cattle.—Study of a disease which attacks ruminants.—Production of nitrous gas during the fermentation of beet-juice.—Niobium and tantalum.—On dissociation.—Phenomena intimately connected with muscular contraction.

AT the meeting on the 27th January, M. Dumas thanked the Academy for the honour it had conferred upon him in making him perpetual secretary. The President announced the loss by death of M. Serres. M. Reiset communicated three memoirs, entitled—(1.) Chemical researches on the respiration of farm cattle, and the influence of dieting. (2.) Study of the gas produced during the meteorisation of ruminants; application to veterinary therapeutics. (3) Note on the production of nitrous gas during the progress of fermentations in distilleries. Estimation of the proportions of ammonia contained in beet-root juice. M. Marignac communicated a research upon the reduction of niobium and tantalum. M. Debray contributed a memoir on "Researches on Dissociation." M. Des Cloizeaux sent a note "on the clinorhombic form, to which harmotome and Wœhlerite ought to be referred, after the late researches on the dispersion of the value of their optic axes." M. Marey addressed a note on phenomena intimately connected with muscular contraction. M. Reiset used in making the experiments which form the subject of his first memoir, apparatus of such dimensions as to enable him to submit the exhalations of calves, full-grown sheep, &c., to examination. During the respiration of calves and sheep, he found a considerable quantity of protocarburetted hydrogen in the gaseous mixture. This, too, is under the normal conditions. Calves in some experiments were fed upon milk only; deprived thus of vegetable food, the gaseous mixture exhaled resembled more nearly in its composition that exhaled by the carnivori. The production of carburetted hydrogen became absolutely *nil*. M. Reiset considers the formation of carburetted hydrogen in the stomachs of ruminants, when upon their natural food, to be a phenomenon of incomplete combustion. He deduces from these and former researches, the general conclusion, that the respiratory products depend much more upon the nature of the food than upon the species of the animal.

M. Reiset's second memoir referred to a disease which attacks cattle feeding on pasturage. The effects are rapid swelling, and, finally, suffocation. He analysed the gas produced, that which in fact causes the swelling. He found it to be almost wholly, 74 per cent. carbonic acid. Alkalies are therefore proposed as remedial agents. The third memoir relates to beet-root juice fermentation. As the manufacture of beet-root sugar is not an English industry, an abstract of this memoir would probably possess little interest for your readers.

M. Marignac communicated to the Academy the account of a number of experiments upon the reduction of niobium and tantalum. Fluoniobate of potash is reduced by heating with sodium in a wrought iron crucible; the product is, however, niobуret of sodium, which remains as a black powder disseminated in the fused mass. Water destroys the combination, nioburet of hydrogen being produced with some disengagement of hydrogen. Nioburet of hydrogen contains about 1 per cent. of hydrogen, agreeing, therefore, with the formula NbH. It is a fine black powder, having a density varying from 6 to 6·6. By roasting it is promptly converted into niobic acid, entering into ignition, though the increase in weight reaches only 37 or 38 per cent., while theory requires 41. This hydride is not attacked by hydrochloric acid; it is very stable; heated to full redness for an hour in a current of hydrogen, it only loses ·1 per cent. An attempt was made to reduce fluoniobate of potash by magnesium; a violent detonation resulted. Similar treatment with aluminium in a black-lead crucible gives place to a compound of that metal and niobium, having for its formula $NbAl_3$, which is obtained upon treating the button of aluminium with hydrochloric acid. This is a lustrous crystalline compound. It is only oxidised very incompletely by roasting. M. Marignac has obtained an analogous compound of tantalum, $TaAl_3$, by heating the fluotantalate of potash with aluminium. It is also a lustrous crystalline powder scarcely attackable by hydrochloric acid, and only oxidised slightly by roasting. The general result of his researches M. Marignac considers to be a confirmation of the analogy that has been already observed between the metals niobium, tantalum, and silicium. He thinks these three metals, with zirconium and titanium, should be grouped together. The atomicity of these metals, he remarks, is not, however, the same; niobium and tantalum are pentatomic, while the others are tetratomic.

M. Marey's memoir upon phenomena, intimately connected with muscular contraction, was purely physiological.

M. Debray states in his memoir that a hydrated salt has for ach temperature a tension of dissociation which is measured y the elastic force of the aqueous vapour which it emits at his temperature. Admitting this, the phenomena of effloescence and hydration are easily explained. A salt effloesces when the tension of its water vapour is greater than hat of the aqueous vapour existing in the atmosphere. A ry salt becomes hydrated when the tension of the aqueous apour contained in the atmosphere is greater than that which he salt emits at the same temperature. Hydrous salts which o not effloresce owe, then, this property to the fact that the ension of the aqueous vapour emitted by them at ordinary mperatures is always inferior to that commonly possessed y the atmospheric aqueous vapour. These same salts effloesce when placed in an atmosphere where the elastic force f the aqueous vapour contained in the air is less than that hich they emit.

FEBRUARY 3, 1868.

ropagation of Waves through Gaseous Media.—Manufacture of Charcoal and Metallurgy of Iron.—Dialysis of Induction Currents.—Beet-root Fermentations.

HE memoirs brought before the Academy of Sciences on e 3rd, of chemical interest, were the following. On the radity of the propagation of waves through gaseous media, by M. Regnault. On the carbonisation of wood, and the metallurgy of iron by M. Gillot. On the decomposition of nitrates during fermentation by M. Schlœsing.

M. Bouchotte sent a third note on the dialysis of induction currents.

M. Gillot states in the first part of his memoir, that the only condition necessary for a good carbonisation of wood to take place, is that the operation be made to proceed slowly. The decomposition of the wood commences at about 100°, wherefore analyses of samples of wood dried at 150°, do not give the true composition. During the decomposition of the wood, resulting in production of carbonic acid, and hydrocarbons, heat is developed in the interior vessel, which is thus raised to a temperature in excess of that of the oven. This result is produced when the temperature of the oven approaches 300°, and it must continue to the end of the operation. Too rapid an increase of this internal heat gives rise to the formation of tar and gaseous products in unnecessarily large quantity, diminishing in a corresponding degree the useful accessory products, as well as the yield of charcoal. The condensed products are richest in acetic acid when the temperature of the oven is 218°; at this heat they contain 48 per cent. Wood, when the operation of carbonising is well carried out, may be made to yield 7 or 8 per cent of monohydrated acetic acid; finally, the resulting volume of carbon is two-thirds of that of the wood employed. The second part of this memoir relates to the employment of fuel in the furnaces used in the metallurgy of iron. M. Gillot says that it has been demonstrated that in operating in the ordinary way, with the blast furnace in general use, the calorific power of the combustible gases, escaping at the mouth, represents, with but slight variations, two-thirds of all the combustible matter employed. It has also been demonstrated that to convert the pig iron produced into steel or iron, the heat required is much less than the total heat which the combustible gases, lost at the furnace mouth, would produce by combustion. M. Gillot collects these gases by means of an exhauster, in a gasometer; he afterwards liberates them according to the requirements of the operation.

M. Schlœsing's note on the decomposition of nitrates during fermentation, referred to M. Reiset's memoir on beet-root juice fermentations. In answer to the request of the subscriber whose letter was forwarded to me, intimating that the subject of beet-root fermentation was one with regard to which details would be acceptable in England, an account of M. Reiset's research is introduced into this letter. The production of nitrous gas during the fermentation of the saccharine juice is regarded by the manufacturers as a serious accident. This result is, however, nearly always observed if the juice does not contain a sufficient quantity of free acid. Under these circumstances the fermentation is arrested, and usually it cannot be made to proceed again, no matter how much yeast is added. The lactic fermentation is developed, it predominates, and the sugar is rapidly converted into lactic acid. Juice which contains before the fermentation only two grammes of free acid, rapidly increases to eight or ten grammes the litre without any further addition of acid being made. M. Reiset has established by numerous experiments, that in a general way the juice resulting from the maceration ought to contain an amount of free acid equivalent to three grammes of monohydrated sulphuric acid in the litre. In well conducted distilleries it is customary to regulate methodically the proportions of sulphuric acid, too often used as a remedy for all accidents. The ammonia present in the beet-root juice, combined with feeble acids, is almost sufficient to completely saturate the sulphuric acid added during the operations. M. Reiset employs the method proposed by M. Boussingault to estimate the amount of ammonia: 30—50 c.c. of saccharine juice are distilled with a litre of pure distilled water, and 5 c.c. of solution of potash of 40 degrees, two fractions of 200 c.c. each are collected, and the ammonia deduced from the amount required to saturate a known volume of titrated sulphuric acid. The production of nitrous gas during fermentations has often been explained as due to a reduction of the nitrates found in the juice, but how then admit with the manufacturers that treatment with sulphuric acid prevents it? M. Reiset, thinking, on the contrary, the formation of nitrous gas to be attributable rather to oxidation of the ammonia when this alkali is not saturated by a powerful acid such as sulphuric acid, always keeps careful account of the amount of ammonia present in the beet-root, and regulates the employment of acid by the amount of this alkali. The idea has been put in practice in a distillery and has been at work three years; excellent results have been obtained, nitrous fermentations have happened only very rarely.

M. Schlœsing takes quite an opposite view with regard to the formation of nitrous gas in fermentations, and he advances experiments to prove that it is really a phenomenon of reduction. Experimenting with tobacco juice (naturally acid) to which he had added nitrates, he found that these latter remained intact until, owing to decomposition of the organic matter, the solution became alkaline, then they gradually diminished in amount. M. Schlœsing explains the nitrous gas as the effect of putrefying organic matters upon nitrates; he asks what there is astonishing in the bodies which are able to reduce sulphates to sulphides, being enabled to reduce nitrates to nitrites. The neutral or alkaline state he considers particularly favourable to the production of reducing matters, and therefore applies the fact observed by the alcohol makers, that addition of sulphuric acid prevents nitrous gas being formed, as a confirmation of his view.

MANCHESTER LITERARY AND PHILOSOPHICAL SOCIETY.

Ordinary Meeting, January 7, 1868.

Edward Schunck, F.R.S., &c., *President, in the Chair.*

"*Variable Spot on the Moon's Surface,*" by W. R. Birt, F.R.A.S., communicated by J. Baxendell, F.R.A.S.

The interest attaching to the phenomena presented by the lunar spot Linné is my apology for communicating a few observations on another spot which exhibits similar phenomena. It will be seen that both spots manifest phenomena which appear to be referable to the presence of a *covering* by which the craters are at times *concealed.* We are not cognisant of any agency such as libration, angle of illumination, or variation of distance which affects the forms and appearances of lunar objects, being capable of rendering a crater *invisible* while its place is occupied by a white *cloud-like* spot of light; nor are any of these agencies capable of rendering an object on the moon's surface *indistinct* while others in its immediate neighbourhood are exceedingly sharp and well defined. With the hope of directing the attention of astronomers to this curious class of lunar objects, may I be permitted to lay the following observations before the Society? They have been made principally by the Rev. W. O. Williams, of Pwllheli, who has undertaken the examination of a zone on the moon's surface of 2° of latitude, viz., from 4° to 6° south.

The spot in question is marked IV Aα 17, IV Aζ 39 on the areas of the British Association Lunar Map IV Aα and IV Aζ, and is situated in 2° W. long., and 5° S. lat. It is also situated on the S.W. side of the ridge forming the N.E. boundary of Hipparchus, and has been described as a bright spot S.S.W. of IV Aα 7 (Beer and Mädler's Hipparchus F). Its diameter is 5″·94 and magnitude 0°·37, the diameter of Dionysius being regarded as unity. On De La Rue's photograph 1858, February 22, it appears as a spot of about 4° of brightness. It is not so bright as Linné, which is about 5°. In this photograph it is seen to stand upon the east edge of a large *depression* running nearly S. by W.—N. by E. This edge, which forms a low ridge, connects the mountainous boundary of Hipparchus with the mountain IV Aζ 37. IV Aα 7, a bright spot smaller than IV Aα 17, IV Aζ 39, stands upon the west edge of this depression, which also meets the mountain IV Aζ 37.

On Rutherford's photograph 1865, March 6, this spot appears brighter that in De La Rue's, viz. 5°. Linné in this photograph is 6°. The observations that have been made of this spot are as under—

Year.	Date.	Authority.		Character.	Brightness.
1858	Feb. 22	De La Rue,	Ph.	A bright spot.	4°
1865	Mar. 6	Rutherford,	Ph.	A bright spot.	5°
1867	May 11 8½......	Birt,	Obs.	A shallow crater.	
1867	Oct. 7 8½ to 10	Williams,	"	A very bright spot.	
1867	" 17	Ingall,	"	A faint shallow crater.	
1867	" 17 13½.....	Ingall,	"	Drawn as a crater.	
1867	" 17 13 to 15	Williams,	"	A very conspicuous crater.*	
1867	" 18 17 to 19	Williams,	"	Crater very conspicuous, with a small central cone casting a shadow.	
1867	Nov. 5 9 to 10	Williams,	"	Very bright, a streak of interior shadow on the west.	7°
1867	" 6 8 to 10	Williams,	"	A bright patch of light, streak of shadow scarcely discernible.	6°
1867	" 15 18 to 20	Williams,	"	Very bright.*	10°
1867	Dec. 5 6 to 8	Williams,	"	A whitish spot, no trace of a crater.	5°
1867	" 6 9 to 10	Williams,	"	A whitish spot, no crater.	5°

* On these occasions Mr. Williams saw a small bright point to the E, which he considered to be the *highest* point of the ridge.

Mr. **Baxendell** stated that on the night of the 3rd instant he had an opportunity of examining the spot referred to by Mr. Birt with Mr. Gladstone's equatorially mounted achromatic of 7¼ inches aperture, using powers from 60 to 250.

It was then a well-marked though shallow crater, having a diameter about three-fourths of that of Beer and Mädler's Hipparchus F. The shadow of the western wall was very conspicuous on the floor of the crater.

Mr. **Baxendell** also read the following extract of a letter dated November 27th, 1867, which he had received from Mr. C. Ragoonatha Chary, the first native assistant at the Royal Observatory, Madras:

"I have prepared the necessary calculations connected with the total solar eclipse to take place in the Indian Peninsula on the 18th of August, 1868, and these, with appropriate description and remarks on the eclipse by N. R. Pogson, Esq., are now in the press and will be published in the leading Madras Almanac. In these calculations I find that a slide-rule constructed for trigonometrical purposes may most advantageously be used even in such intricate cases as the solar eclipse. It saves more than three-fourths of the time and labour; and having calculated independently with the slide-rule as well as by means of logarithms for several places, I found the difference rarely to amount to half a minute in time, which is no great matter in predicting for amateurs, and even for intending observers. Mr. Woolhouse's method is followed, I believe, in the Nautical Almanac. The skeleton forms of this method, which are printed in great detail for logarithmic calculations, may be greatly simplified and facilitated by the use of a slide-rule accurately divided. The one I used was not very accurately divided, and was only two feet in length."

"*On the Examination of Water for Organic Matter,*" Part II., by Dr R. Angus Smith, F.R.S.

At present the conclusion only is given, as no abstract was prepared.

The following may be considered as a summary of the results required for sanitary purposes.

1. Quality of the organic matter, *i.e.* what is produced by standing under favourable circumstances for developing vegetable or other life?

2 and 3. Condition of the organic matter. Products of decomposition. Easily decomposed organic matter. These two can be estimated for sanitary purposes sufficiently by permanganate of potash.

4. Nitrates as remnants of organic matter.
5. Nitrites as remnants of organic matter.
6. Chlorides as indicating animal sources.

7. Oxygen as indicating activity of decomposition or destruction.
8. Total organic matter and ammonia, by weighing and other methods.

PHARMACEUTICAL SOCIETY.

Wednesday Evening, February 5, 1868.

G. W. Sandford, Esq., *President, in the Chair.*

The minutes of the preceding meeting were read and confirmed. The thanks of the meeting were given for several donations to the library, and the President directed attention to a fine collection of drugs from North America, which had been presented to the Society by Mr. William Procter, jun., of Philadelphia, who is an honorary member of the Society.

Professor **Bentley** said the collection was a very interesting one, especially as American remedies had lately been brought so prominently under our notice. The specimens would be placed in the museum for examination.

Mr. H. S. **Waddington** read a valuable paper on "*Micro-Sublimation*," in which he gave the results of his experiments with a number of the alkaloids, such as santonine, salicine, narceine, papaverine, cinchonine, narcotine, strychnine, iodine, etc. Some very interesting slides were on the table illustrating the results of Mr. Waddington's researches, which were examined under the microscope by the members before and after the meeting.

The **President**, in thanking Mr. Waddington for his excellent paper, expressed his pleasure at seeing Dr. Guy present, who had devoted so much time and attention to the subject of sublimation.

Dr. **Guy** said the Society was under great obligations to Mr. Waddington for his paper, and referred to the beautiful specimens which he had seen before the commencement of the meeting. He had obtained some very fine ones himself, but only after thousands of experiments. He attached the greatest importance to the subject, and believed that greater results would be obtained by pursuing it still further.

Dr. **Attfield** made some remarks on the subject, and said that their warmest thanks were due to Dr. Guy and Mr. Waddington, for the fresh facts they had brought before them; several bodies which were believed to be fixed were now found to be volatile.

Professor **Bentley** read a paper contributed by Mr. Broughton, B. Sc., F.C.S., on a "*False Cinchona Bark of India*," at the conclusion of which

Dr. **Attfield** read a paper on the "*Preservation of Syrup of Iodide of Iron*," by Mr. T. B. Groves, F.C.S., who has for some time been engaged in devising means for preserving the syrup. He had found that it kept better when made with iron filings instead of pure iron in the form of wire, which he atributed to the presence of impurities in the filings. He had added dilute sulphuric and phosphoric acids as preservative agents, and had obtained successful results with them. Mr. Groves prepared a number of specimens of the syrup, and to one he added 1 minim of dilute sulphuric acid to the oz.; to another, 2 minims of dilute phosphoric acid to the oz.; to a third 2 minims of dilute phosphoric and 1 minim of dilute sulphuric acid to the oz.; and to another specimen 8 drops of phosphoric acid. He had found that phosphoric acid was the only acid to be relied on, and it was very necessary not to add the acid before the syrup had cooled.

The **President** said that as Dr. Redwood had assisted in compiling the present Pharmacopœia, he would, perhaps, give them his opinion respecting the method proposed by Mr. Groves.

Dr. **Redwood** said that he was not at all prepared to admit there was any occasion to make the alteration; the syrup of the British Pharmacopœia would keep for any reasonable time if properly prepared.

Mr. **Ince** greatly disapproved of such an addition, and thought it quite unnecessary. He had for a long time, before the Pharmacopœia was issued, made it according to that form with the most satisfactory results. The Pharmacopœia form was the same as that of the French Codex, which had always given good results.

Mr. **Gale** had adopted the form given in the present Pharmacopœia for ten years, and he had always found it successful; the syrup would keep well for six months.

Mr. **Wood**, of New York, could not agree with all the remarks he had heard from Mr. Ince and Mr. Gale; he had found that if the syrup of iodide of iron was kept longer than three months, a layer was formed on the surface.

Mr. **Umney** had also found a layer on the surface after three months; the syrup would keep very well for that time by putting it into bottles while hot.

Dr. **Attfield** then explained a simple mould for suppositories which had been forwarded by Mr. Laird of Dundee. The idea suggested itself to him when witnessing the preparation of gelatine pastilles at Keiller's marmalade manufactory.

The **President** said that, as the hour was late, the reading of the other papers must be deferred till the next meeting, which would be held on the 4th of March.

CHEMICAL SOCIETY.

Thursday, February 6th, 1868,

Dr. Warren de la Rue, F.R.S., etc., *President, in the Chair.*

The minutes of the previous meeting were read and confirmed, and the donations to the library announced. The candidates for admission into the Society were B. H. Paul, Ph.D., 8, Gray's Inn Square; Edward Dowson, M.D., 117, Park Street, London; Thomas William White, Ifield, near Crawley, Sussex; and for the second time was read the name of Mr. Martin Murphy, Royal College of Chemistry, Liverpool.

Mr. Reinhold Richter, of the Rothamsted Laboratory, was proposed by the Council to become an Associate. The following gentlemen were balloted for, and duly elected Fellows of the Society, viz.: John Wallace Hozier, B.A., Oxon, Lieutenant 2nd Dragoon Guards, Staff College, Farnborough; Herbert McLeod, Assistant Chemist in the Royal School of Mines, 61, Bridge Street, Southwark; Robert Schenk, 10, Hanover Place, Kennington; and Thomas Charlesworth, Leicester.

The adjourned discussion upon Dr. Frankland's new method of "*Water Analysis*" was resumed, and occupied the first hour of this evening's proceedings. From the press of other business—Dr. Russell's lecture, and two papers to be read—the discussion of this important subject was unduly curtailed.

Professor J. A. **Wanklyn** commenced by replying to Mr. Dugald Campbell, and controverting the accuracy of his statement relative to the evolution of ammonia when albumen was boiled with carbonate of soda. With respect to Mr. Abel's objection, the speaker stated that the peculiar feature of his proposed method of distillation with an alkaline permanganate, was that under its influence the organic nitrogenous matters present in the water were measured by the amount of "albuminoid ammonia" formed, and so long as the ratio remains constant and known, it matters not what is that proportion. Dr. Frankland's statement of the amount of organic nitrogen present in a water was unsatisfactory; he did not say in what form it occurred, whether, for instance, as uric acid or kreatine. In some of the waters lately reported upon, it might, on the other hand, be said that nitrogenous matters occurred in quantities equivalent to 23 milligrammes of albumen. Granting that Dr. Frankland could tell the amount of organic nitrogen existing in any given sample of water, what more could he tell? The nitrogen was probably distributed in various forms of organic combination, and some of these might be more hurtful than others, but the speaker had examined a great number of such compounds, and none failed to give "albuminoid ammonia" upon distillation, and he therefore claimed the credit of suggesting that a new fundamental datum should be employed. Mr. Philip Holland had since proposed, in the Chemical News, the adoption of a new mode of stating the results, according to which the amount of organic impurity in a water would be represented in degrees. There was also the testimony of Professor Way to the effect that "these ammonia values are a sure index of the quality of a water." The speaker characterised the new method of analysis as unsound, requiring a great length of time for its performance, and a high degree of manipulative skill; a litre of water took five days to evaporate *in vacuo*, or ten hours over a water-bath, and then the examination of the residue gave an inaccurate estimate of the amount of organic nitrogen originally present in the water, whereas the method he and Mr. Chapman described was applicable to the water itself and could be carried out in an hour and a half.

Mr. E. T. Chapman considered that the adoption of Dr. Frankland's new process of analysis depended upon the establishment of the following propositions—

1. That nitrates and nitrites are completely decomposed by sulphurous acid.
2. That none of the organic matter suffers decomposition.
3. That the ammonia is perfectly retained.
4. The determination of organic carbon demands its non-volatility in contact with sulphurous acid and sulphites.

In examining these points *seriatim* the speaker found that the decomposition of the nitrates was incomplete or uncertain; for on subjecting the fixed residue of the water to the subsequent action of aluminium and pure hydrate of soda (obtained from sodium), there was ammonia formed in small quantity from that portion of the nitrate which escaped previous destruction. Results obtained in the examination of the pump waters of Portland Street and Bartholomew Lane were quoted, as also an artificial sample of South Essex water. In all these cases some ammonia was indicated by the Nessler test; but inasmuch as the alkaline sulphites interfered, a preliminary distillation with caustic alkali was always resorted to. From Mr. Wanklyn's experiments it appears that the ammonia is not perfectly retained during the preliminary evaporation of the water, but that a loss even of one-third may be experienced. Notwithstanding this loss, however, the discrepancy between their own and Dr. Frankland's results was always on the side of excess, not in defect, and the error was often larger than the total amount of organic nitrogen said to be present in the water. In some cases organic nitrogen had been indicated by the ammonia process when none was obtained by the "gaseous method."

Mr. Dugald Campbell would not occupy the time of the Society by any remarks upon Dr. Frankland's processes, which he thought required more study, consideration, and experiments than appeared to have been bestowed upon them by the gentlemen who have just spoken, but would confine himself to making a few remarks upon his own experiments upon water containing known quantities of urea and albumen,* referred to by these gentlemen. The Society will not fail to remember that Mr. Wanklyn, on behalf of himself, Mr. Chapman, and Mr. Smith, read a paper before it in June last, in which it is stated that solutions containing urea and albumen when distilled with the addition of sodic carbonate, two grammes of carbonate to a litre of water, yielded up all the nitrogen of the urea as ammonia, leaving untouched the nitrogen of the albumen to be afterwards acted upon by caustic potash, and ultimately by permanganate of potash, in order to obtain all the nitrogen as ammonia from the albumen; and in the paper it is distinctly stated that these results were arrived at by "direct experiments in which a known quantity of urea, gelatin, and albumen were taken."† Upon that occasion he (Mr. Campbell) had remarked that this was not his experience, and that he had never, when operating upon solutions of urea in any quantity with sodic carbonate, been able to decompose the urea thoroughly in the way they said they did, and likewise, that he never had distilled albumen with sodic carbonate without obtaining ammonia from it; both these statements were contradicted at the time by Mr. Wanklyn and Mr. Chapman, the latter gentleman detailing an experiment wherein he had acted upon a known quantity of urea with sodic carbonate, and had obtained from it all the nitrogen as ammonia: how this agrees with what is afterwards stated by these gentlemen will be seen. These results being so diametrically opposed to his (Mr. Campbell's) own experience, he was induced to make some further experiments, but before doing so he thought it advisable, as he had been operating upon rather strong solutions, to write to Mr. Wanklyn and ascertain from him what strength of solutions of urea and albumen he should employ in order to obtain results such as it was stated had been obtained by himself and colleagues; to this he got no reply, but Mr. Wanklyn called upon him in a few days, and after discussing the question, he (Mr. Wanklyn) was of opinion that their process would be fairly tried if solutions were made with fresh white of egg containing not more than 1-10th of a grain of dry albumen in a gallon of water, and also with not more than the 1-20th part of a grain of urea in a gallon of water; and these proportions were at the time written down on a slip of paper by Mr. Wanklyn, which was in the possession of his (Mr. Campbell's) assistant until recently, but cannot now be found. From his (Mr. Campbell's) experiments above referred to, he proved that solutions of urea generally were not perfectly decomposed by sodic carbonate, as stated by Messrs. Wanklyn, Chapman, and Smith, and also that all solutions of albumen when distilled with the quantity of sodic carbonate stated by them give off some nitrogen as ammonia. To meet the first case, Mr. Wanklyn now states that pure urea in water does not give off ammonia to any great extent when boiled with sodic carbonate and caustic potash, and that albumen when boiled with half the quantity of sodic carbonate they originally proposed, only gives off a small percentage of the nitrogen in the albumen. This proves exactly what he (Mr. Campbell) had stated on hearing their paper read, namely, that sodic carbonate did not entirely decompose urea, and that when he distilled albumen with sodic carbonate with the quantity of that reagent used by these gentlemen, he never failed to get ammonia evolved. It is rather a remarkable circumstance that Mr. Wanklyn, commenting upon his (Mr. Campbell's) paper when read at the British Association at Dundee, should then have "questioned the purity of the urea employed by Mr. Campbell," and should now write "that the urea occurring in waters contaminated with sewage is not pure urea, and that the circumstance that extreme purity imparts to urea a power of resistance which impure urea does not possess does not in the least degree impair the applicability of our method to natural waters,* this proposition he (Mr. Campbell) ventures to think may be quite satisfactory to Mr. Wanklyn and his colleagues, but would not after a careful consideration of all the circumstances of this case, from first to last, be acceptable to many members of this Society without proper experiments and data fully detailed, and in such a manner as to be capable of being checked by independent operators. In his (Mr. Campbell's) experiments before referred to, every endeavour was made to arrive at the truth, and if they were inaccurate to any great degree, which he can scarcely believe they were, it must be owing to the distilled water with which the standard solutions were made not having been perfectly free from ammonia, although the water he used was tested carefully for ammonia and showed none; but since he had made these experiments he had been preparing some others, but had been prevented completing them from finding it difficult to obtain water on a large scale, which, when more rigorously tested, and in a manner diffe-

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* Paper read before the British Association at Dundee. See Chemical News, vol. xvi., p. 139, et seq.—(*Amer. Repr. Nov.* 1867, *p.* 259.)

† The "Journal of the Chemical Society," ser. 2, vol. v. p. 448.

* The "Journal of the Chemical Society," vol. v., second series, p. 594.

ently to what was done at the time he made his experiments, was perfectly free from ammonia. Without this he was disinclined to proceed, but he hoped to be able to do so soon, when he would be in a position to lay these experiments before the Society.

Dr. **Frankland** could not presume to reply to all the objections raised by Messrs. Wanklyn and Chapman, but would appeal to experiment and not to argument. Since the last meeting he had, with the assistance of Mr. Armstrong, made further experiments in certain directions which were confessedly imperfect. His results were suspected of being erroneous on the side of excess, because the permanganate method furnished lower numbers in all instances, but the speaker thought he should be able to show proof to the contrary by an appeal to figures. Here were some experimental results with artificial waters:—

In 100,000 *parts of Water.*

Expt.	N. by Permanganate.	Organic Nitrogen.	N. as Nitrates and Nitrites.
I.	·016	·068	·015
II.	·016	·042	·006
III.	·022	·076	nil.
IV.	·308	1·015	nil.

No. 3 was a peaty water, made by infusion of the peat cut five feet deep at Preston, in Lancashire, and the last was a much stronger decoction of peat prepared with the aid of an alkali. The figures in the second column were obtained after treatment with sulphurous acid, and evaporation *in vacuo*. Some of this solution was then precipitated by sulphurous acid, and the resultant solid peaty matter was examined both by the permanganate and by the combustion method, when exactly twice as much nitrogen was indicated by the latter. The residual filtrate from the last product was likewise treated comparatively, and gave nearly three times as much nitrogen gas as that furnished by distillation in the form of ammonia. A still stronger infusion of peat evaporated over a steam-bath gave the numbers ·422 and 1·175 respectively.

Proceeding now to natural waters, Dr. Frankland stated that the Thames showed abnormal results during the last month (January), due to the circumstance of the river overflowing its banks, and becoming not only very muddy, but highly charged with organic matters. As before, comparative experiments were made on identical samples.

Water Supply.	Quality.	N. by Mn.	Org. N.
Chelsea........	Muddy	·011	·058
West Middlesex	Clear	·012	·027
Southwark	Very turbid	·024	·061
Grand Junction.	Clear	·006	·031
Lambeth	Turbid	·030	·062

Next, with reference to the destruction of nitrates and nitrites by sulphurous acid, the speaker was to some extent prepared to admit the force of Mr. Chapman's objection; for on treating a mixture of nitre and salt with sulphurous acid only, one-third of the nitric acid was expelled, but if phosphoric acid, or ferric chloride were at the same time present, all the nitrogen of the nitrates would be expelled. The action of phosphoric acid in this case was not easily explained. Dr. Frankland concluded with a description of the behaviour of nitrogenous alkaloids under both circumstances, thus:—

	Ammonia obtained.	
	By permanganates.	By combustion.
Strychnine	·00032	·00101
Narcotine	·00031	·00068
Sulphate of Quinine.......	·00073	·00128

Such were the series of numbers afforded by the two methods of analysis; for his own part the speaker was not at all surprised at the want of accordance manifested in the several instances brought forward. He knew of no precedent in organic chemistry which would lead him to believe that nitrogenous matters should split up in a definite manner, and furnish always the same proportion of ammonia when attacked by oxidising agents. Mr. Wanklyn and himself had both encountered the same difficulty in attempting to fix the nature of the organic matters occurring in samples of water; but at any rate we were not sure of its existence as albumen, and, until something more definite were proposed, he should not be inclined to abandon the combustion process.

Dr. **De la Rue** said that this discussion furnished evidence of the importance to be attached to a right appreciation of the results of water analysis, and no method was now brought forward but it was immediately sifted and discussed, and the truth ultimately elicited. This was a confessedly difficult branch of analytical chemistry, and there seemed to be opportunities of further research.

Dr. **Attfield** had placed upon the table a sample of water from Jamaica, the constitution of which was very remarkable, and the flavour peculiar. It contained:—

	Grains per gallon.
Chloride of calcium	1500
Chloride of sodium	1000
Chloride of ammonium	2½

The **President** also referred to a small but powerful voltaic battery of ten cells, constructed by Dr. Hugo Müller and himself, upon a new principle. The negative element was chloride of silver fused around a central silver wire, which served as conductor; this was bent over and connected by means of a small caoutchouc band or collar, to a rod of zinc, which need not be amalgamated. The exciting liquid was salt water, which in course of time became charged with chloride of zinc, and only required to be renewed when metallic zinc commenced to deposit on the negative plate. Ten of these little couples, three inches or less in height, were mounted on a wooden frame supported and sliding upon glass uprights, so that the battery was very easily put in action; and its tension was so great that a cubic inch of the mixed gases was given off from water in about twenty minutes. Mr. Gassiot thought somewhat highly of this arrangement, and the President was now having made for further trial a battery of two hundred cells.

Dr. W. J. **Russell** then proceeded to deliver a lecture "*On Gas Analysis*," the report of which must stand over until next week.

An important paper, which Dr. Frankland characterised as describing "one of the greatest triumphs of modern synthetical chemistry," was next read by the Secretary. It was communicated by Professor H. Kolbe, and entitled "*Reduction of Carbonic Acid to Oxalic Acid*," by Dr. E. Drechsel. A mixture of pure sodium and dry sand was heated in a flask to the boiling point of mercury, and a rapid stream of dry carbonic acid passed. After a few hours the silvery aspect of the metal changed to a red mass, and ultimately became nearly black; towards the end the heat should be moderated to avoid reduction to carbon, and the whole slowly cooled. Left in the air for the sodium to oxidise and then exhausted with water, it furnished a solution containing oxalate of sodium. From ten parts of sodium one part of calcic oxalate was obtained. Potassium amalgam containing 2 per cent. of the alkali metal acts in the same way.

A paper by Mr. W. H. **Perkin**, F.R.S., "*On some new Benzylic Derivatives of the Salicyl Series*," was then read. [An abstract of this communication will appear next week.]

The meeting was at a late hour adjourned until the 20th inst., when Mr. David Forbes, F.R.S., will deliver a lecture "*On some Points of Chemical Geology*."

Thursday, February 6th.

Dr. **Warren de la Rue**, F.R.S., &c., *President, in the Chair.*

In continuation of our report of this meeting we have now to give an account of Dr. W. J. Russell's lecture, "*On Gas Analysis*." The apparatus in its modified form as now employed by Drs. Williamson and Russell, was exhibited in the meeting room. It consisted of a wooden table on which was mounted a cast-iron mercurial trough of simpler form than that formerly described by Dr. Russell, and figured in the Chemical News, vol. ix. p. 282 (*Eng. Ed.*). The "laboratory tube," or vessel C, is dispensed with, and all the absorptions are conducted in the same tube as that in which the gases are afterwards measured. The pressure tube A, containing a standard volume of air, is retained, and all measurements are observed at uniform temperatures and at the same level. Dr. Russell described a handy little contrivance which enabled him to introduce potash and other reagents into the gas tube without admitting air or interfering with the volume of gas. This little instrument consists of an iron or steel wire, No. 9 or 10, passed through a crooked piece of glass tube and having one extremity roughened for the purpose of enabling it to hold firmly a tuft of moistened cotton wool. The glass tube ¼ inch in diameter being used as a director, the wire is pushed forward, underneath the mercury, until the cotton tuft rises above the level of the quicksilver in the absorption tube. By kneading the cotton wool in water, every trace of air could be expelled, and then the water could be displaced by potash or other solution; a little grease applied lubricated the passage of the wire through the glass tube. A number of analytical details were then given in proof of the accuracy with which a number of operations of this kind could be conducted without sensible alteration of the volume of gas. Carbonic acid introduced and then absorbed by potash (one part of saturated aqueous solution of hydrate of potassium mixed with two parts of water), caused no error, and five parts of such solution absorbed about 80 of carbonic acid. Oxygen could be easily removed by the same alkali, into which a few drops of pyrogallic acid was passed up. Olefiant gas and other hydrocarbons must be attacked by Bunsen's coke-balls, since the strong sulphuric acid would destroy the cotton; the lecturer thought, however, that gun-cotton might be used. In coal gas analysis this apparatus worked exceedingly well, and for hydrogen and other eudiometrical purposes the method of explosion was resorted to in a supplementary wooden trough, to which the eudiometer was transferred in a suitable transfer-spoon. The spindles and catgut adjustments of the old apparatus were retained, and likewise the mode of illumination and the sliding support for the telescope. Alterations of the level of mercury could either be effected by pouring in the liquid metal, or a stout glass tube sliding through a caoutchouc collar could be depressed into the cistern.

The President, in moving a vote of thanks to Dr. Russell, took occasion to notice the ingenious character of the contrivances made use of in several parts of the apparatus.

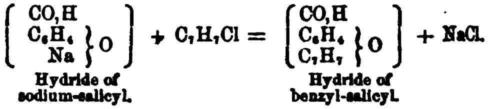
Dr. Frankland spoke in approval of the whole apparatus, and inquired whether the lecturer has constructed and used a reduced model.

Professor Wanklyn found that a piece of india-rubber tube used as a casing, overcame the objection of fragility usually ascribed to Frankland and Ward's apparatus.

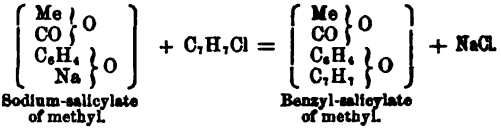
Mr. Maxwell Lyte asked whether the use of soda instead of potassa was admissible?

Dr. Russell said that the efflorescent character of this alkali and all its salts was objectionable, as tending to soil the tubes. In reply to Dr. Frankland, he would simply affirm that very small volumes of gas could be manipulated in the present apparatus, since by raising the tubes high above the level of the mercury in the cistern the volume of gas might be read off when greatly expanded.

A paper "*On some New Benzylic Derivatives of the Salicyl Series*," by Mr. W. H. Perkin, F.R.S., was then read by the Secretary. The author had examined the actions of chloride of benzyl upon the hydride of sodium-salicyl and gaultherate of sodium (sodium salicylate of methyl), respectively, and succeeded in obtaining bodies representing the salicylic aldehyde and acid, in which the phenolic hydrogen is replaced by benzyl. These new products have been named the *hydride of benzyl-salicyl*, and the true *benzyl-salicylic acid*. Combustions of these substances were made, and the ammonium, silver, mercury, lead, and copper salts of the latter were prepared and analysed. Their formation was thus explained—

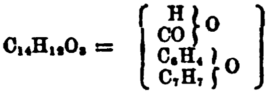
I.

$$\underset{\text{Hydride of sodium-salicyl.}}{\left(\begin{array}{l}\mathrm{CO,H}\\ \left.\begin{array}{r}\mathrm{C_6H_4}\\ \mathrm{Na}\end{array}\right\}\mathrm{O}\end{array}\right]} + \mathrm{C_7H_7Cl} = \underset{\text{Hydride of benzyl-salicyl.}}{\left(\begin{array}{l}\mathrm{CO,H}\\ \left.\begin{array}{l}\mathrm{C_6H_4}\\ \mathrm{C_7H_7}\end{array}\right\}\mathrm{O}\end{array}\right]} + \mathrm{NaCl}.$$

The hydride of benzyl-salicyl is a colourless viscid oil having an odour like that of cloves, and boiling at a point above the range of the mercurial thermometer. It is possessed of aldehydic properties, although combining slowly with alkaline bisulphites.

II.

$$\underset{\text{Sodium-salicylate of methyl.}}{\left(\begin{array}{l}\left.\begin{array}{l}\mathrm{Me}\\ \mathrm{CO}\end{array}\right\}\mathrm{O}\\ \left.\begin{array}{r}\mathrm{C_6H_4}\\ \mathrm{Na}\end{array}\right\}\mathrm{O}\end{array}\right]} + \mathrm{C_7H_7Cl} = \underset{\text{Benzyl-salicylate of methyl.}}{\left(\begin{array}{l}\left.\begin{array}{l}\mathrm{Me}\\ \mathrm{CO}\end{array}\right\}\mathrm{O}\\ \left.\begin{array}{l}\mathrm{C_6H_4}\\ \mathrm{C_7H_7}\end{array}\right\}\mathrm{O}\end{array}\right]} + \mathrm{NaCl}.$$

The benzyl-salicylate of methyl was then decomposed by boiling with alcoholic hydrate of potassium, when wood-spirit was evolved; and the potassium-salt in aqueous solution, then treated with hydrochloric acid, furnished the new acid in the form of an oil which slowly solidified to a mass of minute plates. Its fusion point is 75° C.

Benzyl-salicylic acid has the following composition—


$$\mathrm{C_{14}H_{12}O_3} = \left(\begin{array}{l}\left.\begin{array}{r}\mathrm{H}\\ \mathrm{CO}\end{array}\right\}\mathrm{O}\\ \left.\begin{array}{l}\mathrm{C_6H_4}\\ \mathrm{C_7H_7}\end{array}\right\}\mathrm{O}\end{array}\right)$$

A vote of thanks having been passed to Dr. Russel and to Mr. Perkin, the meeting was adjourned as already reported.

Thursday, February 20th.

Dr. A. W. Williamson, F.R.S., *Vice-President, in the Chair.*

At this meeting there was an unusually full attendance of Fellows, and several guests, amongst whom were Sir Roderick Murchison, Professor John Morris, and other distinguished members of the Geological Society. The limited accommodation available in the meeting room was altogether insufficient to provide for the large audience that attended on Thursday evening. This inconvenience has been sorely felt on several recent occasions, and particularly when special evenings have been appointed for lectures. It is to be hoped that the new apartments, about to be constructed for the Society in Burlington House, will meet not only present requirements, but provide for future contingencies.

Mr. Martin Murphy, of the College of Chemistry, Liverpool, was duly elected a Fellow of the Society. The names of candidates read for the first time were—Mr R. Calvert Clapham, Walker Alkali Company's Works, Newcastle-upon-Tyne; Rustomjee Byramjee, M.D., Assistant-Surgeon in Her Majesty's Bombay Army; and Edward Mensel, Ph.D., recommended by the Council as Associate. For the second time were read the names of Benjamin H. Paul, Ph.D., 8, Gray's Inn Square; Edward Dowson, M.D., 117 Park Street, London; Mr. Thomas William White, Ifield, near Crawley, Sussex; and Mr. Reinhold Richter, of the Rothamstead Laboratory, proposed as Associate.

Mr. David Forbes, F.R.S., &c., then delivered a discourse "*On Chemical Geology.*" The lecturer confined himself mainly to the consideration of those parts of the subject which comprehended the period coincident with and subsequent to that stage in the world's history known as the cosmogenetic era. Confessing at the outset that he was neither absolutely Plutonic nor Neptunic in his opinions regarding the origin of the oldest rocks, the lecturer argued that combinations of the views held by these rival schools of geology best suited the requirements of modern research. It was neither fire alone, nor aqueous agency alone, that accounted for all the natural phenomena observed, but to these forces conjointly must be added the effects of heat, electricity, light, and mechanical pressure, as greatly influencing the consideration of the matter in hand. Referring to specimens on the table, Mr. Forbes showed that silica occurred in nature as an igneous product in recent volcanic lavas; as an aqueous product in different forms deposited from solution; and as a gasolytic product in tubes from the decomposition of the fluoride of silicon. Similar modifications were observed in the case of sulphur, copper, and many other substances. The conditions under which the artificial formation of felspar and zeolites was possible were then alluded to, and stress was laid upon the fact that the production of the latter (hydrous silicates) by fire was consistent with the observation that vast volumes of aqueous vapour escape together with solid and partially liquefied matters during volcanic eruptions. Igneous action in nature was defined to be volcanic action in which the results were much modified by the presence of steam and gases. Aqueous action also was defined to include the action of dissolved saline matter, gases, air, &c., with or without heat and pressure. Going back to the earliest forms of created matter, the chemist assumes that the elements and their affinities were then the same as now, subject, however, to the disturbing causes due to excessive heat, or relative bulk; thus, whilst sodium will at comparatively low temperatures decompose carbonic acid, carbon will, on the other hand, take the oxygen from soda if the heat applied be sufficiently intense. So also with iron, which at a red heat decomposes water, whilst hydrogen at the same temperature effects the reduction of oxide of iron. Claiming a certain amount of latitude in the discussion of the states of combination or balance between the affinities of the earth's contending elements, it was conceived that the first operation of the newly created matters would be to obey the law of gravity, and arrange themselves in zones or strata in and upon the earth according to their respective densities, although modified to some extent by diffusion. Defining the relative position of the silicates, those more basic in character and of greater density underlying the acid silicates, containing probably free quartz, the chloride of sodium and other volatile compounds may be conceived to form a dense vapour or atmosphere immediately surrounding the earth, whilst carbonic acid, and, next, the gaseous constituents of air with aqueous vapour in the upper regions, were the outer zones. Later, when by the abstraction of heat the chloride of sodium was condensed, it formed a solid crust of salt upon the surface of the earth, and by a further reduction in temperature the liquefied water would dissolve the salt to form the ocean. Reasons are given for the hypothesis adopted by the author, which asserts that the central nucleus of the earth must contain an accumulation of the denser metals and their compounds; these considerations are founded upon the knowledge of the mean specific gravity of the earth, about 5·4, and the density of the exterior crust, assumed to be 2·75

By this solidification of the exterior crust, and its becoming subject to volcanic and other forces irregularly exerted, the physical features of the globe were changed from a true sphere to mountains and valleys, some of which have afterwards been covered by the ocean; and then by the disintegrating action of water, the ingredients composing the first formed rocks may have been sorted into sandstones, derived chiefly from the quartz, and the earthy silicates go to form under great pressure and metamorphic action the numerous class of slaty or argillacious stratified rocks. Metallic sulphides occurring in the silicates would by oxidation furnish sulphates, which would also be formed from volcanic emanations, and pass into the sea. Organic life at this stage came upon the scene, and was instrumental in separating the lime as carbonate from dissolved calcareous salts, thus forming limestones; whilst vegetation proceeded apace, and gradually stored up carbon from supplies of carbonic acid abounding in the earth's atmosphere, and fitted the air for the respiration of animals.

The arguments deduced from the specific gravity of the quartz contained in granite as pointing to its aqueous origin, are shown to be fallacious, and the author finds that recent lavas contain quartz of specific gravity 2·6, which exactly accords with that of the common hexagonal variety known as rock-crystal.

After duly weighing the conflicting opinions which have divided geologists as to the origin of granites, the lecturer was satisfied from his experience in the field, assisted by the microscope and laboratory, that many of the so-called granites and gneisses are really sedimentary products of the breaking up of true igneous eruptive rocks, stratified by aqueous agency, and subsequently re-consolidated. True eruptive granites of igneous or volcanic origin also undoubtedly exist, and the lecturer replied to the arguments put forward by those who dispute such an origin. The objections were taken *seriatim*—1st, That the granite contains free quartz; 2nd, That the specific gravity of the quartz is 2·6; 3rd, That the quartz contains water; 4th, That in granite the more fusible minerals have sometimes become solidified and crystallised before the less fusible ingredients; and 5th, That granite frequently contains hydrated minerals Reference was here made to Bunsen's experiments on the retention of water by hydrous sillicates, and Laurent's observations to the same effect in the case of the fused borates. A specimen of crystallized stilbite, found in the lava current from Etna, in March 1865, was exhibited; and the quartz from the volcanic lavas of Peru, and rocks of Ponza, in the Bay of Naples, were said to contain water. In the lava from Vesuvius crystals of refractory leucite were frequently found sitting upon the easily fusible augite. From the general uniformity in composition and physical characters of volcanic products thrown up in such widely distant localities as Iceland and Terra del Fuego, the lecturer argues that there must still exist a vast reservoir or reservoirs of fluid igneous matter in the interior of the earth, and that volcanic eruptions must have some intimate connection with one another. Volcanic action does not seem to be confined to mere local outbursts, for in the Pacific enormous energy is shown in the numerous volcanic islands lying between 80° and 130° west longitude, which includes a range of nearly one-seventh of the total circumference of the globe. By way of conclusion to his discourse the lecturer divided the forces determining metamorphic action into six principal classes, considered under the following headings:—

1st. Pressure alone.
2nd. Heat alone.
3rd. Heat in conjunction with chemical action and crystallisation.
4th. Aqueous action, assisted by heat and pressure.
5th. Gasolytic action.
6th. Combinations of two or more of the above agencies.

The author's aim was neither the introduction of novelty, nor the rehearsal of published opinions with a statement of authorities; but to bring together a mass of "dissociated data," examine the soundness of the separate parts, and if possible, build up a structure upon which the criticism of both chemists and geologists could be centred.

The Chairman moved a vote of thanks to Mr. David Forbes for his highly interesting communication, which bore evidence of much study and thought, and invited an expression of opinion from Sir Roderick Murchison and the other geologists whom he saw in the room.

Sir **Roderick Murchison** said he had listened with great pleasure to the able discourse just now delivered by Mr. Forbes, but felt as yet incompetent to offer any opinion upon the great chemical questions treated of in the paper, particularly those referring to the primitive constitution of our globe, in which the lecturer had grappled so manfully and so successfully with the advocates of the water hypothesis. The facts stated, and inferences deduced from the structure of rocks, go far towards invalidating the opinion of those who assert that the older granites are really sedimentary formations. The igneous origin of at least some of the granitic rocks seemed all but proved, but gneiss may be difficult to determine. With regard to intrusions several interesting examples had been mentioned. When grand ranges of limestone became suddenly changed into gypsum and dolomite, the true explanation could only be furnished by chemical investigation; Daubrée had already done much, and Forbes showed himself ready and willing to go into the arcana of these mysterious regions of speculation, and seek the truth for our science of Geology.

Prof. McDonald acknowledged himself to be a believer in the Neptunian system, and conceived that there was no clear line of demarcation between the granite proper and mica slate; he saw no reason to apply the term "metamorphic" to the latter, since the same ingredients were present in both, and the difference between adjoining portions of rock were often difficult of recognition. If the cavities in the quartz of true granite were carefully examined, they would be found lined with crystals or bounded by plates, and totally different from the hollow amygdaloid spaces occurring in volcanic lavas, and that the fluid contained in them was of an explosive nature.

Professor Morris disputed the accuracy of the inferences drawn from the occurrence of zeolites, which had in the speaker's opinion been crystallised from water. This class of minerals never occurred in modern lavas, but were generally found in the older rocks. Stilbite, chabasite, &c., may be regarded simply as modifications of ordinary felspar. The subject of metamorphism required elucidation from the chemist, and some interesting facts had been brought forward in the paper. Remarkable examples of alteration at the junction of rocks were to be seen in the passage of granite amongst limestone Where grey granite intruded into mountain limestone, the lime felspar was produced, which segregated out in different forms of metamorphism.

Dr. Hugo Muller said, "I have listened with great pleasure to the very interesting discourse of my friend Mr. Forbes, and in pronouncing my concurrence with most of the views put forth, I cannot help expressing some doubt with regard to the validity of his arguments in favour of the igneous origin of the quartz in some of the granite and similar rocks. Mr. Forbes regards the separation of graphite from pig iron, as shown in the beautiful specimen placed before us, as analogous to the separation of quartz in granite, inasmuch as both substances previous to their separation were in the state of igneous solution. Now it appears to me that this analogy is only very superficial, for in the case of pig iron we see the more infusible graphite separate in a *solid* and crystalline form as soon as the affinity to the iron ceases, whereas, on the other hand, in the case of the granite we have the undisputed fact that the more fusible felspar has separated and crystallised first; for the quartz surrounds, and, in fact, to a great extent fills up the space between the crystals or particles of felspar. On the other hand, the well defined surface of the felspar and the absence of all indication of partial interfusion on the faces of contact between the two minerals, excludes all probability that these felspar particles could ever have been in contact with fused quartz, leaving untouched the question whether the once fused quartz is capable of passing again into the crystalline state when solidifying. Mr. Forbes, in support of his views, quotes the highly interesting and important researches of Mr. Sorby on the structure of water cavities in quartz and other minerals. If I recollect rightly, the highest temperature deduced from their experiments for the formation of these cavities, is about 400° C., or about the melting point of lead. But surely this is not a temperature which we can call 'igneous,' or associate with plutonic action; it is in fact a temperature which we, if the present theory on the subject is correct, may find anywhere at a depth of about 20,000 feet below the surface of the globe, and to which in the course of time any of the sedimentary formations may have been subjected. The trachytes of Ponza and Palmarola, a rock of decided volcanic origin contains crystalline quartz, the water cavities of which are, according to Mr. Sorby, formed at a temperature of about 360° C. This fact in itself I consider a proof that their quartz is a secondary product, and could not have crystallised at the time of the eruption of their lava, for it is inconceivable that quartz could remain liquid at the temperature of melting lead. It is hardly necessary to mention that the eruption of these trachyts has taken place in pre-historic times. The fact that quartz and zeolites have been taken from the still flowing lava is not more conclusive, for it seems more than probable that these minerals, along with many others generally named amongst the Vesuvian ejections, are nothing more than particles of the ancient Monte Somma formation, underlying the present volcano, which during the eruption of Vesuvius come occasionally within reach of the lava, and are then ejected from the crater. I have arrived at this conclusion after a personal inspection of the Monte Somma formation, which in reality consist of the lavas, ashes, or tufa, and débris of the ancient volcano mixed up with occasional fragments and blocks of limestone. In the course of time a metamorphic or chemical action has set up in this mineral chaos, the result of which are those numerous well crystallised minerals which are found in such positions as to quite exclude the idea of their formation having taken place simultaneously with the Monte Somma itself. If we find these very same minerals, sometimes even in the very same kind of geodes and association in which they occur at the Somma, ejected from the crater of Vesuvius, I think we may safely conclude that they are *not* the products of the active volcano."

Dr. B. H. Paul considered that an effort should be made towards establishing the broad principles upon which chemists were required to investigate geological phenomena. Schistose rocks were found underlying, or formerly did so, other sedimentary strata. This being the case, the examination of the chemic features of difference was a matter of importance, particularly in the event of their becoming crystalline. The speaker could not agree with Mr. Forbes in considering that the uniformity was not so great in sedimentary as in crystalline rocks. The former class were remarkable for their uniformity; thus mica schist, chloritic schist, and hornblende exhibited differences only of small degree. For his own part, whilst he abandoned both the plutonic and aqueous theories, he could not adopt Mr. Forbes' reasoning in respect to the quartz in granite.

Dr. A. W. Williamson agreed with the lecturer in most of his arguments, but there was one point in his "chapter of Genesis" which seemed to require further explanation. It had been stated that in the primeval atmosphere the gases would arrange themselves, or be stratified, in the order of their density, but for his own part he should not have expected to find them in this order, but rather obeying the law of diffusion. Some time since, when visiting the blast furnaces of the Cleveland district, he was much struck by seeing a block of slag, weighing perhaps 2 tons, standing upon an iron truck, having been just run from the furnace, and whilst cooling a workman perforated the upper crust, when a stream, as of lava, flowed from the aperture, being forced out by the contraction on all sides of the mass.

Mr. Forbes, in reply, reminded Professor McDonald that he did not pin his faith to any school of geology, and, with respect to the cavities in quartz, had always found them very irregular, and certainly not, as a rule, bounded by plates or lined with crystals. Although admitting that zeolites were usually so formed, he could not agree with Professor Morris in considering that they were, in every instance, formed from solution by subsequent aqueous infiltration; although he was indebted to that gentleman for an admirable illustration in the specimen from the aqueduct of Plombières now upon the table. He once had occasion to send a mass of volcanic lava containing zeolites to a lapidary to be cut across; during the process of cutting, water had been used, and so great an action did it exert upon the mass of the rock itself, that it appeared incredible that the zeolites in its interior had been last formed by aqueous infiltration.

Mr. Forbes fully agreed with Dr. Müller that many, if not most, of the Somma minerals could not be regarded as true volcanic products, but it was far different with many of the great eruptions of quartose lavas of enormous extent occurring in other parts of the globe. For some 600 miles along the volcanic range of the Andes of Chili and Peru, quartz in hexagonal crystals occurred in the volcanic rocks, and the microscopic examination of the quartz of recent lavas by Mr. Sorby, showed abundance of "glass cavities" which could only be the result of fusion. The conjoint influence of heat, water, and great pressure, might bring about results which were impossible with heat alone; and this was in harmony with the known prevalence of aqueous emanations (steam) from volcanoes. Mr. Forbes fully admitted that under such influences, the chemical reactions in such volcanic and granitic eruptive rocks may have taken place at temperatures even below a red heat; yet considers this as no reason for not considering them as igneous, since it must be remembered that in geology the terms igneous and volcanic are synonymous. In answer to Dr. Williamson, the speaker stated that the element, time, must be taken into account in estimating the effects of diffusion; he relied upon the instantaneous production of the gases permitting them to obey the laws of gravity, in the first instance, although he admitted that any such arrangement in the atmosphere would ultimately be obliterated by diffusion.

The meeting was then adjourned until Thursday, 5th March, when the following papers will be read, viz.—"*On the Action of Oxidising Agents on Organic Compounds in the presence of an excess of Alkali*"—*Part I.* "*Ammonia evolved by Alkaline Permanganate acting on Organic Nitro-compounds*," By Messrs. J. A. Wanklyn and E. T. Chapman; "*Note on Dr. Frankland's Process of Water Analysis*," by Mr. E. T. Chapman; "*On Chloranil*," by Dr. J. Stenhouse, F.R.S.; "*Action of Nitric Acid on Picramic Acid*," by Dr. J. Stenhouse, F.R.S.; "*On the Hydride of Aceto-salicyl*," by Mr. W. H. Perkin, F.R.S.; "*On the Crystalline form of Arsenious Oxide*," by Mr. F. A. Claudet; "*On the Detection and Estimation of Nitrates in Potable Waters*," by Mr. E. T. Chapman; "*Action of Zinc Ethyl on Nitrous and Nitric Ethers*," by Messrs. E. T. Chapman and Miles H. Smith.

On Thursday, March 19th, Mr. Henry Chance, M.A., will deliver a lecture "*On the Manufacture of Glass.*"

CHEMICAL NOTICES FROM FOREIGN SOURCES.

Sulphophtalic Acid.—O. Loew. This compound is obtained by heating phtalic acid with sulphuric anhydride to 100° or 105° C. in a sealed vessel. The products of the reaction are dissolved in water, neutralised with plumbic carbonate and the plumbic sulphophtalate, which remains in solution, is decomposed with hydric sulphide. The composition of the baric sulphophtalate corresponds to the formula—

$$\left.\begin{matrix}2BaO\\HO\end{matrix}\right\}\ (C_{12}H_3)'''\left(\begin{matrix}(C_2O_2)_2\\S_2O_4\end{matrix}\right)O_2$$

—(*Ann. Chem. Pharm.* cxliii. 257.)

Phenylenediethylacetone and Ethylenediethylacetone have been obtained by G. Wischin, by acting upon phtalic and succinic chloride with zincic ethide. Phenylenediethylacetone

$$\begin{matrix}C_{12}H_4\\(C_4H_5)_2\end{matrix}\left(\begin{matrix}C_2O_2\\C_2O_2\end{matrix}\right)$$

is soluble in ether and alcohol, insoluble in water. An ethereal solution yields large crystals on slow evaporation. Ethylenediethylacetone

$$\begin{matrix}C_4H_4\\(C_4H_5)_2\end{matrix}\left(\begin{matrix}C_2O_2\\C_2O_2\end{matrix}\right)$$

is a pale yellow liquid. Neither of the two acetones combine with alkalic disulphite.—(*Ann. Chem. Pharm.* cxliii. 259.)

Action of Ethylene on Sulphuric Oxychloride.—F. Baumstark. Sulphuric oxychloride absorbs ethylene with disengagement of chlorhydric acid. A thick brown oil is formed which presently solidifies, and on being treated with water, yields anhydride and acid of isethionic, and a new acid of the composition $\Theta_2H_6S\Theta_6$. The latter is obtained as a syrup which gradually assumes crystalline condition. Its salts mostly crystallise well. If sulphuric oxychloride is treated with an excess of ethylene, it takes up 2 mol. of the latter, and a yellow oil is formed, the smell of which resembles that of oil of mustard. It boils at 154° C.; its composition is $\Theta_2H_5S\Theta_2Cl$. It decomposes on exposure to air, separating at the same time into two layers, the upper of which being an oil of the composition $\Theta_4H_{10}S\Theta_6$. The action of dry ammonia upon $\Theta_2H_5S\Theta_2Cl$ gives rise to the formation of a well crystallising body of the composition $\Theta_2H_7S\Theta_3$.—(*Zeitschr. Chem.*, N.F. iii. 566.)

Conversion of Hydrocarbons into Ketons.—E. Linnemann has continued his experiments on the conversion of monobromated hydrocarbons of the ΘnH_2n series into ketons of fatty acids of the same number of carbon atoms, and extended them to ethylene, propylene and amylene. The reaction the author makes use of consists of an oxidation by means of mercuric acetate. In the case of ethylene only traces of aldehyde could be detected. The proportions of ketone obtained from propylene and amylene were likewise very small, acetic acid being the principal product of the reaction in all cases.—(*Ann. Chem. Pharm.* cxliii. 347.)

Iodides of Organic Bases.—S. M. Jörgensen has prepared and examined the superiodides of a great number of organic bases, more especially those of strychnine and brucine, and also such of their derivatives as contain besides iodine an alcohol radical. The former were genrally prepared by precipitating a salt of the base with a solution of iodine in potassic iodide, the latter by mixing alcoholic solutions of the base, and iodide of alcohol radical. All these compounds are obtained in brilliant crystals from their alcoholic solutions. For detail the reader must be referred to the original paper.—(*Ann. Chim. Phys.* [4] xi. 114.)

Determination of Nitric Acid.—C. Nöllner. In the manufacture of potassic nitrate from Chili saltpetre liquors are obtained in which from the presence of large quantities of foreign salts the estimation of nitric acid by any of the methods commonly employed is almost impossible. The author therefore proposes the following new method. About one gramme of the solution or salt to be tested is gently heated with a concentrated solution of ammonic sulphates absolute alcohol is added, and thus all salts are precipitated with the exception of ammonic nitrate. The latter after filtration is precipitated with an alcoholic solution of potassic hydrate (free from silicic acid), and the potassic nitrate washed with alcohol, dried and weighed.—(*Zeitschr. Analyt. Chem.* vi. 375.)

Detection of Aniline in presence of Toluidine.—A. Roseustiehl. Chloride of lime produces a blue colour with aniline, and a brown one with toluidine, but a mixture of the two only shows the latter reaction. If, however, ether is added the brown substance is taken up by the latter, and the blue colour of the aqueous solution becomes visible. The test as proposed by the author therefore is: Dissolve the base in ether, add an equal volume of water, then, drop by drop, a solution of chloride of lime, shake, and observe the colour of the aqueous layer.—(*Zeitschr. Analyt. Chem.* vi. 357.)

Analysis of Silicates.—R. Hofmann. Silicates in which the alkaline metals are to be determined and which are not decomposed by acids, may be brought into solution by the combined action of ammonic fluoride and sulphuric acid. The finely powdered mineral is mixed with three or four times its weight of the fluoride, moistened with sulphuric acid, and the whole gently heated on the water-bath, finally over a flame to expel excess of sulphuric acid. The dry residue is dissolved in chlorhydric acid, and proceeded with as usual.—(*Zeitschr. Analyt. Chem.*, vi. 366.)

NOTICES OF BOOKS.

Principles of Chemistry Founded on Modern Theories. By M. Naquet, Professor Agrégré à la Faculté de Médecine de Paris. Translated from the second Edition by William Cortis, Student, Guy's Hospital. Revised by Thomas Stevenson, M.D., Demonstrator of Practical Chemistry, Guy's Hospital. London: Henry Renshaw, 356, Strand.

One of the best marked features of a truly original mind, universally denoting genius in its possessor, is the power of showing its individuality at once, and stamping with a personal authority all that is produced by it. Every one acquainted with modern scientific literature must have convinced himself by negative evidence of this truth; but the chemist, with ample materials of this kind in the ordinary run of works on chemistry, has more opportunities of estimating positively the value of originality in a text book than most scientific teachers or students. M. Naquet shows his originality, clearness of expression, and facility of explanation in his very first sentences, and by his power of blending new facts and ideas with old facts and ideas expressed in a clear and definite manner, he manages to sustain the interest of the reader from the very first words to the last. This force of language, with no disparagement to the translator (for the work is translated as well and accurately as could be), is more noticeable, of course, in the original language.

There is a style in the language of the opening sentences that reminds us of Lamartine, Guizot, or Victor Hugo—clear, precise, forcible. Interest is at once excited, and, as in a well-written tale, never flags.

"Si nous jetons les yeux sur ce qui nous environne, nous sommes frappés par la vue d'une multitude d'objets d'une diversité infinie. Tous ces objets, quels qu'ils soient, ont reçu le nom générique de corps.

"Ce qui constitue les corps s'appelle matière ou substance. D'une manière générale on peut dire, que la matière est tout ce qui frappe nos sens, et d'une manière plus scientifique, que la matière est tout ce qui obéit aux lois de la gravitation."

The original is quoted here to show the easy way in which the French language adapts itself to the expression of scientific thought; and it will be easily seen from this one specimen (space forbids more) how much grace and ease is lost in translation. A translation (we are sorry to say) was necessary, and the work could not have been more ably done than it has been by Mr. Cortis. In considering M. Naquet's book as a chemical work, and not a translation only, a clue to its character is at once given by the opening sentence just quoted. It must be classed, then, as the production of a chemical physicist, rather than that of a chemical mathematician or chemical naturalist. Matter and its properties open the chapter, and so throughout the work. To a physicist a definite law is all supreme; for him there are no exceptions, everything has definition, and must suit that definition; his teaching is absolutely material; and granting that elementary mathematics formed the basis of physics, he, nevertheless, will not allow the abstract deductions of mathematics to hold an equal value with material facts. The majority of English chemists belong to neither of the above schools, but belong rather to a naturalist class, which recognises gradation with no sudden sharply drawn boundary line. With them a sum of characters defines a position. To illustrate by an example of what is a metal and what a non-metallic body, or, as M. Naquet calls it, a metalloid. One person will argue that hydrogen is similar to silver, and that if silver is a metal hydrogen is a metal also to all intents and purposes. The naturalist will say that from the sum of characters the extreme members of the series are undoubtedly the one or the other; but that when we reach the mean members, we may as justly say silicon as silicium, arsenicum as arsenic. M. Naquet, as a physicist with his definite rules, gives six characters as follows for an absolute determination:—

METALLOIDS.

I. Several metalloids are gaseous.
II. Metalloids have not the lustre called metallic.
III. Metalloids are bad conductors of heat and electricity.
IV. Metalloids have a density relatively low.
V. Oxides of metalloids, on combining with water, ordinarily produce acids, seldom bases.
VI. Metalloids are *always* electro-negative in the compounds which they form on uniting with metals.

METALS.

I. There is no gaseous metal.
II. Metals possess metallic lustre.
III. Metals are good conductors of electricity and heat.
IV. Metals have a density relatively high.
V. Oxide of metals, on combining with water, produce bases, seldom acids.
VI. Metals are *always* electric-positive in the compounds which they form on uniting with metalloids.

Thus the first and the sixth characters are given as absolute. It will be seen at once, however, that there is a very considerable element of relativeness in these absolute characters. If the gaseous state be a standard, mercury is more accurately a metalloid than carbon is; and electro-positive and electro-negative are surely relative terms, and, in a chemical classification as such, should not have undue weight. As the result of this arbitrary classification to the neglect of a sum of characters, tin, titanium, thorium, antimony, bismuth, uranium, tantalium, and niobium figure as metalloids. Carbon is not to be found in either list, we suppose from inadvertence; nevertheless, it might afford difficulties to the theoretical classifier. It surely would be better, in the face of these facts, to abolish the meaningless expressions, metals and metalloids, altogether, than do violence to our preconceived ideas with no satisfactory result.

The above is only an instance of a plan generally followed of harmonizing ancient with modern ideas. Thus, in nomenclature when the old chemistry is done violence to, in accordance with modern ideas, for a given rule of four lines, we find four exceptions that require for a terse explanation as many pages of the volume. The consistency of the resulting nomenclature will at once be seen by glancing at the following list:—Mercuric nitrate, mercurous nitrate; mercuric chloride, protochloride of mercury (calomel); biniodide of mercury, protoiodide of mercury; protosulphide of mercury, subsulphide of mercury, mercuric sulphate, subsulphate of mercury, for the two sets of salts respectively.

We venture to say that the "safe middle course" of different systems of nomenclature will satisfy no English chemist, and that many modern English text books are in this respect decidedly superior to that of M. Naquet, both in consistency and simplicity.

But not to prolong our criticism further, we will freely accord to M. Naquet what is claimed for him by his translator—that he "explains and gives with great ability the arguments for and against his theories and those of other chemists." M. Naquet designed it for the classic text book of the French student of medicine; "the work only pretends to be a point of departure." "In our opinion students enter upon a false path when they neglect a knowledge of laws to gain *simply* an acquaintance with a number of facts, with which they uselessly overload the memory." Now, we must do the English medical student the justice to say that in very exceptional instances does he overload his memory with chemical facts; whether usefully or not we will not pretend to decide. It seems to us that the work is adapted for those who have a sound knowledge of chemistry, whether it be ancient or modern. In this case any bias will be avoided. In fine, the book before us is not solid enough for a foundation; but the thoughts suggested by every page are necessary for a completion of a chemical education. To go further, in one sense, this text book of chemistry is more than any other suited for the medical or chemical student's training; for its mastery he must use his brains and enlarge his ideas; but it should not be used too early in his career. We will add that, if the French medical student is accustomed to express himself as follows: "The coefficients representing the quantities of salts decomposed in two saline couples, containing the same radicles grouped in inverse order, are complementary;" he is infinitely superior to his ordinary British representative, who has not yet mastered the law of Volkmann, who stated that the frequency of the pulse in man, as connected with his stature, was in the ratio of the ninth root of the fifth power of the height.

This translation of M. Naquet's book has been conscientiously and accurately done. A great need felt for it among certain students will be amply satisfied; and as a further recommendation to a very well printed and got-up book, the casts of the original plates of M. Kekulé's diagrams have been used.

A Manual of Inorganic Chemistry, arranged to facilitate the Experimental Demonstration of the Facts and Principles of the Science. By Charles W. Eliot and Frank H. Storer. Second Edition. London: John Van Voorst. (Pp. xiv. and 605).

The favourable reception awarded to this work in America is doubtless the immediate cause of its re-publication in England. Its plan, moreover, differs in several essential points from that usually adopted by the compilers of elementary chemical manuals in this country, since it is primarily designed directly to accompany a course of practical study in the laboratory. In some respects it resembles the once popular manual of Stoeckhardt; indeed to this book the authors avow their obligations for many experimental details. The work is not written in support of any particular theory, or in the interest of any one system of notation, the fundamental idea of its authors having been to facilitate the acquisition of a knowledge of inorganic chemistry as far as possible by, as they say, the experimental and inductive method. To this end they give a large number of experiments simple and inexpensive to perform, although perfectly adequate to demonstrate the leading facts and generalisations of the inorganic portion of the science, for the authors plainly indicate that much of the complicated paraphernalia with which our modern lecture-rooms are equipped is by no means absolutely necessary to elucidate the radical laws and principles of chemistry. These experiments are intended to be made by the student himself, and in general sufficiently minute instructions are given to insure their successful performance. We cannot, however, always compliment the authors on the elegance or clearness of the style in which these instructions are conveyed; but as an example of the character of the experiments we quote the following method of demonstrating that ammonia is actually produced from materials which are proved to generate a mixture of hydrogen and nitrogen, since a more direct synthesis is still a desideratum (p. 79).

"Exp. 45. Place in an ignition-tube an intimate mixture of 3 grammes of fine iron filings with 0·2 grammes of caustic potash; adapt a delivery-tube to the ignition-tube, heat the contents of the tube over the gas-lamp, and collect the gas which escapes in a test-tube over the water-pan. Examine this gas, which will prove to be the inflammable hydrogen. Caustic potash, as we have already learned (p 74), consists of potassium, hydrogen, and oxygen; at a high temperature, metallic iron is able to seize upon a portion of the oxygen in this compound, setting free hydrogen, which finds no place in the new combinations.

"Exp. 46. Heat in a second ignition-tube, similarly disposed, a mixture of 3 grammes of fine iron filings and 0·2 grammes of nitrate of potassium, and collect the gas, as before, over water. This gas has neither taste nor smell, and when tested with a lighted splinter it is found to be uninflammable, and in fact to extinguish the taper. It is nitrogen. Nitrate of potassium contains, as has been already stated (p. 75), potassium, nitrogen, and oxygen; at the high temperature employed, the salt is partially decomposed, the metallic iron combines with the oxygen of the nitrous vapours formed, and their nitrogen is set free.

"Exp. 47. In a third ignition-tube, heat the same quantities of the same materials which have been used in the last two experiments, at once and together. A delivery tube is not necessary in this case; the tube may be held by the wooden nipper by the open end. Neither hydrogen nor nitrogen will be evolved as before, but instead of them we have ammonia, whose presence may be manifested by holding a bit of reddened litmus paper at the mouth of the tube. The intense alkaline reaction of the gas, and its odour, sufficiently distinguish it from both hydrogen and nitrogen."

The authors, however, clearly discriminate between chemistry and chemical manipulation, and give, in the form of an appendix, very practical instructions on the latter subject.

Many arguments may be adduced in support of such a method of instruction—arguments, too, which would probably have greater weight now than formerly, when the practical study of physical science was ignored and banished from the ordinary *curricula* of our schools. Not the least weighty of these arguments will be found in the incontestable fact that this method necessarily tends to discipline and develope the student's perceptive faculties—one of the capital results of a well-devised method of teaching physical science. The student is enabled directly to examine for himself—to see, smell, and handle for himself—and he thus becomes acquainted with the main facts of the science by a process similar to that by which the facts themselves were originally perceived and established. Every one will readily grant to the authors that scientific study fails of its true end if it become a mere exercise of the memory. Moreover, such a method, above all, materially facilitates the attainment of a definiteness and exactitude in the knowledge of the subject, without which, as an authority on this matter has recently declared, the introduction of physical science into our school system is worse than useless; and although chemical lecture illustration has never been carried to such a degree of perfection as at this time, when our professors appear almost to base their reputation, in the lecture theatre, on the brilliancy and effectiveness of their demonstrations, we venture to assert that under the present system of science tuition this exactitude is by no means so generally acquired as it ought to be. The result to the student is not commensurate with the labour of the teacher. Doubtless, the evil to some extent is inherent in the system, but much of the ill-success, it seems to us, is to be directly ascribed to the student. The almost universal complaint of teachers, even in this age of the multiplication of manuals, is that students, as a rule, will not sufficiently study their text-books. An undue prominence is given to the teaching in the lecture-room; by some it is invested with a value which it cannot in strict reason intrinsically possess. There is a proneness to regard the text-book as merely supplementary to the lecture. Even the most conscientious students err in considering they gain their object merely by a regular attendance in the class-rooms, unremitting attention to the lecturer, and a careful transcript of their voluminous notes at leisure. But how is it possible for the student mentally to digest the lecture when his sole aim is apparently to get a verbatim report of it? How frequently in the hurry of mechanically noting the definition of a principle, or the description of a property, does the student miss the point of the experiment by which the one or the other is intended to be illustrated? That *ex abusu non arguitur ad usum* everybody knows; but the practice of note-taking is often carried to an injudicious excess, and operates injuriously against both the teacher and the taught.

Hence it appears to us that the method employed in this book will go far to obviate this tendency, and we venture to predict that manuals based on this or a similar plan will multiply with the more general introduction of the practical study of physical science into our school system.

The authors presuppose the students of their manual to be already acquainted with the rudiments of physics, and, therefore, contrary to the practice which obtains in England, they plunge at the very outset *in medias res.* Still, as they have thought fit to recapitulate at length many of the physical properties of bodies, it would, we take it, have been well to have directed the student's attention to the natural effects of many of these properties—to the beneficial consequences resulting from the singular anomaly of water possessing a condition of maximum density, for example. And, as the question at issue between Tyndall and Magnus is apparently settled at last, we also regret that all mention of the effect of atmospheric moisture in retarding and modifying the intensity of solar radiation is omitted. Every true student of physical science knows the quiet innate sense of enjoyment to be derived from the knowledge and contemplation of such phenomena.

We observe that the statements of the older manuals with respect to the existence of definite hydrates of the so-called "mineral" acids are repeated, although Roscoe and Dittmar showed some years ago that the uniformity of the composition of these bodies was only apparent, and in reality an accidental circumstance depending simply on the pressure under which their distillation had been effected; they proved that for every pressure an aqeous solution exists which, when distilled under that pressure, possesses a constant boiling point, and fixed composition.

The chapter on antozone is mainly made up of the vague and unsatisfactory statements of Meissner and Houzeau. The authors, however, plead in extenuation for thus setting forth whatever is known respecting antozone, "the impossibility with so obscure a subject of making such a just discrimination between salient and unimportant points as with a well studied subject is both easy and desirable." In our opinion, there is but little satisfaction to the unfortunate student who is thus shown "how vague and uncertain the prospect is when once the narrow limits of established knowledge are past, and the inquirer ventures out into the obscurity which perpetually separates the knowledge of to-day from that which shall be knowledge to-morrow" (p. 154).

In taking leave of a work to which it gives us pleasure to direct attention, we cannot refrain from quoting the following just discrimination of the relative position and value of fact and theory:—"In the midst of the doubts and discussions which to-day envelope chemical theories, the student will do well to remember that all these questions lie without the sphere of fact. They do not affect the actual composition of properties of a single element or compound; they are questions of interpretation, classification and definition. The existence of atoms is itself an hypothesis, and not a probable one; all speculations based on this hypothesis, all names which have grown up with it, all ideas which would be dead without it, should be accepted by the student provisionally and cautiously, as being matter for belief but not for knowledge. All dogmatic assertion upon such points is to be regarded with distrust. The great majority of chemists, devoted to the applications of chemistry in mineralogy, metallurgy, dyeing, printing, and the manufacture of chemicals, remain completely indifferent to discussion of chemical theories. Hence the student will find that in technical chemical literature the older notation and the corresponding smaller atomic weights are almost invariably employed. Theories, however, are of great importance to the progress of the science and to the clear ordering of the ground already won. It is on this account very much to be wished that the great attention now devoted to the discussion of the best methods of representing symbolically the constitution of chemical substances and the changes to which they are subject may result in the elaboration of a system good enough to command general acceptance."

CORRESPONDENCE.

Water Analysis.

To the Editor of the Chemical News.

Sir,—Will you allow me to state, that after a careful consideration of the points in my report of the Chemical Society, alluded to in the letters which appeared in last week's Chemical News (*American Repr.*, *March*, 1868, *page* 148). I am convinced that the report was substantially accurate. With regard to the point raised by Mr. Thorpe, discussion upon an error (granting such to be the case) affecting himself only, is now futile, since he has corrected what he considers to have been misinterpreted.—I am, etc.,

The Reporter.

Impurities in Glycerin.

To the Editor of the Chemical News.

Sir,—The writer of the article on Glycerin in *Kunst und Geweberblatt* is correct in attributing the acrid, irritating properties of some glycerin to the mode of preparation; but I have seen distilled glycerin which was quite as unsuitable for medicinal or surgical purposes as any spoken of. The volatile fatty acids, and ethers, which exist in crude glycerin, are sometimes condensed with the glycerin, and these have very irritating properties.

In the glycerin which is made without distillation, the volatile acids and ethers exist, but not in the same state as after distillation, the high heat required for this process decomposing them into some modification of their original state.

The great cause of irritation in glycerin which has not been properly prepared, is the presence of oxalic acid and of formic acid; these are produced by the action of sulphuric acid upon the glycerin, forming the first-mentioned acid, and this in its turn acts upon the glycerin, giving rise to formic acid, the irritating properties of which are well known.

The nitrate of silver test I have always considered the most reliable. Glycerin which shows no reaction with this salt may be considered suitable for all uses, as it indicates not only the presence of chlorine or chlorides, but is, as well, reduced by acids, which may exist in the glycerin.—I am, etc.,

Henry Bower.

Philadelphia, January 16th, 1868.

Beet-Root Sugar.

To the Editor of the Chemical News.

Sir,—Your foreign correspondent in yesterday's number remarks, "as the manufacture of beet-root sugar is not an English industry, an abstract of this memoir would probably possess little interest for your readers." (*Am. Reprint, April,* 1868, *page* —.) As the manufacture of beet-root sugar will probably be commenced this year, in more than one locality in England, all information respecting it will be peculiarly valuable at a time when the best processes should be at once adopted. Your correspondent in Paris, by communicating the earliest information, will probably be conferring great benefit on an industry which will in all probability soon become of national importance.

If you can obtain for me the title of the best and most recent French works on the growth of the sugar beet, and the manufacture of sugar from it, you will greatly oblige.—I am, etc.,

ROBERT OXLAND.

Compton Gifford, Plymouth, February 8th, 1868.

The Recent Discussion at the Chemical Society.

To the Editor of the CHEMICAL NEWS.

SIR,—Having only renewed—not commenced—the discussion at the last meeting of the Chemical Society, I believe that I had no right of reply at the end of the discussion.

Mr. Campbell's remarks, however, demand an answer. Mr. Campbell stated that I had not published a single experiment in which I took white of egg, and failed to get ammonia from it on boiling with dilute solution of carbonate of soda.

I quote a passage from my paper (*Laboratory*, 28th September, 1867, page 442), and make the remark that Mr. Campbell had read that paper.

"III. A litre of spring water, 1·864 grm. of carbonate of soda, and 3·5 milligrm. of fresh white of egg (weighed on a bit of platinum foil), were introduced into a retort and distilled:—

1st distillate, 100 c.c. = 0·000 milligrm. NH_3
2nd " 100 c.c. = 0 000 " "
3rd " 100 c.c. = 0·000 " "

0·000"

This extract speaks for itself, and is surely sufficient to justify my interruption of Mr. Campbell's speech.

Equally contrary to the fact, is Mr. Campbell's representation that the dispute between us was whether traces of white of egg (not a considerable proportion) were decomposed.

If your readers will turn to Mr. Campbell's paper (*Lab.*, September 21st, 1867), they will find that according to Mr. Campbell, he got off about 33 per cent. of the total nitrogen in the form of ammonia, when he boiled 0·093 grain of (moist) white of egg with dilute carbonate of soda, and that on taking still more dilute solutions of albumen *all* the nitrogen came off as ammonia.

In my reply (*Lab.*, 28th September, 1867), your readers will find the following:—

"I have thus taken 5·00, 0·40, and about 0·04 milligrm. of *albuminoid* ammonia in the shape of white of egg, and in no case got over two and a half per cent. of the albuminoid ammonia evolved by carbonate of soda."

Mr. Campbell's paper is of a piece with his speech. According to him he took a quantity of urea, containing nitrogen equivalent to ·0062 grain of ammonia, and having boiled it with dilute carbonate of soda, then with potash, and finally with permanganate of potash, got altogether ·0061 grain of ammonia, ·0015 grain of this ammonia being evolved by permanganate. In a second experiment, he describes himself as having taken the same quantity of urea, and obtained accurately ·0062 grain of ammonia, this time ·0025 grain by the permanganate. When I add, that since the publication of Mr. Campbell's paper, the observation has been recorded, that boiling with alkaline permanganate actually oxidises urea, and evolves its nitrogen, in great part, as nitrogen gas, or as nitric acid, the character of these experiments of Mr. Campbell's will become intelligible. Notwithstanding this oxidation, Mr. Campbell finds accurately all his nitrogen in the form of ammonia.—I am, etc.,

J. ALFRED WANKLYN.

London Institution, February 8, 1868.

The Recent Discussion at the Chemical Society.

To the Editor of the CHEMICAL NEWS.

SIR,—In the report of the meeting of the Chemical Society, given in your last number, we observe that our speeches are rather inadequately given, and that inaccuracies have crept in. This is not to be wondered at, inasmuch as both of us read them rapidly from manuscript, which was afterwards (at the request of the President) handed to the Secretary for publication by the Chemical Society.

Our speeches contained matter which might have been appropriately given in answer to Dr. Frankland, who spoke later in the evening.

We wish now to make a few remarks on Dr. Frankland's speech as reported in your last number. We notice four examples of results obtained by taking what are there termed "artificial" waters, and operating on them by Dr. Frankland's method, and by our method. These "artificial waters" were prepared by treating water with peat, and were, therefore, waters containing unknown quantities of organic impurities, and consequently the want of coincidence between results got from them by the employment of the rival methods of analysis is in itself evidence of nothing beyond the fact that the methods give different results.

In Dr. Frankland's recent lecture, he gave four instances of the employment of his own method on another sort of "artificial" water, viz., on water of known composition. In these instances he dissolved known quantities of sugar, and in one case known quantities of sugar and chloride of ammonium in water, and so prepared waters containing known quantities of organic carbon and organic nitrogen.

In these cases—and these are the only published instances of a testing of Dr. Frankland's process—he had errors of about 3 cubic centimeters, 0·8 c.c. and 0·4 c.c. of carbonic acid, and in the nitrogenous instance he observed about 0·07 c c of nitrogen more than he had put into the water, and probably had committed an error of much greater magnitude.

As was said by one of us during the debate in the Society, errors of this magnitude are a satire on the claim to work to the 1-100th of a cubic centimeter, and hardly any severer censure could be passed on our method, which really does work to the 1-100th of a cubic centimeter, than for such a process as this of Dr. Frankland's to furnish results coincident or parallel with those given by it.

In reference to Dr. Frankland's experiments on the action of alkaline permanganate on some alkaloids, we have to remark that earlier in the evening one of us handed to the Secretary a short account of the action of this reagent on certain alkaloids, and on a variety of organic nitrogenous substances, and since the last meeting we have much extended these researches.

In your report, in giving Dr. Frankland's alkaloidal results, you give "Ammonia obtained." "By permanganates." "By combustion." In place of "by combustion," it should be "ammonia calculated from the formula." In point of fact, Dr. Frankland compared the ammonia equivalent to the total nitrogen of the alkaloid with the ammonia got from it by our process.

Dr. Frankland was unfortunate in his selection of strychnine, narcotine, and sulphate of quinine to exhibit want of *parallelism* between the ammonia given by our process, and the ammonia equivalent to the total nitrogen of the substance.

Dr. Frankland's numbers are:—

	"Albuminoid" NH_3	Total NH_3
Strychnine..............	·00032	·00101
Narcotine...............	·00031	·00068
Sulphate of Quinine......	·00073	·00128

The real numbers, however, exhibit the "albuminoid" ammonia as exactly one-half of the ammonia which the total nitrogen could furnish. In place of ·00032 for strychnine, Dr. Frankland should have given ·00051.

At any rate, we have obtained from strychnine exactly one-half of its total nitrogen in the form of ammonia.

The total nitrogen got from strychnine, narcotine, and sulphate of quinine, and the "albuminoid" ammonia which their alkaloids yield, are quantities parallel to one another.

In conclusion, we have to remark that we do not remember to have heard Mr. Campbell make the admission of the possibility of error (owing to the possibility of there being ammonia in the distilled water used in his former experiments), which we find at the end of his speech as reported by you.—I am, etc.,

J. Alfred Wanklyn.
Ernest T. Chapman.

London Institution, Febuary 17th, 1868.

Crystallogrophy and the Blowpipe.—Law of Horizontal Crystallisation.

To the Editor of the Chemical News.

Sir:—May I ask you to allow me to add to the paper published in last Friday's Chemical News (*Amer. Repr., April*, '68, *page* —), that having by the kindness of the Secretary to the R. A. Institution been allowed the use of their splendid compound microscope, by Smith and Beck, I have been able, since that paper was written, to examine the diaphanebulous vesicles whose crystals, appearing at first like a slight cloud, were far too minute to be distinguished by my pocket lens.

Under an object glass, magnifying 1000 diameters, the primary crystals of baryta had that peculiar hieroglyphical appearance which I have termed grammatè. Those of silver were like small flowerets, with three petals, and sulphur (whose vesicle was nebulous) appeared in myriads of tripedal marks like "crowfeet." Under the same glass the zones of the magnesian disc I found to consist of innumerable dark, if not black spots, too small for their shape to be distinguished, even by this powerful lens.

It is now, I think, evident, and I think I may fairly claim the discovery of the fact, that—"When the process of crystallisation in nature is confined to the plane of the superficies of the crystal, and not allowed to proceed in a direction either above or below it, as is the case in the thin 'walls' of the borax vesicles made by me, a distinct system of crystallisation is followed, producing forms widely differing from those generated under other conditions,—never geometrical, generally in the shape of flowers, ferns, trees, or stars, and not isomorphous."

I have the pleasure also to inform you that pieces of the crystalline vesicle can be fastened on clean smooth glass merely by the pressure of a finger, so firmly that they cannot be easily rubbed off, and may be carried about, forming excellent slides for the microscope; when no longer required, they can be washed off with soap and water. I tried electrifying the glass previously, but the vesicles being attracted electrically, they were of course soon repelled.

The truth of the above law may be easily demonstrated by an experiment which I have made since the above was written. I placed a solution of common salt in cold distilled water between two plates of glass, under a pressure of 3½ lbs.; next morning a reticulate crystallisation was observable on the inner side of both plates, while some drops of the solution, left on the platinum spatula, with which I had mixed it, had crystallised in a modification of the cube.

Nitre treated in the same way produced a kind of floral net-work, while outside it assumed the usual prismatic needles. Carbonate of soda crystallised in a very distinct dendroidate form.

It is necessary to use cold water, because if warmed with some substances, as nitre, the secondary or isomorphous crystallisation is set up so rapidly that the primary kind has not space to form.

It would appear from this that although the law of planiform crystallisation, as above demonstrated, holds good, primary crystals from solution by fire are different from those produced by a solution in liquids.—I am, etc.

W. A. Ross.

Woolwich, 24th February, 1868.

Phosphorescence of Potassium and Sodium.

To the Editor of the Chemical News.

Sir:—In your issue of January 31, 1868 (*Amer. Repr., March*, '68, *page* 144), is an extract from the *Journal für Praktische Chemie* relative to the oxidation of potassium and sodium. It is there stated that "the oxidation of potassium and sodium, when exposed with a clean surface to the air, is accompanied, according to H. Baumhaur, with evolution of light."

Mr. H. Baumhaur thinks, doubtless, that he is the author of this discovery, but his observation is, in reality, about 17 years old. In the year 1851 M. Pétrie discovered that the metal potassium is phosphorescent when exposed to the air, like phosphorus. He covered the potassium with bees'-wax, and then cut it into two. Each segment remained luminous for about half an hour, the light being one-tenth the intensity of that produced by a piece of phosphorus of the same size.

In 1859 Herr Linnemann, ignorant of M. Pétrie's observation, published another note (in the *Journal für Praktische Chemie*, lxxv.) upon the same subject. He showed that both potassium and sodium are luminous upon their freshly cut surfaces. The light emitted by potassium is of a reddish tint, that of sodium greenish, according to this author. At 60° or 70° C. the light of sodium is quite as intense as that of phosphorus at the ordinary temperature.

In 1859 I also had occasion to examine the same phenomenon, and recorded it in 1862 in a work which has been more than once quoted in your valuable journal. I found the light of sodium to be very feeble at the ordinary temperature of the atmosphere, and that it ceased when the newly exposed surfaces are covered with a layer of soda. The luminosity lasts for a few minutes, and increases in brilliancy as the temperature rises. Potassium also becomes vividly phosphorescent in the preparation of boron.—I am, etc.,

T. L. Phipson, Ph.D.

The Cedars, Putney, S.W., Feb. 12, 1868.

The Royal School of Mines.

To the Editor of the Chemical News.

Sir:—Now that the subject of technical education is under discussion, I think that perhaps it might not be amiss to say a few words about the Royal School of Mines.

One, and the principal reason why our Royal School of Mines turns out so few scientific men, in comparison with the corresponding French and German Institutions, is because it is so little known, and many who are aware of the existence of it know little or nothing of its mode of working. The School is itself well worthy of a higher reputation than is at present accorded to it; the Professors are among the most eminent men in their several departments; and the course of study prescribed for the students, extending over a period of three years, and embracing several distinct branches of science, seems to demand more general recognition as an efficient and thoroughly practical scientific education. The School at present is merely an appendage of the Geological Museum, instead of being an institution distinct from everything else, as is the case with the French School of Mines.

In 1854 the Ecole des Mines had 600 associates, whereas ours at the present day has only 40; and as it has been established 17 years, this shows an increase of only 2¼ per annum.

I have no doubt that were the Institution brought more prominently before the public the number of students would be greatly increased, and this might be done in several ways.

1st. The chemical and metallurgical laboratories, lecture theatres, etc., should be all in one building. The present arrangement involves considerable loss of time and inconvenience.

2nd. There should be a public opening of the school at the commencement of each session, and addresses should be given, the same as in all our medical schools.

3rd. The diplomas of associateship, scholarships, prizes, certificates, etc., should be awarded publicly at the end of the term; this is done, I believe, at every other educational establishment of any standing.

4th. The "prospectus" and calendar (if it be worthy the name) should be printed and kept in a separate form, instead of, as now, being printed on some of the spare pages of a pamphlet belonging to the Geological Museum.—I am, etc.,

A. L. E.

The Royal School of Mines.

To the Editor of the Chemical News.

Sir,—Will you kindly allow me to make a few remarks on some of the statements made by A. L. E. on the School of Mines, which appeared in the Chemical News of last week? —(*Am. Repr., April,* '68, *page* 196.)

A. L. E. says the School of Mines is an appendage to the Geological Museum; this is quite a mistake,—if the School had 200 to 300 students, the Museum would seem just as much an appendage to the School, as the School now seems to be to the Museum.

"The prospectus or calendar, if it be worthy of the name," writes A. L. E.; now, no one dreams of calling the prospectus a calendar, or wishes to do so (always excepting A. L. E.): as to its being printed "on the spare pages of a pamphlet belonging to the Geological Museum," this is not correct. If A. L. E. will look at the prospectus of the present session, he will find that it consists of 42 pages: the first page is the title-page; the next contains a table of contents; pp. 3, 4, and 5 contain a short account of the origin of the School of Mines; pp. 6, 7, and 8 give an account of the Geological Survey Mining Record Office and Library; the rest of the prospectus, pp. 9 to 42 (incl.), *i.e.*, 34 pages, are devoted to the School of Mines; *ergo*, the prospectus is printed on 34 pages, being spare pages of a pamphlet, consisting of three pages, belonging to the Geological Museum.

It would be better, no doubt, if the chemical laboratory was under the same roof as the Geological Museum: but this has very little influence on the number of students, its present position really producing but little practical inconvenience. The real reason of the small number of students is, in my opinion, that it is so little known to the general public. I am sure the names of the lecturers are far more widely known than the Royal School of Mines with which they are connected. If, as A. L. E. suggests, the session was opened publicly, and the reports of the examiners, at the end of the session, were read out publicly to the council, an account of the proceedings would appear in the papers, and thus the public would be informed of the existence of an English School of Mines far more effectually than in any other way.

If the council would adopt this plan for two or three years, long enough to give it a fair trial (and there is a theatre luxuriant in cushions ready built for the purpose), I cannot but think the number of students would be considerably increased.—I am, etc.,

A Student at the Royal School of Mines.

Jermyn Street, March 3rd, 1868.

To the Editor of the Chemical News.

Sir,—I notice with pleasure a letter in your last week's number calling attention to the present condition of the Royal School of Mines (*Am. Repr., April,* '68, *page*—). It is indeed to be regretted, that this, which should rank foremost amongst our scientific training institutions, and which has abundantly proved its utility, should be compelled to exist under so many disadvantageous circumstances. In a silent and unobtrusive manner it has already done much; but there can be no doubt, that by a more judicious arrangement of details, and by the infusion of more energy and spirit into its management and maintenance, its sphere of usefulness might be considerably enlarged. Undoubtedly, bringing the School before the notice of the public, will have the effect of inducing a greater number of students to avail themselves of the excellent opportunities it affords for obtaining scientific instruction; but this is not all: publicity will render more important service by contributing to give the Associates, who must necessarily share the obscurity of the School, a better status in scientific circles than, by virtue of their title, they have hitherto possessed. The course of study which must be pursued in order to obtain the distinction of "Associate," is sufficiently arduous to make the title, were the School better known, one of considerable merit. Candidates are obliged to have a knowledge, not superficial, but somewhat extensive and practical, of chemistry, physics, geology, and mineralogy. These studies occupy the first two years, after which the student may confine himself to that division in which he desires to take his Associateship; thus, if he wishes to pass in the mining division, the third year's subjects are mining, assaying, and applied mechanics; if in the metallurgical division, metallurgy, (theoretical and practical), and applied mechanics; if in the geological division, natural history and palæontology. It appears that about one-third of the Associates have taken up all three divisions; and I should mention that it is necessary to pass in the first class in the third year's subjects. After having completed this course, which it must be acknowledged represents a fair acquaintance with the principal branches of science, the students are sent out into the world with a title which is both little known and recognised, for it is not very flattering to be superciliously asked, "Where is the Royal School of Mines?" "What is the meaning of an Associate?" and whether it is an honorary title. In Dublin what used to be the Mining School is now called the Dublin College of Science, and perhaps if the School of Mines were called the London College of Science, it would be more appropriate, as comparatively few of the students ever have anything to do with mines. But, apart from that, it is really desirable that the title should at least rank equal to the Associateship of King's College, and that the Associates should be allowed to make use of the initials A.R.S.M., or L.C.Sc. (Licentiate, College of Science), as indicative that the title is by no means an honorary one.

The state of things which renders some such public explanation as this necessary is probably attributable to the manner in which the school has allowed itself to be tacitly ignored; it has no outward sign of existence, because there is no building which bears its name; it has never, in fact, assumed that position amongst educational institutions which, as a State school, it is entitled and privileged to occupy.

It is reported that the School does not pay its own expenses; if this be so, the authorities must have been strangely blinded to their own interests to have omitted such simple remedial measures as those suggested by your correspondent, for I feel convinced that were its existence and benefits more universally known, and if the title it confers carried with it any adequate amount of social standing, there would be an increase in the number of students, and consequently the School would no longer prove unprofitable, either in a pecuniary sense to the Government, or, in an intellectual sense, to the people. Apologising for intruding so much upon your space.—I am, etc.

Delta.

March 3rd, 1868.

MISCELLANEOUS.

Faraday.—Several letters have recently appeared in the daily papers urging the propriety of continuing Faraday's pension to his widow. It has been thought, however, by many, and especially by those best able to judge, that our great philosopher, had he been alive, would have regarded anything of the kind with repugnance. This is borne out by the letter written by Dr. Bence Jones to the *Times* of the 30th ult. Dr. Bence Jones says that he has been requested by Mrs. Faraday to express her thanks for the interest the public are disposed to take in her behalf. The whole course of her husband's life was so marked by his love of retirement that she feels most keenly the intrusion of his name even, while she cannot but be grateful for the kindness which causes her so much pain. She wishes him to assure all those who value Mr. Faraday that the recognition that has already been made of his merits has given her more than she either requires or desired; and she is most anxious that his name should not be used in a way which he never would have approved.

Country Wells.—Dr. Attfield has written a letter to the *Times* on this subject. After alluding to the fact that wells are generally sunk where most liable to contamination, and often receive the contribution of sewers, he says that mineral matter dissolved from the soil is comparatively harmless; animal and vegetable matter may be kept out by every precaution. Good soil is here our best friend, Nature's own purifier, entirely destroying the substances last mentioned, if only allowed to have fair play; but its power for good is limited, its power for harm terrible, when saturated by drainage from adjacent accumulations of filth. Polluted water does not generally betray its condition till possessed of a strong odour; earlier intimation may, however, be obtained by the following tests:—Half fill a common water-bottle, cover its mouth with the hand, violently shake for a minute, and quickly apply the nose. If nothing unpleasant is detected, tightly cork the bottle, set it aside in a warm place at about the temperature of one's body for a couple or three days, and repeat the shaking, etc. Water of very bad quality may thus be recognised without the trouble and expense of analysis.

Famine in Eastern Prussia.—The *Berlin National Zeitung* writes as follows: The government district physician, Dr. Pinkus, of Insterburg, which town is in the centre of the country suffering from famine, appealed to the public to send, among the many gifts of food, above all Liebig Company's extract of meat. He says: Already in several districts typhus appeared; great misery exists, and greater misery must be expected. Even were money always at hand, it would not be possible in many cases in distant villages and cottages to procure fresh meat for the patient, and still less good strong beef tea, the best and most indispensable of all medical comforts in such cases. Medical men in Germany who are in the habit of visiting the poor, find it very useful to carry with them a small jar of extract, so as to dispense beef tea at once where they find it necessary.

Glycerin.—The value of glycerin as a remedy for various skin affections is now generally known and admitted; it was therefore both natural and desirable that it should be presented to us in the solidified and therefore most convenient form of a soap. So numerous are the uses and purposes to which glycerin may be applied, especially in combination with other remedial substances, that glycerin compounds abound. Unfortunately, many of these so-called mixtures of glycerin are so in little more than name; they are either destitute of that substance, contain it only in minute quantities, or, when even present in larger amount, the quality is often by no means good. This observation applies with more or less force to many of the so-called glycerine soaps, perfumes, and cosmetics. In Price's solidified glycerin, however, we possess an article of really definite composition and of superior quality, and one on which we believe that the profession and the public may fully rely. It is stated of this glycerin compound that it wears well, gives a rich lather, and that it contains over half its weight of Price's distilled glycerin, the accuracy of which statement we verified by the following percentage analysis:—

Water....................	21·5
Fatty acids..................	29·5
Soda......................	3·7
Glycerin..................	45·3
	100·0

The Lancet.

Amido-acids from Chlordracylic and Chlorsalylic Acid.—H. Hübner and R. Biedermann. Chlordracylic acid is converted into nitro-compound, and reduced by means of tin and chlorhydric acid to chloramidodracylic acid $\text{Є}_7\text{H}_5\text{Cl}(\text{NH}_2)\Theta(\Theta\text{H})$. The latter is treated with sodium amalgam, which removes the whole of the chlorine, being thus converted into amido-acid isomeric with amidodracylic acid. Similar experiments made with chlorsalylic acid have shown that chloramidosalylic acid is distinguished from chloramidodracylic acid, although they both have the same fusing point, *i. e.*, 212°.—(*Zeitschr. Chem.*, N.F. iii. 567.)

Manufacture of Sugar.—The following modification in the process of refining sugar has been invented (and patented) by L. Pierre and R. Massey. The saccharine juice, after being clarified in the usual way by means of lime and carbonic acid, is precipitated at boiling temperature with caustic baryta (60 parts of the latter for every 100 of sugar), the precipitate suspended in water and decomposed with carbonic acid. A pure solution of sugar is thus obtained which only requires to be evaporated.—(*Zeitschr. Rübens. Ind. Zollv.* 1867, 85, *and Zeitschr. Chem.* N.F. iii. 667.)

Blowpipe Vesicular Reactions. — Captain Ross desires us to state that the colouration of borax and P. salt by certain substances, is so *extremely delicate* when it is blown into a vecicle, that he believes many reactions of metals will soon be made under this head. In the meantime, the two following additions may be confidently made to the blowpipe tables of Plattner and others:—

Silver (oxide of)—**In Borax.**—Opaline by *reflected* and a beautifully delicate pink colour by *transmitted* light. (No other substance can be mistaken for this.)

Tungstic Acid (*in Wolfram*)—**In Borax.**— A peculiar *amber-yellow*, which cannot possibly be mistaken for the yellow given by oxide of iron.

Obituary.—We have this week to record the decease of another of our greatest philosophers—Sir David Brewster,—who died on Monday evening, at Allesley House, near Melrose. To Sir David we owe many of the vast researches made in physical science. Commencing his scientific career at the University of Edinburgh, he very quickly had the honorary degree of M.A. conferred on him, and a few years afterwards was elected a Fellow of the Royal Society of Edinburgh. In 1815 he gained the Copley medal of the Royal Society, for a valuable paper on the "Polarisation of Light by Reflection," and was also elected a Fellow. He afterwards gained the Rumford medal for further discoveries relating to the polarization of light, and the Keith prize from the Royal Society of Edinburgh, for his discovery of two new fluids in minerals, and his analysis of solar light. He was also a member of most of the foreign academies. In 1831 Sir David proposed the scientific meeting at York, which resulted in the establishment of the British Association for the Advancement of Science. During the same year he received the honour of the Hanoverian Guelphic Order, and in 1832 he was knighted by William IV. We here cannot but express our surprise that no greater honour than knighthood can be conferred on such men as Sir David Brewster and Sir Charles Wheatsone, whose discoveries have added so much to the wealth and prosperity of our country. Sir David Brewster retained his love for science to the last, almost weekly contributing papers to the scientific journals.

Improved Spectroscope.—Professor Osborn, of Lafayette College, Easton, Pa., has made improvements in the spectroscope, by which it may be readily applied to a variety of practical purposes, especially in metallurgical operations. In a letter to the *Scientific American*, he says:—"The instrument complete is so arranged that the observer reads the degree on the scale by the actual light which he is analysing. The very light which comprises, in its flame, the vaporized metal, as lime, iron, chromium, titanium, sodium, etc., discloses to the observer in the spectral form its own nature, not only, but often to a great degree, the approximate quantities found in the original ore or even in the coal used or from the wasting brick of the furnace. Nothing can exceed the beauty of the spectral forms which suddenly appear and disappear in the otherwise darkened tube, as the observer stands at the 'tunnel head' of the furnace, watching, as it were, the spectral secrets of that terrible flame which pours forth from the stack, especially when, after the 'cast' and consequent cessation of the blast, that blast is again turned on. The bright yellow bar of sodium is almost always present during examination of all flames resulting from the use of any and all forms of anthracite in the furnace and forge, or from decomposing soda feldspars. But one of the most striking facts in my examinations occurred at our last analysis of a flame from a reheating furnace on the Lehigh, at the wire works of Stuart & Co. The workmen held partly out a bar of intensely heated iron on the hearth of the furnace, when, at rapid intervals, the dark lines which are seen in the solar spectrum appeared faintly, but certainly flitting over the spectrum of the fierce flame by which the intensely heated iron was enveloped. An instrument, of a circular form, is in course of construction, under my direction, for the easy examination of these flames, and which may be used at any time and at considerable distances, and I am hoping that such shall be its sensitiveness that the furnace master may sit in his room and know much of the efficiency and value of the operations proceeding at the furnace by its use. I am situated on a hill, and by means of my instrument, placed upon my dinner table, I can get a beautiful spectrum from a reheating furnace situated not much less than a half mile from my instrument, and am able to detect the sodium in the coal, or from the decomposed fire brick, and also any lime, potash, etc., which proceeds from the furnace mouth. I have no doubt that some exceedingly important uses may be made of this discovery of the spectroscope in the line of metallurgical operations."

Nitroglycerine and Greek Fire.—We have been requested to publish the following memorandum, which has been prepared under authority, and has been issued by Lieutenant-Colonel C. B. Ewart, R.E., by order of the Secretary of State for the Home Department:—"Nitroglycerine is not applied as an incendiary agent, and, if used as an explosive, it will not be scattered loosely about, but will be employed in cans or other closed vessels. If such should be discovered, they should be carefully removed, some heavy body should be attached to them, and they should be thrown into deep water, without any attempt being made to open them. True Greek fire is simply a solid highly combustible composition, very similar to 'Carcass Composition.' What is now commonly called Greek fire consists of a solution of phosphorus or of sulphur and phosphorus in a very volatile liquid, the bisulphide of carbon, to which occasionally some mineral oil is added with the view of increasing its incendiary powers. When this liquid is thrown on to any surface exposed to the air, the solvent evaporates, leaving a film of the phosphorus or sulphide of phosphorus, which will then inflame spontaneously, but will not very readily set fire to wood or combustible materials. The proper mode of extinguishing the flame produced by such an incendiary agent is to throw upon the burning surface a quantity of wet or damp sand, ashes, sawdust, lime, or any other powder, or wet sacking or carpeting, any material, in short, by which the flame can be stifled by exclusion of air. No attempt should be made to remove the covering for some time after the flame has been extinguished. The place should afterwards be thoroughly scoured by playing upon it for some time with a powerful jet of water. Should any scattered liquid be discovered which has not become inflamed, it should be washed away, as above directed, as quickly as possible; and if a jet of water is not immediately at hand, it should in the meantime be covered in from the air by application of any of the materials named above."

Obituary.—Mr. W. Herapath, sen., who was well known as an analytical chemist, died on the 13th Feb., at his residence, the Manor House, Old Park, Bristol. For some time he had suffered from diabetes, but till a few days previous to his decease he persevered in his professional pursuits. He was professor of chemistry and toxicology at the Bristol School of Medicine, of which institution he was one of the founders. The subject of this notice was the father of the well known analytical and toxicological chemist, Dr. W. Bird Herapath, F.R.S.

On some sources of Coal in the Eastern Hemisphere.—By Cuthbert Collingwood, M.B., F.L.S. 1. *Kelung Formosa.*—The coal is found in depressions in red sandstone, and is of comparatively recent origin. It is light, burns very rapidly, gives out great heat, produces 50 per cent. of ash, and forms considerable quantities of clinker. 2. *Labuan, Borneo.*—Several seams of coal crop out conspicuously near the coast, the lowest being 11 feet 4 inches in thickness. It is heavy, close-grained, fast-burning, and gives out considerable heat; it is of very recent date, dammara resin and leaves of recent trees being found associated with it. 3. *Diu Saghalien.*—Coal excellent, burns quickly, with little ash. Presents a fracture similar to Welsh coal. 4. *Japan.*—The author describes coal from several localities in Japan as bright, clean, and resembling Sydney coal, but having a tendency to form clinker. He concludes with a description of some coal from Ivania, Niphon, which is very clean, highly bituminous, burns with a flame in the flame of a candle, and would probably be valuable as a gas producing material.—*Abstract of a paper read before the Geological Society.*

Dr. Jelf.—In consequence of the meditated retirement of Rev. Dr. Jelf from the Principalship of King's College, London, a subscription is being organised by his admirers, including past and present students of all departments of the College, for the purpose of presenting him with a testimonial, which we hope will be worthy of the dignity of this vast institution in its extent and aims. It is computed that between 10,000 and 20,000 living men, mostly engaged in professions, have here received their education. Subscriptions will be received by the Hon. Treasurer, Henry Worms, Esq., Captain of the King's College Rifle Volunteer Corps, 15, St. George's Place, S.W.; or at the London and Westminster Bank.

The Influence of Chemical Knowledge on Sugar Manufacture.—*The Produce Markets Review* says:—Of all countries, England is the most interested in sugar, not only as the greatest consumer, but as owner of some of the richest producing countries in the world, yet no nation displays greater ignorance or apathy with regard to this subject. Like the Lotos eaters, we are content to listen to the distant waves of progress, confident that the protective system of sugar duties will keep the boundaries of our fool's paradise inviolate. But the old proverb, "Where ignorance is bliss 'tis folly to be wise," has certainly no application to commercial matters, for the country that remains in ignorance, whether it be from choice or from indifference, is sure to fall into the rear. In no part of the world is scientific knowledge on mechanical subjects turned to such practical account as in England, and many of our greatest men have made science the handmaid of commerce by applying scientific discoveries to the purposes of every-day life. The telegraph, and more recently the aniline dyes, and Bessemer's iron-working process, are a few instances among many; but sugar, of which the manufacture is completely a chemical process, is entirely overlooked by our *savans*—and yet there is a wide and almost unlimited field for chemical science in perfecting sugar manufacture, which has hardly advanced from its barbarous infancy of crushing mills, windmills, and open pans. The problem of sugar-making, which has yet to be solved, is this:—To extract all the saccharine matter, as it exists in the cells—that is, in a pure condition, and white in colour—without extracting the injurious salts or acids, which co-exist side by side with the sugar, and to do this at as small an expense as possible. A problem scarcely less important is the power of detecting by chemical analysis the exact proportion of extractable saccharine matter in any sample of sugar, for it must be observed that the percentage of extractable saccharine matter is a very different thing from the saccharine strength shown by the polarising saccharometer. We do not hesitate to say, that any chemist who would solve these two problems would render a service to the sugar world of similar importance to that rendered to the world at large by the discovery of the steam engine. While our English chemists are mute upon the subject, the ablest chemists of France and Germany have for the last eighty years been employed in solving the delicate problem of the crystallisation of sugar, and the result of their labours, so far, may be seen in the vast continental beet-sugar crops, which are entirely due to the labours of a generation of chemists which has hardly yet passed away."

College of Chemistry.—All chemists who have studied at the Royal College of Chemistry, as well as present students, will be sorry to hear of the death of Richard Coppins, who has acted as porter in that Institution for more than twenty years. He was particularly obliging and ready in attending to the wants of the students. Apart from his other duties he did some business in chemicals and apparatus, which he was always ready to buy or sell; and most who have passed a session at the College have felt the convenience of having Richard's varied stock to select from in an emergency. He died last week from apoplexy.

Lectures at the School of Gunnery.—Mr. E. O. Brown is now delivering a course of lectures "On Gunpowder, and its substitutes," including gun-cotton and nitro-glycerine. The modes of firing by electricity and special fuzes will also be described. A similar course of six lectures "On the Chemical History and Military Applications of the Metals," was delivered before Christmas, at Shoeburyness, by Mr. J. Spiller.

Separation of Niobic and Titanic Acid.—C. Marignac. The great difficulties attending the estimation of titanic in presence of niobic acid have been overcome by the invention of the following method: 0·5 grm. of the mixed acids are fused with 1·5 grm. of potassic fluorhydride. The mixture after cooling is dissolved in about 250 c.c. chlorhydric acid of 1·015 sp. gr., and reduced by plunging a rod of zinc into the solution, precautions being taken to prevent access of air. Under these conditions niobic acid remains unchanged, and only titanic acid is reduced to sesquioxide. After 24 hours the zinc is removed, and a standard solution of potassic permanganate added.—(*N. Arch. ph. nat. August*, 1867.)

Preparation of Bromides.—A. Faust describes the following method for the preparation of bromides: Bromic sulphide is first prepared by mixing together 2 parts of sulphur (flower) and 24 of bromine; this is added to calcic hydrate, suspended in water, when the following reaction takes place:

$$2\mathrm{S}\,\mathrm{Br}_6 + 8\,\mathrm{Ca}\,\mathrm{O} = 6\,\mathrm{Ca}\,\mathrm{Br}_2 + 2\mathrm{Ca}\,\mathrm{S}\,\mathrm{O}_4$$

The filtrate is saturated with carbonic anhydride, concentrated, and mixed with twice its bulk of alcohol. After a few days the solution containing pure calcic bromide is filtered off from the calcic sulphate, and evaporated. From this salt any other bromide may be obtained by mutual decomposition.—(*Arch. Pharm.*, clxxxi., 216.)

Mock Scotch Soda Crystals.—These are properly sulphate of soda. It is, however, difficult to tell them from Scotch soda (washing soda of the shops) by the eye alone. They are prepared in the following manner:—A quantity of the ordinary "salt cake" is dissolved in a pan to 40 or 45° Twaddell; the liquor, if it shows acid, is neutralised with milk of lime, and 12 lbs of soda ash to every 100 lbs of the sulphate of soda is added in solution. The liquid is allowed to settle and then drawn off into coolers to crystallize. The resulting crystals are large and hard; they are dried in the air for a short time, and then packed in casks. To make the crystals larger and more firm a greater quantity of soda ash is used. One ton of roasted cake will make about 40 cwt. of these crystals. They are frequently sold for the best Scotch soda.—*J. H. Swindells.*

Diffusion.—Some very elegant and simple methods of exhibiting the phenomena of diffusion are given by Herr Merz, in a recent number of the *Journal für Praktische Chemie.* A portion of the shell of an egg having been removed by the action of hydrochloric acid, leaving the membrane exposed, the egg is to be suspended in water from the arm of a balance, a counterpoise being placed in the opposite scale. In about half an hour the weight of the egg has sensibly increased, as the position of the balance-beam will show, in consequence of the passage of water through the membrane. If, now, alcohol be substituted for the water, and the weights readjusted so as to bring the beam horizontal, it will soon commence to move in the opposite direction, showing that the egg has become lighter by the diffusion of water into the alcohol. The diffusion of vapour may be exhibited by tying a diaphragm of india-rubber—a portion of a small toy balloon will answer the purpose—over the mouth of a funnel, the other end being in communication, by means of an elastic tube, with a vessel of water. The funnel being inverted over a dish containing ether, which, however, the diaphragm is not to touch, the vapour of this fluid will pass rapidly into the funnel, the air being observed to escape in bubbles in the water at the small end. Remove now the vessel of ether, and the operation will be reversed, the vapour passing through the diaphragm into the atmosphere. In order to fill the vacuum thus created the water will rise in the tube, the lower part of which should be of glass to render this apparent, and the diaphragm will be curved inwards. These experiments are particularly instructive, and are within reach of every one. The balance may be extemporized by means of a light bar of wood.

CONTEMPORARY SCIENTIFIC PRESS.

[Under this heading it is intended to give the titles of all the chemical papers which are published in the principal scientific periodicals of the Continent. Articles which are merely reprints or abstracts of papers already noticed will be omitted. Abstracts of the more important papers here announced will appear in future numbers of the Chemical News.]

Monatsbericht der Königlich Preussischen Akademie der Wissenschaften zu Berlin. July, 1867.

G. Rose: "*On the Preparation of Crystallised Bodies before the Blowpipe.*" "*On the Behaviour of Titanic Acid towards Borax, and on the Preparation of Rutile and Amorphous Titanic Acid.*" "*On the Behaviour of Oxides of Iron towards Borax, and on the Preparation of Crystallised Hæmatite and Magnetic Iron Ore.*"

[English Edition, Vol. XVII., No. 426, pages 60, 61; No. 430, page 108; No. 431, pages 121, 117; No. 413, page 227; No. 421, page 323; No. 427, page 74.]

"*On the Behaviour of Titaniferous Iron Ore towards Borax, and on the Preparation of Crystallised Titaniferous Iron Ore, and Titaniferous Magnetic Oxide of Iron.*" "*On the Preparation of Rutile by Fusing Titanic Acid and Titaniferous Iron Ore with Phosphate of Soda and Ammonia.*"—A. Oppenheim: "*New Researches on the Isomerism of Chloride of Allyl and Chlorinated Propylene.*"

Poggendorff's Annalen. October, 1867.

A. Wangerin: "*On the Theory of Newton's Rings.*" W. Weyl: "*On Tetramercurammonium and its Compounds.*"

No. 9. October.

R. Ruhlmann: "*Researches on the Effect of Temperature on the Velocity of Light in Water.*"—J. C. Poggendorff: "*On the Development of Heat in the Path of the Electric Spark.*"

Annales des Sciences Naturelles (Zoologie). Nos. 5-6. May-June, 1867.

Rabuteau: "*Experiments on the Physiological Action of Fluorides and of Metallic Compounds in general.* (*Review.*)

Annales de Chimie et de Physique. September, 1867.

Berthelot: "*On the Formation of Pyrogenous Bodies.*" "*On the Reciprocal Action of Hydrocarbons.*" "*Synthesis of Styrolene, Naphthalene, and Anthracene.*" "*On the Polymers of Acetylene, and on the Synthesis of Benzine,*" "*On the Theory of Polymers, and on the Aromatic Series.*" "*On the Formation of Pyrogenous Bodies—Continuation.*" "*On the Synthesis of Toluene, and on the different Principles contained in Coal Tar.*" *On the same subject.* "*On some Thermo-chemical Conditions which determine the mutual Action of Hydrocarbons.*"—V. de Luynes and G. Esperandieu: "*On the Preparation and Properties of Pyrogallic Acid.*"—Berthelot: "*On the Formation of Pyrogenous Bodies—Continuation.*" "*On the Action of Heat on the Homologues of Benzine.*"

October.

Berthelot: "*On the Formation of Pyrogenous Bodies—Continuation: On the Action of Heat on Retene.*" "*On the simultaneous Formation of Homologous Bodies in Pyrogenous Reactions*" "*On the Oxidising Properties of the Homologues of Benzine.*" "*On the Action of Potassium on Hydrocarbons.*" "*On the Isomeric Conditions of Styrolene.*" "*On the Characteristics of Benzine and Styrolene as compared with those of other Hydrocarbons.*" "*On the Combinations of Picric Acid with Hydrocarbons, and on their Use in Analysis.*" "*On the Melting Point of Waxy and Resinous Bodies.*" "*On the different Carbides of Hydrogen contained in Coal Tar.*"—A. de la Rive: "*On a Photometer for Measuring the Brightness of Distant Objects, and on the increased Transparency of the Atmosphere due to the Presence of Moisture.*"—Vaillant: "*On the Transparency of the Atmosphere and its Signification.*"

Dingler's Polytechnisches Journal. October, 1867.

T. Gerlach: "*On the Specific Gravity of Aqueous Solutions of Crystallised Acetate of Lead.*"—H. Swarz: "*A Method of Separating Magnesia from Lime.*" "*On the Manufacture of Carbonic Acid and Magnesia from Magnesite.*" "*On the Detection of Metallic Copper in Aventurine Glass.*" "*On a Process for Smelting Tellurium Ores.*" "*On some Points in the Preparation of Chromic Acid by Acting on Bichromate of Potash by Sulphuric Acid.*"—M. Kosler: "*On the Manufacture of Salts of Tin.*"—L. Walkhoff: "*On Dubrunfaut's Process for Freeing Molasses from Salts by Dialysis.*"

October.

G. T. Gerlach: "*On Crystallised Chloride of Tin.*"—H. Vohl: "*On Naphthalin and its Applications.*"—A. Ott: "*A Method of Testing Commercial Phenic Acid.*"—W. A. Herb: "*On the Estimation of Vinegar, with special reference to an improved Apparatus for using Fleck's Volumetric Method.*"

Journal des Fabricants de Papier. October 1, 1867.

E. Bourdilliat: "*On Testing the Materials and Chemical Products used in Paper Making.*" (*Continuation.*) "*Detection of the various Fibres used in the Manufacture of Paper.*" "*A Method of ascertaining whether a paper has been Sized with Gelatine or Resin.*"

Journal für Praktische Chemie. October, 1867.

Carstanjen: "*On Thallium and its Compounds.*"—J. Lowe: "*On the Transformation of Gallic Acid into Tannic Acid.*"—H. Baumhauer: "*On the Production of Light during the Oxidation of Potassium and Sodium when exposed to the Air.*"—K. Frisch: "*On the Composition of the White External Coating, and of the Black Inner Mass of a Flint from the Island of Rügen.*"

Comptes Rendus. November 4, 1867.

Sir D. Brewster: "*Letter to Chevreul on the Nature of the Relations which existed between Newton and Pascal.*"—Chasles: "*On the same Subject.*"—Becquerel: "*Third Memoir on some newly discovered Electro-Chemical Effects of Capillary Action.*"—E. Peligot: "*On the Distribution of Potash and Soda in Plants.*"—E. Bouchotte: "*On the Dialysis of Induction Currents.*"—C. Blondeau: "*On the Action of Induction Currents on Plants.*"

November 11.

Sir D. Brewster: "*Letter to Le Verrier on the Nature of the Relations which existed between Newton and Jacques Cassini.*" "*Letter to Chevreul on the Authenticity of the Newton and Pascal Correspondence.*"—Balard: "*On the same Subject.*"—Chasles: "*Answer to Sir David Brewster's two Letters on the Nature of the Relations which existed between Newton and Pascal.*"—Grant: "*Letter to Le Verrier on the Astronomical Observations of which Newton and Pascal may have made use.*"—F. Laroque: "*On the Penetration of Air Bubbles into a Liquid on the Passage of a Projectile into the same.*"—Velter: "*On the Value of Sea-Salt as Manure by reason of its Transformation into Carbonate of Soda, and finally into Nitrate of Soda.*"—F. Bellamy: "*On the Use of Subsulphate of Alumina for detecting and estimating certain Organic Matters in Water.*"—H. Schiff: "*On Condensed Ureas.*"—A. Bobierre: "*On the Manufacture of Chloride of Lime, and on Chlorimetry.*"—C. Mene: "*An Analysis of some Samples of Coal from Prussia.*"

November 18, 1867.

Sir D. Brewster: "*Letter to Chevreul on the Authenticity of the Newton and Pascal Correspondence.*"—Chasles: "*Answer to R. Grant's further Communication on the Newton and Pascal Correspondence.*"—A. Gautier: "*On some new Nitriles of the Fatty Series.*"—De Romilly: "*On the Preparation of Cyanides.*"

Poggendorff's Annalen. October, 1867.

R. Ruhlmann: "*Researches on the Effect of Temperature on the Velocity of Light in Water.*"—G. Quincke: "*Optical Researches.*" 9. "*On Jamin's Compensator, and on a new Method of Determining the Refractive Index of Glass Plates for different Lines of the Spectrum.*" C. Freese: "*On the Combinations of Iron with Phosphorus.*"—L. Sohncke: "*On the influence of the Motion of a Source of Light on the Refraction of the Light emitted, being some Critical Remarks on Professor Klinkerfues' Recent Discovery.*"

Bulletin de la Société Chimique de Paris. October, 1867.

Berthelot: "*Answer to Fritzsche's Remarks published in the Bulletin de la Société Chimique for September, 1867, on the Author's Paper on Anthracene.*"—Berthelot: "*On the Hydrocarbons of Coal Anthracene. Fluorene. Acenaphthene or Tar; Styrolene; Cymene. Hydrides of Naphthalene. Acetylonaphthalene.*"—Wiederhold: "*A new Green, derived from Linseed Oil and Oxide of Copper.*"—Bernard, Scheurer, and Tempe: "*A new Process for the Extraction of Indigo from Dyed Woollen and Cotton Rags.*"—Bernard: "*A new Process for Dyeing Stuffs Turkey Red.*"

Journal für Praktische Chemie. November, 1867.

Carstanjen: "*On Thallium and its Compounds.*"—C. F. Schonbein: "*On the Transfer of the Oxygen absorbed from the Atmosphere by Turpentine and similar Organic Substances to Water.*" "*On the Presence of Active Oxygen in Organic Substances:* 1, *On the Quantity of Ozone contained in Blue Guaiacum Wood.* 2, *On the Free-Active Oxygen of Quinone.* 3, *On the Combination of Cyanine with Ozone.* 4, *On the Combination of Olefiant Gas with Ozone.*" "*Researches on Guaiacum Resin.*" "*On Brasilin and on its Fluorescent Properties.*"—J. Wolff: "*On Two new Derivatives of Aniline.*"—W. Stein: "*Contributions to the Knowledge of Orellin, a Yellow Colouring Matter derived from Bixa orellana.*"—Otto: "*On the Characters of Thallium, and on the Metallic Group to which it belongs.*"—G. Vorbringer: "*A Method of producing a Black Pharaoh Serpent.*"

Dingler's Polytechnisches Journal. November, 1867.

O. Zabel: "*An Electro-Magnetic Apparatus for regulating the Temperature at which Substances are dried in Chemical Operations.*"—G. Lunge: "*On the Analysis of the Materials used in and of the Bye-Products of the Manufacture of Soda.*"—E. Brescius: "*On the Simulation of Arsenic by Chloride of Zinc in Marsh's Test, and on the Detection of Arsenic and Antimony in Hydrochloric Acid.*"

Bulletin de la Société Industrielle de Mulhouse. October, 1867.

F. Kopp: "*On the Preparation of Extracts of Garancine for Calico Printing.*"

November.

Rosenstiehl: "*Report on the Methods used in Dieuze for Utilising Chlorine Residues and Soda Waste.*"

Journal des Fabricants de Papier. October 15, 1867.

E. Bourdilliat: "*On Testing the Materials and Chemical Products used in Paper Making.*" (*Continuation.*) "*Researches on the Mineral Substances contained in Paper.*" "*On the Detection of some Colouring Materials used in Paper Making.*" "*On the Determination of the Tenacity of Paper.*"

November 1.

E. Bourdilliat: "*On Testing the Materials and Chemical Products used in Paper Making.*" (*Continuation.*) "*On some Changes which take place in Paper.*"

L'Invention. October.

P. Alfraise: "*On Raddisson's new Process for the Manufacture of Aniline Colours.*"

November.

O. Saix: "*On the Natural Formation and Artificial Production of Diamonds.*"

Kunst und Gewerbeblatt. August—September.

H. Le Play: "*On the Use of Lime for Extracting Crystallisable Sugar from Saccharine Juices, Syrups, and from the Molasses of Beet and Cane Sugar.*"—C. Dehaga: "*On the Use of an Infusion of Guaiacum Wood for Testing Kirschwasser.*"—C. R. Von Hauer: "*On an Alloy of Bismuth, Lead, Tin, and Cadmium fusible at a very Low Temperature.*"

Bulletin de la Société d'Encouragement. September.

E. Peligot: "*On some Processes now in Use for Engraving on Glass by means of Hydrofluoric Acid.*"—De Luynes: "*On a Method of Obtaining Colouring Matter from Orcine.*"

Comptes Rendus. November 25.

E. Bourgoin: "*On the Electrolysis of Organic Acids and of their Salts.*"—A. Scheurer-Kestner: "*Some Experiments on the Manufacture of Chloride of Lime.*"—A. Gautier: "*On the Isomers of the Nitriles of the Fatty Series.*"

December 2, 1867.

Sir David Brewster: "*Letter to Chevreul on the Authenticity of the Newton and Pascal Correspondence.*"—Chasles: "*Remarks on the preceding Letter, and on one from G. Govi on the same Subject.*"—L. Larroque: "*On a Collection of Geological Specimens from Chili.*"—G. Govi: "*Remarks on the Letters alleged to have been written by Galileo, which have been published with reference to the Newton and Pascal Correspondence.*"—T. Schloesing: "*On the Simultaneous Estimation of Carbon, Hydrogen and Nitrogen in the elementary Analysis of Organic Substances.*"—Musculus: "*On the Hydrates of Stannic Acid.*"—Alvergniat: "*An Apparatus for showing that the Electric Spark cannot pass through a Perfect Vacuum.*"—Langlois: "*On the Formation of Cyanide of Ammonium by passing the Vapour of Ammonia over Incandescent Charcoal.*"

Bulletin de l'Academie Royale de Belgique (Classe des Sciences). October 12.

"*Obituary Notice of Michael Faraday.*"—Haidinger: "*On the Collection of Meteorites at the Museum of Vienna.*"

Annalen der Chemie und Pharmacie. October.

A. Butlerow: "*On the Derivatives of Trimethylcarbinol (Tertiary Pseudobutylic Alcohol.*") "*On the Isomerism of the Saturated Hydrocarbons C_4H_{10} and of the Butylenes C_4H_8.*" "*On Isobutylic Alcohol (Primary Pseudobutylic Alcohol or Pseudopropylcarbinol).*" "*On the Action of Water on the Chlorides of some Alcohol Radicles.*" "*On the Occurrence of Tertiary Pseudobutylic Alcohol amongst the Products of Fermentation.*" "*On the Action of Hydriodic Acid Gas on the Iodides of the Alcohol Radicles.*" "*On the Crystalline Form of Hexamethylenamine.*" "*On the Non-poisonous Properties of Zinc Methyl.*" "*On the Preparation of Chlorhydrin of Glycol by Carius' Process.*"—A. Butlerow and M. Ossokin: "*On Iodhydrin of Glycol and on a new Method of Forming Alcohols by Synthesis.*"—H. Schiff: "*On the Ammoniacal Derivatives of Isatin.*"—Schwarzenbach: "*On the Mutual Relations between the Equivalents of Albuminoid Substances.*"—P. Lessen: "*On some Oxidation Products of Naphthaline.*"—W. Heintz: "*Note on the Preparation of Diglycolate of Lime.*" "*On the Action of Dry Carbonate of Soda on Monochloracetic Ether.*" "*On Diglycolic Ether and Diglycolamide.*"—A. Bettendorff: "*On the Allotropic Conditions of Arsenic.*"

Annales de Chimie et de Physique. November.

J. Kolb: "*Researches on Chloride of Lime, being an Introductory Essay on the Use of that Substance for Bleaching Fabrics.*"—E. G. Lambert: "*Analytical Researches on the Nature of the Potable and Mineral Waters of Orizaba, Queretaro, and Monterey, Mexico.*"

Journal für Praktische Chemie. November.

D. Huizinga: "*On the Detection of Ozone, and on the Presence of this Substance in the Atmosphere.*"—F. Reindel: "*On Basic Sulphates of Copper.*" "*On the Production of Prussic Acid from Ferrocyanide of Potassium and Sulphuric Acid.*"—Buchner: "*Chemical Analysis of the Mineral Waters of Neumarkt, Upper Palatinate.*"—H. Gruner: "*Contributions to the Knowledge of Binitrophenylic Acid.*"—W. Kubel: "*On the Estimation of Nitrous Acid by means of Permanganate of Potash.*"—R. Wagner: "*On the Solubility of some Earthy and Metallic Carbonates in Water impregnated with Carbonic Acid under Pressure.*"—F. Reindel: "*On Soluble Prussian Blue.*"

PATENTS.

Communicated by Mr. Vaughan, F.C.S., Patent Agent, 54, Chancery Lane, W.C.

GRANTS OF PROVISIONAL PROTECTION FOR SIX MONTHS.

3518. A. T. Carr, Winkleigh, Devonshire, "An improved manure."—December 11, 1867.

3544. J. H. Johnson, Lincoln's Inn Fields, "Improvements in the manufacture of artificial fuel."—A communication from A. F. P. A. G. Decamps, T. de Joly, and F. F. Cholial, Paris.—December 13, 1867.

3559. J. Hargreaves, Appleton-within-Widnes, Lancashire, "Improvements in the manufacture of soda and potassa.

3566. A. M. Clark, Chancery Lane, "Improvements in the extraction of ammonia from fermented and other liquids, and in the regeneration of the agents used in such extraction."—A communication from A. Coste, and L. T. de Reanay, Boulevart St. Martin, Paris.—December 14, 1867.

3573. W. Huskisson, Swinton Street, Gray's Inn Road, Middlesex, "Improvements in the manufacture of soda and other aërated waters, and in the manufacture of aërated bread."

3574. J. Dawson, Greenock, Renfrewshire, N. B., "Improvements in treating sugar syrup."

3586. W. Ross, Grove Street, Walworth, and A. Long, Aylesbury Terrace, Walworth, Surrey, "Improvements in means for preventing and removing incrustation in steam boilers."—December 17, 1867.

3614. W. H. Richardson, Glasgow, N. B., "An improved apparatus to be employed in the manufacture of iron and steel."—December 19, 1867.

NOTICES TO PROCEED.

2292. W. R. Dawson, Great Saint Helen's, London, "Improvements in the preparation of titaniferous iron sands for melting."—Petition recorded August 9, 1867.

2305. R. Girdwood, Edinburgh, Mid Lothian, N. B., "Improved composition to be applied to sheep and other animals, for the purposes of destroying vermin, and parasitical life thereon, and for protecting them therefrom."—August 10, 1867.

2308. C. D. Abel, Southampton Buildings, Chancery Lane, "An improved method or process for removing sulphur, phosphorus, and other impurities from iron, steel, and other metals."—A communication from J. F. Bennett, Pittsburgh, Penn., U.S.A.

2314. A. McDougall, Manchester, "Improvements in the extraction and separation of the sulphur contained in products resulting from the alternate exposure of certain metallic oxides to gases containing sulphuretted hydrogen, and to oxygen."—August 12, 1867.

2344. J. T. Way, Russell Road, Kensington, Middlesex, "Improvements in treating phosphate of lime for the manufacture of manure, and for other purposes."—August 14, 1867.

2377. J. Hooker, Oberstein Road, New Wandsworth, Surrey, and F. Brady, Euston Road, Middlesex, "Improvements in fire lighters, also applicable for fuel generally."—August 19, 1867.

2410. J. G. Marshall, Leeds, "Improvements in solvent or detergent processes."—August 22, 1867.

2436. E. Sonstadt, Manghold, Isle of Man, "Improvements in the treatment of sea-weed for obtaining valuable products from it."—August 26, 1867.

2522. F. Versmann, Ph.D., High Street, Stratford, Essex, "Improvements in the manufacture of varnishes."—September 5, 1867.

2541. J. Whitham, Kirkstall Road, Leeds, "Improvements in machinery for puddling, and in puddling and other furnaces."—September 7, 1867.

2546. W. E. Gedge, Wellington Street, Strand, Middlesex, "Processes of extraction of the colouring matter of indigo from the waste textile fabrics which contain it."—A communication from C. Bernard, P. Scheurer, and J. B. Tempé, Colmar (Haut Rhin), France.

2549. F. Tolhausen, Boulevart Magenta, Paris, "An improved process and apparatus for instantaneously disinfecting fecal and manuring matters, improving the same, and also rendering them fit for feeding domestic animals."—A communication from L. J. B. A. Lemoine, and A. M. Turrell, Paris.—September 9, 1867.

3248. J. Swindells, Kegworth, Leicestershire, "Improvements in the process of, and in apparatus employed in treating and separating minerals, earths, and other substances, when ground or pulverised."—November 16, 1867.

3384. J. Baylis, Durdham Down, Bristol, "An improved chemical preparation or compound to be used in preparing mixed textile fabrics for dyeing or colouring."—November 28, 1867.

3389. C. Albiser, Mincing Lane, London, "Improvements in the preparation of sulphate of magnesia applicable to the treatment of the crude potash salts of Stassfurt, and the refuse from the manufacture of chloride of potassium."—A communication from J. Vorster, and H. Gruneberg, Cologne, Prussia."—November 29, 1867.

3440. J. Gjers, Middlesbrough, Yorkshire, "Certain improvements in the manufacture of cast steel and homogeneous iron."—December 3, 1867.

3469. P. G. L. G. Designolle, Rue de la Steine, and I. Castbelay, Rue Ste. Croix de la Bretonnerie, France, "Improvements in the manufacture of explosive and fulminating powders."—December 5, 1867.

3473. J. Durrans, Thurlstone, near Peniston, Yorkshire, "An improved material or composition to be employed for covering or coating the interior surfaces of moulds, crucibles, or ducts, previous to their receiving the molten metal in the process of casting, and for other purposes."—December 6, 1867.

[English Edition, Vol. XVII., No. 429, page 98; No. 430, pages 109, 110.]

NOTES AND QUERIES.

It has been represented to us that our column of Notes and Queries has occasionally been made the vehicle for the surreptitious disposal of trade secrets by subordinates in chemical works, unknown to their principals. This column has proved to be sufficiently useful to a large class of our readers for us to be reluctant to discontinue it for the sake of a few who abuse its privileges. Probably a more rigid supervision will enable us to obviate the difficulty. There will be no objection to a correspondent asking for information on trade subjects; but the answer must likewise be made public, and in such cases no name or address can be given, no private communications forwarded through us, and no offer of payment for information can be published.

Waterproof Paste.—I understand that calico printers use a paste called "resist paste," which is waterproof, of the following proportions: 1 lb. of binacetate of copper or distilled verdegris; 3 lbs. of sulphate of copper, dissolved in one gallon of water; this solution to be thickened with 2 lbs. gum senegal; 1 lb. British gum; 4 lbs. pipe clay; 2 oz. nitrate of copper, as a deliquescent. Can any of your readers kindly inform me if I can use it to stick lithographic bills on cards without detriment to the colours or the paper on which they are printed?—C. E.

Sprengel Air-Pump.—Could any one give me about the dimensions for a Sprengel air-pump, and the quantity of mercury required. I wish to use it for exhausting vacuum tubes for a 4-inch spark induction coil.—Ein Englander.

Sulphur in Pyrites.—Can any of your readers kindly inform me of the method usually adopted for ascertaining the amount of sulphur in iron or copper pyrites, to determine its value for sulphuric acid making? Is there not a practical method of assaying it?—T. W. W.

Estimation of Tannin in Gall-nuts, Sumac, &c.—Can any of your correspondents favor me with a good method for estimating the amount of tannin in either of the above, and also the percentage amount of tannin in the different qualities of sumac in the market?—Astringent.

Sulphite and Hyposulphite of Soda.—Can any of your correspondents enable me to discover the consumption of sulphite and hyposulphite of soda in England, and also its uses.—G. W. R., Liverpool.

Chlorimetry.—Is it possible to devise a ready way of estimating the chlorine in a solution of bleaching powder containing a large proportion of nitrate of lime?—S.

Price of Sulphuric Acid.—Perhaps some reader of the Chemical News can inform me what price I ought to pay for sulphuric acid of 1·6 specific gravity, supposing I pay £4 per ton specific gravity, 1·72.—V. C.

Swedish Cooking Apparatus.—I see you ask the address of the inventor of the Swedish. or rather Norwegian, Cooking Apparatus. It is Mr. Sörensen, 13 Duke Street, Grosvenor Square, W. I can vouch, from my own use, for the practical efficiency of this simple converse of a refrigerator.—Marshall Hall, 3 Cleveland Terrace, Hyde Park, W.

Portable Cooking Apparatus.—In the Chemical News, "To Correspondents," I note "Swedish portable cooking apparatus," and it occurs to me that a knowledge of a lamp I fortunately came across in Scotland, and took with me to Iceland, might prove interesting to travellers, though I may be describing what you have seen. The lamp is of copper, and is partly filled through the cock with methylated spirit, the tap closed, and a little spirit poured into open centre and lighted—the flame soon warms the sides and top of the lamp, and the consequent expansion of the vapour inside presses the spirit in a strong et from a bent tube connected with bottom of spirit chamber, which lights, and increases the heat of the lamp, thus getting a stronger flame. Mine would boil a pint of water in five minutes, and use from one to one and a half ounce spirit.—John Cliff.

The Sprengel Air-pump.—"Ein Englander" will find a paper entitled "Sprengel's Researches on the Vacuum," wherein his mercurial air-pump is described, in the January number, 1865, of the *Journal of the Chemical Society*. With an instrument of total height of bout 6 feet, and from 10 to 15 lbs. of mercury, a receiver of half a litre apacity may be exhausted in from 20 to 30 minutes: the fall tube must not be of greater calibre than 8 millimètres, and had better be of 2½—2¾ millimètres only.—C. R. A. Wright, B. Sc.

Sulphur in Pyrites.—In answer to T. W. W. I beg to point out that f it is *not* required to obtain the absolute quantity of sulphur met with n pyrites, a pretty near estimate of the value of the mineral for use in he manufacture of sulphuric acid may be obtained by taking a fair verage sample of the pyrites ground to an impalpable powder, weigh f 25 to 30 grains, and expose these on a flat platinum dish, best in a horoughly heated muffle to pretty strong incandescence for two or hree hours; if no muffle is at hand a good gas flame will answer, and fter cooling the loss in weight may be taken; but this method is *not* o be used but as an approximative estimation; the analysis of pyrites r sulphur requires many precautions to yield good and reliable reults; of course the sulphur is oxidised to sulphuric acid, and the latter timated, and from this result the amount of sulphur is calculated.—R. A. A.

Oxychloride of Magnesia.—If J. E. Hamilton will apply to J. B. iles, at this address, he may hear of a crude carbonate of magnesia hich would doubtless answer his purpose.—Borax Works, Old Swan, iverpool.

Solvent for Essential Oils—Can any of your able correspondents inform me of a solvent for the essential oils or the resinous essences such as cinnamon or bergamote, so as to enable me to mix them with water without creating a muddiness? Information upon the above will oblige a Subscriber.

Clarke's Process for Softening Water.—What quantity of lime. or lime water (assuming that 1 lb. of lime would dissolve in 90 gallons of water) would be required to precipitate the calcareous matter in spring water (by Clarke's process), the analysis of spring water being as follows: Per gallon in grains—carbonate of lime, 12·8, sulphate of lime, 1·5, chloride of sodium, 3·8, organic matter (vegetable), 2, hardness 9°, ditto after boiling, 4°?—Aquarius.

Determination of Free Sulphuric Acid in Superphosphates.—The following is a simple and yet efficient plan: Estimate the sulphuric acid in the original sample by dissolving a weighed portion of the superphosphate in hydrochloric acid, not contaminated of course with any sulphuric acid, separate the insoluble matter by filtration, and after thorough washing with boiling distilled water, precipitate with chloride of barium, and let stand for some hours, separate the sulphate of baryta, and calculate the sulphuric acid from its weight. Now take another weighed portion of the superphosphate, ignite it gently, and proceed as just described; the difference of the two results as regards sulphuric acid obtained represents the free sulphuric acid in the sample.—Dr. A. A.

To Prevent Water Freezing.—Your correspondent "Volta" desires to know (1.) what proportion of salt has to be added to water to prevent it freezing when exposed to the coldest weather known in this country. I would advise "Volta" not to use salt, especially not when in contact with iron, but rather apply caustic soda, which dissolved in water does not affect iron, and at the same time, if the solution is made strong enough, will not freeze, while, as is well known, a solution of salt freezes, partly leaving a more concentrated saline mixture unfrozen. 20 parts by weight of caustic soda upon 100 parts of water are sufficient to keep water fluid even at as low a temperature as 10° F. As regards the use of alcohol, I think methylated spirits might do, a mixture of water and spirits of the specific gravity of 0·9760, *i.e.*, containing 20 per cent. by volume of absolute alcohol will not congeal even at 8° F. A strong solution of commercial glycerine is very much used in the colder climes of Europe, *e.g.*, Russia, Poland, Sweden, to substitute water in wet gasmeters, as such a solution of glycerine does not freeze even in the cold weather, which is usually very severe in these countries in winter. During the winter from 1847 to 1848 at Stockholm, the temperature for several days was below the freezing point of mercury, that is—40° F. During these days the late celebrated Berzelius made some experiments with frozen, *i.e.*, solid mercury, which could then be obtained readily in large quantity; this, in passing, and without reference to the non-freezing of the mixtures above alluded to at so low a temperature.—Dr. A. A.

Solvent for Essential Oils.—The only available solvent for your correspondent's purpose is perhaps water itself, in which the oils may be dissolved, or at least mixed in a way which will be equivalent to solution, by the process used by many druggists for making their "aquæ." For every ½ oz. of oil put a good handful of $MgCO_3$ into a mortar and triturate it well with the oil until the latter is thoroughly divided, then add water paulatim, triturating diligently all the time, and filter. The division of the oil is facilitated by previously diluting it with its own bulk of strong spirit of wine, in which case add the water only a few drops at a time at first, but this is not indispensable.—Volta.

Estimation of Tannin.—The most reliable and undoubtedly best mode to estimate tannin is the process devised by Dr. H. Fleck, but a full and complete account of this mode of estimation cannot be given in N. and Q., since the space cannot be spared. If "Astringent" would give his address I shall be happy to give him a full account of this mode, and also of Müller's mode for estimating tannin. As regards the percentage of tannin in tanning and dyeing materials, the following information may perhaps serve the purpose: oak bark varies from 10·86 per cent. to 15·83 per cent., *Polygonum bistorta* varies from 17 to 21 per cent.; sumac, from 11 to 18 per cent.; dividivi, from 25 to 18 per cent.; nut-galls (Aleppo galls), from 58 to 66 per cent. valonia, from 40 to 45 per cent.: catechu, Gambia, from 40 to 50 per cent. of a peculiar tannic acid; kino contains from 30 to 40 per cent. of the same principle as catechu.—Dr. A. A.

Hofmann's Modern Chemistry.—Lecture Experiments.—Your "Notes and Queries" often have been most valuable to many who seek for information, and cannot find it in books which they consult; for it is a well known saying, that you may have many books and yet never find what you require. The reason I think is, that authors do not trouble themselves about minor points of a subject which perhaps they think only trifles, forgetting all the while that trifles make perfection, and perfection is no trifle. As an illustration of this point I have been reading and closely studying that admirable work "Hofmann's Modern Chemistry," and to make myself master of its contents, I performed nearly all the experiments, having apparatus made especially for the occasion; it certainly is one thing to read a book of experiments, and another to work them out, as I found when I commenced operation. The first difficulty I encountered was the examination of HCl gas by sodium amalgam. The question I should like answered is, what percentage of sodium ought you to use to form the amalgam for this purpose? I have consulted several works, but cannot obtain the desired information. When you have finished the experiment, and emptied the U-tube of sodium amalgam, what is the best method of recovering the pure mercury? I poured warm NH_4Cl upon the very weak amalgam, and let it stand for some days. Is that the best method? My second stumbling-block was in decomposing HCl by the electric current. What ought to be the sp. gr. of the acid? The book says, 1·1. That is very indefinite, because there are about a dozen beginning with 1·1. Does it mean the HCl of commerce, sp. gr. 1·16? In using this said acid I was not

[English Edition, Vol. XVII., No 427, page 73; No. 428, page 85; No. 430, page 109; No. 429, page 98; No. 430, page 109.]

successful in performing the experiment, because the liquid became so hot by the action that HCl gas was evolved along with the gases H and Cl. I certainly must admit that these are trifling points, but then they are so necessary for the success of an experiment, and in performing new experiments one desires to know all the little difficulties which may beset them. If some of your correspondents will help me, I shall feel obliged.—Amateur.

Hofmann's Modern Chemistry—Lecture Experiments.—Having repeatedly performed all the experiments described in this book, I may be able to remove some of the difficulties which your correspondent "Amateur" has encountered. First, with regard to the percentage of sodium in the sodium amalgam to be used for the decomposition of hydrochloric acid gas. This is quite immaterial, provided that sufficient sodium is contained in the amalgam to decompose all the hydrochloric acid in the U tube, and not so much as to render the amalgam solid. The U-tube is not likely to contain more than 100 c. c. of gas, which would weigh 0·16335 grm., and would require about 0·105 grm. of sodium to decompose them, so that if your correspondent dissolves about one gramme of sodium in the quantity of mercury necessary to fill up the U-tube, he will probably have a sufficient excess of sodium to ensure the decomposition of the gas: but as the experiment, as far as I am aware, has never been performed with an amalgam of a known composition, it would be advisable to try it in this manner before positively affirming that the quantities above given would produce the desired result. Next, as to recovering the mercury from the amalgam. If "Amateur" had reflected, he would probably have come to the conclusion that, as he was using sodium amalgam to decompose hydrochloric acid, so hydrochloric acid would be the best reagent for decomposing sodium amalgam; and he will find that if he places the amalgam into dilute hydrochloric acid, the sodium will be removed in a few minutes, and quite as efficiently as by the circuitous process of digesting with a solution of ammonic chloride. "Amateur" complains that the direction to use hydrochloric acid, of the specific gravity 1·1, is indefinite; perhaps 1·10000 will convey a more distinct impression to his mind—for this is what is meant. The object of employing acid of this strength is to avoid the inconvenience of which your correspondent speaks; the liquid not fuming in the air, and not evolving hydrochloric acid when heated.—H. M.

Estimation of Free Sulphuric Acid in Superphosphates.—Permit me to express my astonishment at the Chemistry of "Dr. A. A." as displayed in his replies to "V. C." on the Estimation of Free Sulphuric Acid in Superphosphates; and to "F. W. W.," on the Determination of Sulphur in Pyrites. "Dr. A. A." informs "V. C." that by first ascertaining the entire amount of sulphuric acid in a hydrochloric acid solution of the superphosphates, and then the quantity of sulphuric acid left, after igniting another portion of the sample, the difference will be free acid. This looks remarkably simple, and straightforward on the face of it. But "Dr. A. A." ignores the fact that most superphospates contain sulphate of ammonia, and no inconsiderable proportion of organic matter. It is not difficult, then, to conceive how these would militate against the accuracy of the results by "Dr. A. A's." process. To say nothing of the entire volatilization of the sulphate of ammonia, or how the sulphate of lime would stand affected by ignition in the presence of carbonaceous matters, a considerable quantity of the free acid would in all probability, under this exciting condition, combine with the lime of some of the undecomposed phosphates. Should "V. C." require a more ready, though not nearly so accurate process as the admirable one volunteered by Dr. Moffat, Glasgow, I may say that an aqueous solution of the manure, treated with a very dilute normal solution of ammonia, gives tolerably fair results. Again, as regards "Dr. A. A's" expeditious mode for determining the sulphur in pyrites—that is a more extraordinary outrage on chemical science than the former one, on which I have just commenced. However he gets even "an approximate estimation" by submitting a sample of pyrites (FeS_2) to "strong incandescence for two or three hours in an open capsule;" producing, of course, an indefinite mixture of sesquioxide and protoxide of iron ($Fe_2O_3 + FeO$), and afterwards reckoning the loss of weight as sulphur, is best known to himself; but to me is an inexplicable enigma. I have already, I apprehend, trespassed too much on your space, and will only therefore refer "F. W. W." to almost any handbook on analysis for a speedy way of arriving at the value of pyrites.—S. A. S.

ANSWERS TO CORRESPONDENTS.

NOTICE.—The American Publishers of The Chemical News *give notice that in accordance with a suggestion of* Mr. Crookes, *the Editor and Proprietor of the English publication, they will be pleased to receive and forward to him in London any scientific publications issued in America, for review—and also any Notes and Queries, Articles, Correspondence, etc., for publication or reply. Their facilities of communication with* Mr. Crookes *render this very desirable to all persons in the United States who wish to confer with him. Address,*

W. A. TOWNSEND & ADAMS,
434 *Broome Street, New York.*

Sutton.—Not yet published.

J. Horsley.—Received with thanks.

G. J. De Winton.—See below.

Astringent's letter shall be attended to.

John C. Tellers.—Will our correspondent forward a few specimens with particulars as to cost of production, etc.? We shall then be enabled to form an opinion.

J. T.—The odour is one of the properties of naphtha inseparable from it.

Swedish Portable Cooking Apparatus.—Will the maker of this apparatus please communicate address to our office?

Chemicus.—We have been quite unable to find out the name of the firm. The announcement was made on the authority of the late Dr. Richardson.

R. B. C.—A communication is waiting for you at our office. Please forward your address.

Ein Englander.—A solution of isinglass in weak spirit is the best cement with which to fasten on the sides of a hollow prism for bisulphide of carbon. Chloroform will be the best solvent for indiarubber to be used for coating a bladder. Zinc may be quickly amalgamated by dipping it into mercury containing a little sodium amalgam dissolved in it. Apply to Asher's foreign bookseller.

H. P. M.—The piece has a similar composition to a Roman mirror, which according to an analysis by Professor Church, contained 70·29 per cent. of copper, and 29.91 per cent. of tin. The similarity, however, is not likely to be more than a coincidence.

W. Kirk.—In a short time.

J. Leasson, Glasgow.—We expect to receive the report in a few weeks.

W. B.—We will refer to the paper, and communicate particulars if they appear likely to be useful.

D. S.—Some Technological Dictionary would probably give you the required information. See "Ure's," "Brande's," "Cooley's," or "Watts's" Dictionaries.

Jurors' Report of the Paris Exhibition, 1867.—We have had many applications for this report. On inquiry we find that no report of the Jurors is likely to be published in this country, and it is not known if such will be published in Paris

Marshall Hall.—Dr. Roscoe has made the discovery of an ore of vanadium in the Alderly Edge copper ore, in Cheshire. This discovery formed the basis of the Bakerian Lecture, delivered before the Royal Society, by Dr. Roscoe. A report will shortly appear in our columns.

F. Blair.—The extract from the *Panama Star and Herald* is received with thanks. Professor Delisse's theory of the cause of earthquakes, however improbable it seems, appears to have been borne out by the accurate fulfilment of the predictions founded upon it. It requires careful attention before we could say whether this appeared a coincidence or not.

Communications have been received from O. W. Parker (with enclosure); Rev. B. W. Gibsone, M.A. (with enclosure); J. Cliff; E. Dring (with enclosure); Professor Tyndall, F R.S.; W. Scott; E. Hopwood (with enclosure); The Abbé F. Moigno; F. Montgomery; H. Octavius; J. Horsley (with enclosure); W. Valentin; G. J. De Winton; Mr. Sutton; R. Googan; W. Ackland (with enclosure); W. Isbister (with enclosure); J. Emerson; H. Hodgkinson; W. Richardson; J. Murray (with enclosure); T. Cobley; Alfred Payne; W. Miller and Sons (with enclosure); W. G. Drysdale (with enclosure); Rev. E. Smith, M.A.; G. A. Keyworth; Dr. Aloin Pflughaupt; R. McAlley; Rev. M. Kiernan; J. Wilkinson and Co.; W. Smith; F. Sutton; Dr. R. Angus Smith, F.R.S.; Mottershead and Co.; W. Poole Balfern; Rev. R. Kirwan; D. Shaw and Sons; Nicholson, Maule and Co.; G. W. Eccles; T. W. Tobin (with enclosure); J. E. Thorpe (with enclosure); Rear Admiral Sir F. Nicolson; O. Grimet; Dr. Adriani (with enclosure); Johan Sorensen; J. Cliff; O. Hopkinson; J. Rugeley; F. L. Bigg (with enclosure); A Chorlton; E. Wittingham; Professor Wanklyn; E. F. Jones (with enclosure); R. Oxland; Dugald Campbell; H. Bird; W. Ellis; T. E. Thorpe; H. Stephenson (with enclosure); J. Dempsey; W. Schofield; F. Sarl (with enclosure); W. Harding; W. Valentin (with enclosure); M. Phillips; E. Smith; E. B. Marten (with enclosure); W. Hofman; O. Kerl; H. J. Helm; Marshall Hall; H. R. Williams & Co.; W. Bird Herapath, M.D., F.R S.; J. Taylor; Lord Sackville Cecil; A. H. Allen; J. Marples; J. C. Lee; F. Shaw; D. Forbes, F.R.S. (with enclosure); Mottershead & Co.; Marshall Hall; G. Marrison, Tasmania; F. Blair; W. Thompson (with enclosure); G. Hain; W. Kirk; F. Foord, Victoria, Australia; W. Kellner; Captain W. A. Ross (with enclosure); A. L. E.; T. G. Barlow; Dr. T. L. Phipson (with enclosure); C. R. C. Tichborne; T. Hill; W. Venables (with enclosure); J. Samuelson; Dr. Adriani; J. Stephens; Dr. W. Bird Herapath, F.R.S. (with enclosures); S. Dowell; J. Taylor; M. L'Abbé Moigno; Philip Holland (with enclosure); Phillips and Co.; J. H. Atherton; T. M. Drown; F. Muspratt; Professor Pavesi; Negretti and Zambra; E. Leeds (with enclosure); M. Leoni and Co.; F. Suckling; W. Smith; J. B. Giles (with enclosure); R. Eaton; Marshall Hall.

Books received.—"El Correo Hispano-Americano;" "On the Magnetic Attraction of Cosmical Bodies," by John A. R. Newlands, F.C.S.; "Scientific American;" "American Artisan;" "Pharmaceutical Journal;" "Hardwicke's Science Gossip;" "Journal of Gas Lighting;" "Chemical Notes for the Lecture Room. On Heat, Laws of Chemical Combination, and Chemistry of the Non-Metallic Elements," by Thomas Wood, Ph.D., F.C.S., 2d edition. London: Longmans & Co.; "The Transference of the Telegraphs to the State," by John Stephen. London: Longmans & Co.; "Brief Extracts of Reports and Boiler Explosions in 1867," by Edward B. Marten; "Minutes of Proceedings at the Extraordinary Meeting of Shareholders of the Atlantic Telegraph Company;" "Reform Scientifique;" "Reports of the United States Patent Office, 4 vols. for 1863 and 1864," from the Hon. the Commissioners of Patents. Stevens Bros., Agents for Europe; "The Harrowgate Herald;" "Bristol Times and Mirror;" "Western Daily Press;" "Rollison and Sons' Catalogue of Floricultural and Culinary Seeds;" "Journal of Gas Lighting;" "Darlington and Stockton Times;" "El Correo Hispano-Americano;" "Scientific American;" "American Gas Light Journal;" "Zeitschrift für Chemie," 4 parts.

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THE CHEMICAL NEWS.
Vol. II. No. 5. American Reprint.

ON SOME POINTS IN CHEMICAL GEOLOGY.

BY DAVID FORBES, F.R.S., ETC.

No. III. Dr. Sterry Hunt's Geological Chemistry.

In considering the mutual relations of the sciences of chemistry and geology, the student must always bear in mind which of these two sciences is to form the basis or starting-point for his inquiries, since this cannot fail to exercise an important influence on his reasonings and deductions.

In what Dr. Hunt calls my chemical geology, I have taken geology as my basis, and then endeavoured to apply chemistry, especially experimental chemistry, to the explanation of known geological phenomena.[*]

On the other hand, however, Dr. Hunt, in what may be termed his geological chemistry, starts from data purely chemical, and then looks round for geological instances to which these may be applied; thus, for example, starting from the fact well known to chemists, that a solution of carbonate of soda will precipitate carbonate of lime from a solution of chloride of calcium, he at once asserts that—

> "The whole of 'the calcareous strata, the marble and various limestones which we find on the earth's surface' have been precipitated from the ocean by a solution of carbonate of soda."

Again, observing that in the laboratory the reactions of the compounds of magnesia with carbonic acid under a dense atmosphere of that acid, might be used in facilitating the separation of the sulphate of lime (gypsum), or of the double carbonates of lime and magnesia (dolomites), he at once jumps at the conclusion—

> "That all the magnesian limestones and gypseous strata from the most ancient up to those of the tertiary period, were formed in a dense atmosphere of carbonic acid."

In the face of these assumptions, I contend and feel confident the geological world will support me in believing, that no geologist whosoever if applying the study of chemistry to the explanation of the phenomena of his science could by any possibility ever have arrived at such sweeping generalisations.

When the safety of Rome was endangered by the victories of Hannibal, the advice of Scipio to the Romans was to save Rome by attacking Carthage; and the communications of Dr. Hunt contained in the Chemical News of January 17th (*Am. Repr. Mar.* '68, *page* 107) and the *Geological Magazine* of February 1st, evidently prove that he is determined to pursue a similar course, yet I trust with a very different result, since in the present case I imagine that the forces at command will be found quite adequate for offence as well as defence.

In this discussion, however, much more trouble is likely to be caused to me by the method in which Dr. Hunt carries on his scientific warfare, and which seems to partake of the character of the country in which he resides, where the Indian system used to be to worry out the enemy by skirmishing, but never to attack strong points; and the history both of scientific discussion as well as of nations, shows how very effective such a plan of operations may prove even in the defence of a very weak cause.

For this reason, therefore, I have considered it prudent to keep the main points under consideration as prominently in view as possible, and not to allow the discussion to become so diffuse as to risk losing sight of them, which I fear the reader of Dr. Hunt's communications may be likely to do; acting on this determination, therefore, I have in my reply to Dr. Hunt's paper in the Chemical News, given a plain and concise statement of the points (numbered 1 to 9) in which I have presumed to differ from Dr. Hunt's opinions, and as I now find nothing in his subsequent communication to the *Geological Magazine* of February 1st, which could in any way tend to shake my previous conviction as to the unsoundness of these points, I must be content to wait until Dr. Hunt may condescend to bring forward further evidence in their defence.

If now, however, after a perusal of Dr. Hunt's paper in the February number of the *Geological Magazine*, it is compared with the text of his previous communication in the Chemical News of January 17th (*Am. Repr. Mar.* '68, *page* 107), it will be perceived, as the editor of the *Geological Magazine* has already observed, to be to a great extent the same, and in many parts even *verbatim*; and remembering the puerile accusation brought against me by Dr. Hunt, that I "for some unknown reason withheld from the readers of the Chemical News" matter which I published in the pages of the *Geological Magazine*, it really is amusing to observe that Dr. Hunt in like manner has reserved for the readers of the *Geological Magazine* several most interesting observations which probably he may have considered (and with some reason also) as beyond the capacity of the chemists who patronise the Chemical News; as, for example, the following lucid expositions:—

> "As for the noble metals, whose compounds with oxygen are decomposed at elevated temperatures, their great volatility as compared with earthy and metallic oxides would keep them in the gaseous form till the last stage of precipitation of earthy oxidised matters, when by far the greater part of the globe was probably solidified. Hence we now find them in the earth's superficial crust."

And a little further on, he adds:

> "We cannot conceive anything else than the production of a homogeneous oxidised silicated mass, upon which at a late period would be precipitated the noble metals."

Chemists will not require any comments upon the above, but as they have been accustomed to regard some of the noble metals, platinum for example, as amongst the most refractory bodies known, they will be interested in Dr. Hunt's discovery of their great volatility at heats below which silicates solidify at, as well as the information that the extreme refractory

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[*] Here it should be explained that Dr. Hunt, by his having some time back published, both in France as well as in England, an outline of his principles of chemical geology, has fairly laid himself open to having his views both criticised and disputed, whilst, on the contrary, Dr. Hunt's knowledge of my views on this subject appears to have been derived from sketchy allusions to my opinions scattered throughout the two papers in connection with this discussion contained respectively in the *Geological Magazine* of Oct. 1, and the Chemical News of Oct. 4 last year (*Am. Repr. Dec* '67. *page* 281). Although his virulent onslaught might for this reason be considered hardly as altogether fair, I so far from objecting to it, am on the contrary truly thankful to Dr. Hunt for thus enabling me to strengthen any weak points, and for inspiring me with more confidence than before in the views on chemical geology since brought forward by me and now in the press.

nature of the other metallic oxides had been so completely demonstrated, since some of them at least, as lead, bismuth, antimony, molybdenum, &c., have not been hitherto so considered.

Geologists, however, will not all feel convinced by Dr. Hunt's mere assertion, that the noble metals have from the beginning been in the earth's superficial crust, precipitated on to it from the skies like Jupiter's golden rain, but may also be inclined to believe that they may possibly have been carried up from below.

By a curious coincidence my answer to the criticisms of Dr. Hunt, which appeared in the following week's Chemical News, is also to be found in the *Geological Magazine* of Feb. 1, in which Dr. Hunt's second communication appeared, and I was glad to find that had I even been previously acquainted with the contents of this latter paper, I would not materially have altered my remarks, although I should have added a few more words in reply to some minor points brought forward by Dr. Hunt, which did not appear in his previous one in the Chemical News.

The only important one now advanced is set forth in Dr. Hunt's courteous request for Mr. Forbes to explain "the intervention of water in all igneous rocks, which, as he declares, are outbursts from the still fluid interior of our globe."

The above words do not exactly express my views, since I maintain that igneous rocks have their source in some *reservoir or reservoirs* of still fluid matter in the earth's interior; and I see no difficulty in explaining, by the action of capillarity and heat, the infiltration of the requisite amount of water for the supply of such a source.

Not wishing, however, to accuse Dr. Hunt of "unfamiliarity with geological literature," to use his own words, I could not suppose him ignorant of the writings of Daubrée, whose labours in the field of experimental geology are well known, and it seemed strange that Dr. Hunt should have overlooked the fact that this question had been fully answered by this gentleman, whose words are—"En résumé sans exclure l'eau originaire et en quelque sorte de constitution initiale, que l'on suppose généralement incorporée aux masses intérieures et fondues, M. Daubrée est porté à conclure de l'expérience ci dessus relatée, que l'eau de la surface pourrait, sous l'action combinée de la capillarité et de la chaleur descendre jusque dans les parties profondes du globe."

Always preferring, when possible, a reference to fact or experiment than to authority, I would advise Dr. Hunt, in order to form a conception of such strange action, to examine a common Gifford or other injector used to supply feed water to a high pressure boiler, and he will soon perceive how it is possible that the very forces which otherwise would prevent the entrance of the water into the boiler can become the very means of forcing it in.

Dr. Hunt next asks me to remember "that the oldest known series of rocks, the Laurentian, consists of quartzites, lime-stones, and gneiss, evidently of sedimentary origin, and derived from still older sedimentary rocks." When I was in Canada, what little I did see of the Laurentian rocks, did not at all prove to me that they had been derived from still older *sedimentary* rocks, but on the contrary, whilst believing that the Laurentian and quartzites were of metamorphic sedimentary origin, and that the lime-stones were of metamorphic organic origin, I inclined to the conclusion that the materials out of which they had been reconstructed had most probably been the débris of still older igneous rocks, a view which I have maintained since 1854, with regard to some of the analogous Norwegian rocks, which I understand Dr. Hunt claims as Laurentian.

To refresh my memory, however, I have read over the description of the mineral characters of these rocks contained in the report of the geological survey of Canada, pp. 24-29, but can find therein no evidence whatsoever to the contrary, and therefore without disputing the correctness of Dr. Hunt's assertions as to points where he ought at least to be at home, I would ask whether this statement is founded on facts or hypotheses.

Dr. Hunt then devotes a whole page to what appears to be an inquiry as to who first showed that water played a part in igneous action, a subject which may be of personal or historical interest, but which is quite unconnected with the questions at issue, for in the consideration of nature all geologists will persist, notwithstanding whatever Dr. Hunt opines to the contrary, in regarding igneous action as volcanic action, and volcanic action as igneous action, nor can they imagine for a moment that any person except one who never had seen a volcano in eruption could be blind to the evidence of his senses, and deny the co-operation of vapours and gases in volcanic action.

That the results of Mr. Scrope's admirable researches should have been discredited, ridiculed, and declared unchemical, should be a warning to chemists in future not to hazard such opinions without having studied them in the field as well as in the laboratory.

As Dr. Hunt now brings forward the question of the density of quartz, it may be as well to remind him that all arguments based upon such data must necessarily be invalidated by the fact that the specific gravity of quartz crystals out of true volcanic lavas is found to be 2·6, or the same as the quartz in granite, whilst Mr. Sorby's microscopical examination of the quartz found in recent lavas conclusively proves that it can have crystallised out of the molten mass, and not necessarily, as Dr. Hunt would have us infer, merely been entangled from the débris of originally sedimentary strata.

Having long occupied myself with the application of the microscope to geology, and having repeated many of Mr. Sorby's experiments relating to this subject, I do not even think it necessary to contradict Dr. Hunt when he accuses me of not understanding Mr. Sorby's views, being quite content with that gentleman having expressed himself most decidedly to the contrary. Whilst I now recommend Dr. Hunt to commence the study of microscopic geology, I can at the same time well imagine his being disconcerted when on opening the February number of the *Geological Magazine*, in which his own paper appeared, he at the same time found a few lines from Mr. Sorby quite sufficient to annihilate the deductions he had so elaborately arrived at from a study of that observer's memoirs with a view to make them serve his own purposes.

All the other points have already been considered in my paper in the Chemical News, and I would only remark with regard to Dr. Hunt's criticisms upon my chemical geology, that it is probable that some of them would not even have been brought forward by Dr. Hunt, had he waited until an outline of these views, now in the press, had appeared, instead of selecting for attack disjointed fragments or sentences apart from their context; thus for example, in the case where he accuses me of being ignorant of the laws of diffusion: he would have found my opinion expressed as follows:—

"Whilst on the one hand the zones formed in the earth are considered to have possessed a somewhat stable or permanent character, those present in the atmosphere would on the contrary be the reverse, for no sooner had the gasiform products forming them, by in the first instance obeying the impulse of gravity, and so overcoming the counteracting tendency of the laws of the diffusion of gases, than these latter would assert themselves, and in process of time entirely obliterate this arrangement."

And again:—

"As before stated, this arrangement would gradually be obliterated by diffusion, but as the element of time is one of vital importance in considering the effects of diffusion, it is imagined that before being obliterated, this arrangement may still have had considerable influence in modifying the chemical reactions which took place at this period in the earth's history."[*]

Dr. Hunt, whose knowledge of the laws of diffusion does not seem to include any appreciation of the importance of the element of time in their consideration, might just as well maintain that a lump of sugar could not reach the bottom of a tumbler of water because sugar will dissolve in water.

As Dr. Hunt seems to have great respect for authorities on each subject, I will have great pleasure in submitting the question as whether my proposition is invalidated under these circumstances by the action of laws of diffusion to Mr. Graham, the great expounder of these laws, and abide by his verdict.

In the discussion of new views, more, however, is required than mere quotations from old authorities, what is specially required are facts and experimental evidence; it must also be remembered that much depends upon the mode in which authorities are made use of in such discussions, since it is often an easy matter to select passages or disjointed fragments from the published works of authorities which may appear to support almost any view which may be taken of a subject under consideration.

Dr. Hunt, whose papers consist in greater part of a compilation of references to numerous authorities, from the time of Thomas à Kempis down to that of Sterry Hunt, seems to be quite aware of this fact, as an instance or two will testify.

Thus, when Dr. Hunt quotes Hopkins in support of his views as to the consolidation of the molten sphere, he takes good care not to inform his readers that Hopkins distinctly declares his opinion that the exterior was not the last to solidify, but would have consolidated and formed a crust before the interior had become entirely solid, a view which I have adopted on his authority, and which is diametrically opposed to Dr. Hunt's opinion, that—

> "The surface of the earth immediately previous to its entire solidification was a 'a liquid bath of no great depth surrounding the solid nucleus.'"

Again, although he finds it convenient to quote Forchammer in reference to some minor points quite beyond the limits of the present discussion, he seems to be quite unaware of the fact that the idea of the saline crust of chlorides, &c, which he ridicules my having adopted, was long before propounded by Forchammer, who first made the calculation that the quantity of chloride of sodium in such a crust would have been sufficient to have clothed the entire sphere with a coating of salt some 10 feet in thickness.

And yet, again, when he refers to Sorby's experiments as corroborating his views, as for example that quartz cannot be a volcanic product, *i. e.*, a product of igneous fusion in nature, his deductions are at once put to rout by the few lines from Sorby himself brought forward in my last communication to the Chemical News.

On the other hand, after a full consideration of the various memoirs of Hopkins, Forchammer, and Sorby, along with a careful repetition of many of their experiments, I have failed to discover any point inconsistent with the views I have advanced, and I am further enabled to find much evidence in their favour in the writings of Daubrée, Durocher, Bunsen, Phillips, and other eminent scientific men whose opinion Dr. Hunt evidently considers as quite beneath his consideration.

To prove that it is better to stay at home in one's laboratory than to travel wide and far in order to study nature's operations in the field, as is considered necessary to the geologist by Sir Charles Lyell and other eminent men, Dr. Hunt quotes, from Thomas à Kempis, "the wise saying passed into a proverb among churchmen"—that "those who make many pilgrimages rarely become saints."

In this we are quite of accord, since it is well known that a knowledge of the world acquired by travel is the best antidote to bigotry or one-sided opinions. What we require are geologists, not saints, and although it may be that in Canada geologists are esteemed in proportion to their saintly pretensions, experience on this side the Atlantic does not tend to prove that any of the natural sciences have been as yet much advanced by the labours of the would-be-saintly portion of the community.

As I have previously explained, I was induced to enter into this discussion (which I am still confident will do good to science by energetically ventilating some obscure points) by the special invitation conveyed in writing from Dr. Hunt "to have a friendly fight;" but I now find, if I may judge from the style of that gentleman's communications to the Chemical News and *Geological Magazine*, that his idea of scientific warfare consists in an attempt to overwhelm and crush his opponent with sneers and countless accusations of incompetency and ignorance:[*] ignorance of chemistry, of geology, of petrology, mineralogy, microscopy, literature of the subject, &c., &c., &c., whilst at the same time he does not fail to herald in his own views as what might be termed the quintessence of the combined "results of modern investigations in physics, chemistry, mathematics and astronomy." Would it not have been more prudent, as well as more becoming, to have left to our readers the task of forming their own judgment upon the evidence on both sides brought before them in the course of this discussion?

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[*] It must be remembered that these great bodies of gases and vapours are supposed to be the results of a general and simultaneous act of chemical combination *in situ*, and not to have been slowly gathered together from the realms of space.

[*] Dr. Hunt does not content himself with mere accusations of ignorance, for when disputing my statement that the reactions of the compounds of magnesia with carbonic acid in an artificially compressed atmosphere of that acid, had long been employed in manufactures,—he uses the words, "here it becomes difficult to admit the plea of ignorance which suggests itself for most of Mr. Forbes's errors and mis-statements." I may merely add, that since the appearance of Dr. Hunt's paper in the Chemical News of Jan. 17th (*Am. Repr.*, *March*, '68, page 107), I have received various communications from chemists and others connected or acquainted with this manufacture, not only offering to supply facts in full corroboration of the truth of my assertion, but also directing my attention to a long expired patent (No. 9102. 1841) of the late Mr. Pattinson, of Newcastle, in which these very reactions are distinctly embodied.

Having no pretensions, either to being a saint, nor like Dr. Hunt, to be versed in saintly lore, I cannot quote Thomas à Kempis, yet I can nevertheless follow his example and wind up with an old quotation; for even at the risk of appearing still more uncourteous, I really cannot resist the temptation to remind him of the old saying, passed into a proverb amongst laymen, that "curses, like chickens, come home to roost."

ON THE USE OF THE

SPECTROSCOPE AND MICROSPECTROSCOPE

IN THE DISCOVERY OF

BLOOD STAINS AND DISSOLVED BLOOD, AND IN PATHOLOGICAL INQUIRIES.

BY W. BIRD HERAPATH, M.D., F.R.S.

The discovery of and recognition of blood stains, and more especially of human blood, has been a problem which has long baffled the skill of the chemist and the more highly-trained medico-legal eye of the microscopist, and any means by which our medical jurists can lessen the difficulties, and facilitate the inquiry, must be hailed as a boon by all scientific observers. Hitherto the chemical difficulties of the question have been the greater in the inverse proportion to the quantity of stain, and many minute and perfectly evident spots would even evade recognition by the test tube in consequence of the smallness of their size, or the disadvantages of their position. Whilst the microscope would also fail in their detection if from any peculiar circumstances the globules could not be safely and securely removed from the tissues which were under careful examination; independently of which the chemical and physical changes induced in the characters of the blood globules by the various menstrua employed in their removal, rendered recognition by micrometrical admeasurement a very doubtful and uncertain operation.

When blood globules are to be discovered floating in a saline fluid, such as urine, saliva, or the generality of mucous discharges, the microscope will readily detect their presence; and should the density of the fluid be very closely equal to the specific gravity of the serum of the blood, scarcely any change in their physical characters would occur, and it would then be possible to determine their exact form and size, and render their probable source a question of easy solution; but when the blood has been dried and long exposed to the air, it is no longer easy to reproduce the blood globules in their pristine form and optical characters, as the various media employed to dissolve the clots act on the globules with more or less celerity. It is usual to employ either solutions of cane or grape sugar, or mixtures of glycerine and distilled water having a density of 1·030°. Some observers have employed saline solutions for this purpose, others have used a strong solution of arsenious acid in distilled water; the objects which each have in view being the removal of the blood discs, and the non-alteration of their physical characters. If the action of the solvent or medium, from its deficient density or peculiar chemical properties, has resulted in a destruction of the blood globules and a solution of the colouring matter, the microscope would no longer either recognise its mammalian character or even assure us of the presence of blood.

In this condition of the enquiry the chemist alone, by the employment of tests, could decide the question of the presence of dissolved hæmatine, and would chiefly rely on the action of heat in coagulating the albumen and destruction of the colour; whilst upon another portion of the fluid he would assure himself that ammonia would not produce any great change of colour, thus deciding the non-vegetable character of the colouring matter. But during the past two or three years an addition has been made to our optical instruments in the aid which we have obtained in the recognition of various substances by the effects which they have on the absorption of different portions of the spectrum, and we have various forms of spectroscope according to the purposes for which they were intended.

The first invented spectroscope was a very efficient but cumbrous instrument, and astonished the world by the discovery of four new metals in consequence of the remarkable peculiarities of their coloured flames, and thallium, cæsium, rubidium, and indium have been thus isolated from other bodies, and added to the list of elementary bodies. Shortly afterwards Professor Stokes introduced a modification of this instrument, which enabled liquids and coloured fluids to be submitted to the same mode of testing them by their absorptive spectra, and on the table is one which I have long employed for this purpose, a more powerful and efficient instrument than that recommended by him for these experiments. In this instrument, essentially a direct vision or Hofmann's spectroscope, the liquid to be examined is placed in a small test-tube, and that is held in a clipped spring, which supports it during the examination, whilst a bright light is transmitted through the liquid previous to its analysis in the spectroscope,—the spectrum showing various bands of absorption in well-marked optical liquids, some of the most beautiful of which are certainly weak solutions of permanganate of potassa, and dilute solutions of cruorine and hæmatine. In the first case five dark bands are seen in the green part of the spectrum, and in that of blood two sharply defined black bands are seen, one in the green, and another on the border of the orange ray. The intensities and positions of these bands vary according to the age of the blood stain, and result from the alteration in the colouring matter of the blood, from the effects of drying, and from exposure to air.

The stains when old have a much less decided or evident absorption, the bands are weaker, and an additional band of a diffuse character is found in the red ray. But it does not appear to be possible to form any positive, or accurate, opinion on the age of blood; from various observations it has been ascertained that these changes take place with more rapidity under some circumstances than in other apparently similar cases.

In all optical experiments on blood, it is necessary to use *excessively* dilute solutions of the colouring matter; otherwise the fluid is absolutely opaque to light—or if it transmit any light at all, nothing but the extreme red rays are observed. When still more dilute, the blue end of the spectrum is quite absorbed, and so are two bands in the green, and occasionally, also, one in the red. These optical properties of blood were first pointed out by Hoppe (*Virchow's Arch.*, 1862, vol. xxiii. p. 446), and subsequently by Professor Stokes (*Proceedings of the Royal Society*, 1864), and Mr. Sorby (*Quarterly Journal of Science*, 1865).

Professor Stokes has investigated the effects of different chemical reagents upon the colouring matter of blood (*Proceedings* of the Royal Society, 1864, p. 355, *et seq.*) and he has arrived at a very intelligible solution arterial and venous blood in the recent fluid condition contains a substance called cruorine, which like indigo is capable of existing in two different states of oxidation and colour. That in arterial blood, scarlet cruorine is the form in which it is found, and in this condition very dilute solutions produce two very sharply defined black bands of absorption, one close to and parallel with the sodium line D, and is the more intense of the two; whilst the second is found in the green ray about the breadth of the previous band distant from it.

On submitting the scarlet cruorine to deoxidising agents to a moderate extent, the cruorine becomes the deoxidised or purple cruorine, and then the solution is light or deep purple according to its degree of concentration.

Examined by the spectroscope in this condition, only one deep broad band of absorption is found, which commences about the solar or sodium line D, and then passes onwards to the green, absorbing the whole of the yellow and part of the green band of the spectrum. It is remarkable that upon shaking up a dilute solution of scarlet cruorine with an atmosphere of carbonic acid the fluid does not exhibit the appearance, or spectrum of venous blood. It is evident therefore that the blue colour of venous blood is not produced by the presence of carbonic acid in solution, but to a *reducing action* in the capillaries analogous to that of other reagents, whilst the influence of sulphide of ammonia, sulphuretted hydrogen, or the hydrated protoxide of iron, or the protochloride of tin, or the peculiar deoxidation of arterial blood due to the changes going on in the systemic circulation, are all instances in which such a deoxidation or reduction has been in action. A dilute solution of scarlet cruorine set aside in a full closely corked phial, or with very little air, will shortly pass by spontaneous deoxidation to the purple cruorine, and will then exhibit all the optical phenomena of this peculiar substance, but resumes its scarlet colour by agitation with air. Professor Stokes says that of all reducing agents, an ammoniacal solution of protochloride of tin (previously treated with sufficient tartaric acid to prevent the precipitation of oxide of tin), is the most efficient reducing agent, and as it is colourless it does not interfere with the spectroscopic appearances. He says that when a few drops of this solution are added to a solution of scarlet cruorine, the latter is presently reduced and we have the spectrum of purple cruorine. If the solution be now shaken up with air, the cruorine is reoxidised to the scarlet form. On standing a few minutes it again becomes reduced, and the solution may be made to go through these changes repeatedly until all the tin has passed to that of complete oxidation.

But when blood or scarlet cruorine is treated with an *excess* of deoxidising material, or has become changed by long exposure to air or by drying, or by the effect of sulphurous and some other acids, the colouring matter becomes brown and more insoluble in water. It is in fact then changed into *brown hæmatine*, which has very different optical properties from that of either scarlet or purple cruorine. The two dark bands of absorption are still found, but have become very faint, and much less sharp in outline, whilst a third dark band is seen in the red ray; of course, on the less refrangible side of the solar or sodium line D. This change, when produced by age and exposure, is sometimes months in

Now just as cruorine can exist in two different forms, so can hæmatine; and we have either the brown or the red hæmatine, the latter being produced by deoxidising the brown hæmatine by some reducing agents, as hydrated protoxide of iron. Then two bands of absorption occur, as in scarlet cruorine, but capable of being readily distinguished from those by their position and different degrees of intensity.

In red hæmatine an interval exists between the sodium or solar line D and the margin of the first band of absorption, which in red hæmatine is less distinct or *sharp* than that in the scarlet cruorine, or than its fellow in the same spectrum; both of which therefore are in red hæmatine found in the green rays. Most chemical reagents convert scarlet cruorine into brown hæmatine without any previous passage through the stage and properties of purple cruorine. The effects of reduction on brown hæmatine are evanescent, and the solution rapidly becomes deficient in optical power; it does not assume the former properties of brown hæmatine. The absorption bands fade away and disappear, the one nearer to the sodium line D being the more persistent, and remaining sharp during several days; this takes place even if the bottle be air-tight. Re-treatment with protoxide of iron will again reproduce the two absorption bands as before. It is brown hæmatine which is usually discovered in old blood stains, but red hæmatine in dry and more recent blood clots. A specimen on the table six months old still shows the two bands of red hæmatine, whilst one of two years old shows the spectrum of brown hæmatine.

It may be as well to state, that these specimens of dried blood had been kept in a moderately dry room, powdered, and in a paper pill-box for some portion of the time, but during the major part of the period on cloths exposed to the air without any care, and in the ordinary atmosphere of an inhabited room, with gas burning nightly. Solutions of these specimens of blood in distilled water, after having been kept closely corked for some days, underwent a peculiar change. Both the brown and red hæmatine were *spontaneously changed* into purple cruorine, and now show only the one broad band of absorption due to that colouring matter. This effect is probably due to reduction by sulphuretted hydrogen. In *no specimen* would it be possible to detect *any globules by the microscope*, as the colouring matters have been all dissolved in distilled water, and the globules destroyed. The microscope would therefore fail in detecting blood in all of them, whilst the chemist might probably recognise it in most of the fluids, and so will the spectroscope. When an old blood stain has been so changed by exposure to air that the hæmatine gives but very faint and indistinct absorptive bands, it is possible by deoxidising the solution of hæmatine by means of a little recently precipitated hydrated protoxide of iron to reproduce the bands in nearly the same intensity, though slightly differing in position, than they were in blood stains of a very recent period of their formation, when scarlet cruorine would of course be the colouring material.

This experiment is readily made by adding a few drops of a weak solution of proto-sulphate or protochloride of iron, and then a few drops of liquor ammoniæ; the green hydrated protoxide of iron resulting has a great affinity for oxygen, and at once reduces the hæmatine and restores its colour and optical properties.

It is essential that putrefaction should not have actually destroyed the hæmatine. The spectroscope which has been mounted upon this stand is so perfect in its action that it readily exhibits Fraunhofer's lines in solar or lunar light, and it divides the bright yellow sodium line D into two when properly adjusted. It therefore gives great accuracy of observation, and the stronger dark lines of the solar spectrum become so many fixed points for the comparison of the position of the dark absorptive band of various coloured solutions when they are observed by daylight.

One disadvantage attendant upon the employment of this form of spectroscope is the quantity of material necessary to be employed. Several drops of blood must be at the disposal of the operator to get a sufficient coloured solution to fill the little test-tubes used in the optical examination. By modifying the instrument, and introducing a larger tube for containing the liquid to be examined; say a column of six or eight inches in length, it would be possible to discover one drop of scarlet cruorine in a pint of distilled water without much difficulty, a quantity so minute that no perceptible colour could be visible to the unaided eye, and no other method of analysis would be capable of detecting it. Quantities like these are not always to be had, and a recent well-known case in which I had the opportunity of first using the micro-spectroscope in a medico-legal inquiry would have altogether failed if I had depended alone upon this "fluid spectroscope," as nearly all traces of blood stains had disappeared from the weapon employed by the murderer in consequence of the hatchet having been left exposed in the woods near Mountain Ash for several weeks after the deed was accomplished (case Reg. *v.* Robert Coe, Swansea Spring Assizes, 1866). It was only on the removal of the head of the hatchet that any appearances of blood were to be obtained from the surface of the handle, which had been protected by the iron ring, and on carefully making thin sections of these stained portions of wood, and treating them with distilled water, a few drops only of a brownish coloured fluid were obtained, which coagulated and became discoloured on boiling; also another drop when placed in a very minute tube, about half an inch long, and the $\frac{1}{8}$th of an inch in diameter, the total contents of which tube were one grain and $\frac{1}{3}$rd of distilled water, gave the optical absorptive bands due to old blood.

This little drop of bloody-coloured fluid was placed on the stage of the microscope, and examined with an inch Ross objective, illuminated by an achromatic condenser, and the microspectroscope was inserted into one of the tubes of a binocular microscope as an ocular lens would be employed. This form of instrument is that known as the Sorby-Browning spectroscope, and it admits of great precision, as it has a lateral spectroscope as well as a terminal one. These two spectra appear side by side in the field of view, and being perfectly parallel, admit of examining substances by two sources of light at the same time, or enable us to make comparisons between two different or similar substances at the same time and by the same kind of illumination —the two spectra being both visible with the same eye. This form of instrument is very sensitive to small quantities of blood; and it would be perfectly easy to detect and ocularly examine the blood contained in the stomach of a rascally "flea," and even dilute it with a teaspoonful of water without losing its properties; especially if he had made anything like a decent forage upon some sanguineous individual. But other forms of spectroscope have been adapted to the microscope by different opticians or inventors. One great objection to the other forms consists in the greater complexity of the arrangements and the variety of adaptations to be made, rendering the observations both difficult and troublesome, involving great loss of time, and, of course, greatly multiplying the chance of failure. But to show how small a quantity of blood is really necessary for recognition with this instrument, Mr. Sorby has distinctly obtained the absorptive bands in a single half globule of dried blood; in order to obtain this result the object was illuminated by a powerful achromatic condenser, and one of Smith and Beck's new $\frac{1}{25}$ objectives was employed.

However, without having gone as far as this, my own observations have proved that it is possible to obtain very evident results from less than one-thousandth of a grain of dried blood, the colouring matter of which had been dissolved out by one drop and a half of distilled water. In fact, comparative experiments proved that in the Mountain Ash case the quantity of blood experimented on, and productive of conclusive results, did not exceed one-thousandth part of a grain; and the justness of the sentence was afterwards proved to the satisfaction of all parties by the confession of the prisoner previous to his execution.

However, this optical or microspectroscopic result was in this case also confirmed by the microscopic examination and detection of the blood globules, as well as by the chemical testing of the solution of hæmatine, or rather red hæmatine, for it had not passed to the extreme state of change visible in old dried blood.

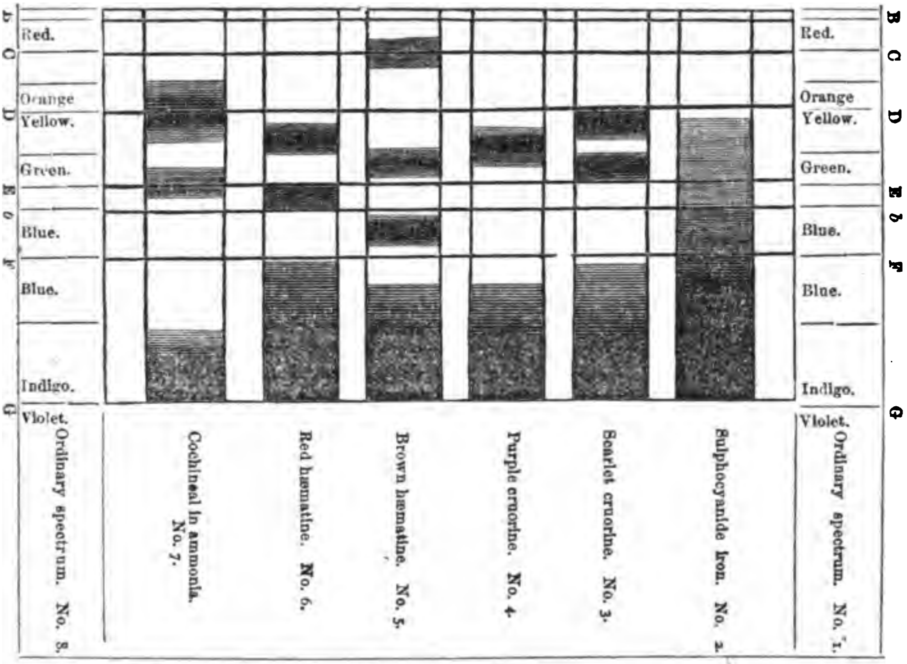
It is somewhat remarkable that though various other bodies have, to the eye, all the appearance and colour of blood, yet none of those usually met with have any spectra to be mistaken for those of the various forms of blood colouring matters herein described. The generality of soluble red colouring matter absorbs more or less of the violet, blue, green, or yellow, and even orange, rays of the spectrum *continuously*. Some wholly absorb the spectrum with the exception of the red ray which they transmit.

The well-known sulphocyanide of iron often called (and used by conjurors and by chemists) artificial blood, is strikingly wanting in those optical absorptive powers or bands so indicative of cruorine or hæmatine in their various forms. Spectrum analysis is capable of rendering great service in chemical and pathological enquiries, as by means of the optical spectra blood may be easily recognised in urine, and detected in some forms of albuminuria, even if it be also charged with the colouring matter of bile. Highly jaundiced urine absorbs all the blue end of the spectrum, but as the green, orange, and red rays are unaltered, the two bands of scarlet cruorine are readily seen. The recent menstrual fluid, when dissolved and properly diluted, gives the spectrum of scarlet cruorine; so does urine mixed with menstrual fluid even if highly bilious. Two substances only have been found comparable in their optical effects to those of hæmatine,—a dilute ammoniacal solution of carmine, and a similar solution of cochineal in ammonia, which colouring matters are very unstable and fade quickly. In both these liquids the same colouring matter exists, as carmine is produced from cochineal. The two absorptive bands are much broader and more diffuse than any of the optical appearances due to the colouring matter of blood, though most like those of brown hæmatine, and only a novice in spectrum analysis could possibly mistake the one for the other, whilst the least attempt at chemical investigation would pronounce them different, the action of heat alone being sufficient to coagulate the colouring matter of blood, whilst the cochineal and carmine would remain unchanged. Reducing agents would also settle the question definitely.

Acids immediately change the colour of cochineal solutions to a reddish orange, deficient in absorptive, and even a spontaneous coloration will take place in the ammoniacal solution of cochineal. Solutions of carmine are more permanent. Acetic acid produces no change at first, but eventually the colouring matter is precipitated. Sulphide ammonium does not alter it in the least, nor does the alkaline solution of protochloride of tin. On adding protochloride of iron and ammonia to any solution of carmine, the colouring matter is immediately precipitated in combination with the oxide of iron as a brown or maroon coloured compound.

But one great safeguard in medico-legal enquiries will be the absence of cochineal or carmine from those positions in which blood may by any possibility be found, some cloth fabrics alone being dyed by a mordanted cochineal. Some scarlet clothes also are of this character, and the carmine colour being fixed by alumina, would be insoluble in cold water; whereas the cruorine or hæmatine would dissolve with more or less facility according to its age. It is evident, therefore, that all these considerations render the detection of blood stain by spectrum analysis a matter of but little doubt or difficulty, even when in minute quantities; and, in conclusion, although spectrum analysis does not go one step farther than we were before in our powers of discriminating human blood from that of other mammalian, or red-blooded creatures, yet it gives us greater facilities of demonstrating the presence of the colouring matter of blood, even in inconceivably minute and almost invisible proportions, whilst the facility with which the observations are made is a great, if not the greatest recommendation to the employment of this method whenever practicable.

Chart illustrative of Dr. Herapath's Paper on the Spectroscope.



In this Chart eight spectra are exhibited, two being the ordinary solar spectra, with seven of the more strongly marked solar lines drawn perpendicularly through all the other spectra as indices for the real position of the various absorption bands. The strong black bands towards the violet ends of the spectrum (which are also absorbed) show the amount of the usual absorption of red fluids of that end of the spectrum; but all the other bands are indicative of the fluids examined.

METHOD FOR THE

DETERMINATION OF SILICON IN IRON AND STEEL.*

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(Concluded from p. 171, Am. Repr., April, 1868.)

The oxide of iron is easily dissolved in the heat of a water-bath. The silica is again thrown on a filter, washed, dried, ignited, and weighed. 0·016 gramme of silica answers to each 0·001 gramme of silica when 3 grammes of iron have been used in the analysis. To ensure the purity of the silica, it may be mixed in a platinum crucible with ten times its weight of pure fluoride of ammonium, diluted with water to the thickness of syrup. The water must be evaporated on a water-bath, and the crucible heated, with a cover on it, by a gradually increasing heat over a spirit-lamp to a full red. If nothing is left in the crucible, the silica was pure, and has passed off as silicon fluoride; but, if any thing remains, the operation with fluoride of ammonium must be repeated until a constant weight is obtained. When iron contains tungsten, for instance,

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* From *Engineering*, July 24, 1868. Translated by C. P. Sandberg.

some tungstic acid is formed, and this accompanies the silica for the most part, being dissolved by the soda solution, but not volatilised by the use of fluoride of ammonium. Vanadic acid also accompanies the silica, behaving as tungstic acid. Instead of using fluoride of ammonium, it is preferable to use hydrofluoric acid, with which the silica is moistened, and the evaporation is conducted on a water-bath. 0·1 gramme of pure silica obtained from analysis is easily dissolved by 2 c.c. of hydrofluoric acid (of the strength that 2 c.c. of it are neutralised by 1·5 c.c. of a saturated solution of ammonia 0·95 sp. gr.). When using hydrofluoric acid, getting it on the hands or exposure to the evaporating gas must be carefully avoided. The mass left on the filter from the soda solution may be composed of—besides graphite—slag, oxide of iron, oxide of titanium, etc. (but not copper, at least when the iron does not contain more than 1 per cent.); this is dried, ignited, and weighed. The method of separating oxide of iron and slag, when the iron or steel contains both these, is not yet known. If the composition of slag were always alike (which it is not), it would be easy to calculate its amount from either the silica or oxide of iron obtained in the analysis. In a piece of Bessemer iron very red short (that is, it could not be bent at a white or yellow heat without being broken), which contained no sulphur, by several experiments 0·3 per cent. of oxide of iron has been obtained, and only traces of silicon. After ignition, the oxide of iron may possibly be found as sesquioxide. The amount of oxygen, in case that the red shortness is due to this, as it probably is, amounts to less than 0·1 per cent.

When the iron or steel for analysis contains titanium, a part of this substance follows the slag in the form of titanic acid. If such is the case, this must be melted with ten times its weight of acid sulphate of potash, by which it is dissolved; the mass is dissolved in cold water, and the solution precipitated by boiling; the weight is determined, and subtracted from that of the slag. This ingredient has not, however, been found in bar iron or steel in such a quantity as to merit special attention.

Regarding the determination of silicon in cast steel where only a trace of slag is found, the method given below for cast iron may be employed; but 3 grammes at least ought to be taken for each experiment, and the acids for solution in proportion.

In experiments conducted at the Mining Institution for the determination of silicon and slag in bar iron and steel, the amount of silicon has generally varied between 0·01 per cent. and 0·1 per cent.; but in two sorts of good cast steel from Krupp's it has amounted to about 0·3 per cent. Slag in cast steel has been found only in traces, but in another case it amounted to 0·2 per cent.; in good iron wire, prepared from bar iron, converted in a refinery hearth, from charcoal pig iron, 0·33 per cent.; in puddled iron (armour plate), from 0·75 to 3 per cent.; and in an English iron rail, to 4 or 5 per cent.

For the determination of silicon in cast iron, in which no finery slag is found, and only exceptionally blast-furnace slag, the following method has proved suitable: 2 grammes of iron, which has passed a sieve with holes of a diameter of $\frac{1}{60}$ in. at the most. is put into a beaker of 100 c.c. capacity, containing 30 c.c. of hydrochloric acid, sp. gr. 1·12. The beaker is covered with a close-fitting watch-glass, heated without delay, and the liquid kept at a gentle boiling for half an hour.

All the carbon chemically combined with the iron is separated from the liquid in the form of an ill-smelling hydrocarbon gas, by which operation a disadvantageous formation of humus and oxide of iron is prevented (in case that the solution should be required for further researches).

If the carbon formed in the solution is left in contact with the air some minutes before the boiling is begun, it undergoes such a change that it cannot afterwards be decomposed into gas and evaporated; if necessary, some hydrochloric acid is added, and the solution evaporated on a water-bath, until the smell from hydrocarbon gas has ceased. If the graphite in the iron is to be determined at the same time, it is placed, as well as the silica, on a filter previously dried at 95° or 100°, and weighed; well washed with hot water containing 5 per cent. nitric acid, again dried, weighed, and ignited in a porcelain crucible. By deduction of the weight of silica and of the filter ash, the amount of graphite is determined. (It must be observed that the silica dried on a filter contains 6 per cent. water, or only 94 per cent. silica.) If, for instance, the filter weighs 0·125 gramme, and its ash, 0·001 (the combustible substance being 0·124 gramme) and the filter + graphite + silica 0·182 gramme, and the residue after ignition 0·025 gramme (0·001 gramme of this is filter ash), the weight of the silica (0·024 gramme) is, after the drying, 0·0255 gramme, and thus the weight of the graphite becomes 0·182—(0·124 + 0·001 + 0·0255) = 0·0315 gramme. To the solution, after the separation of graphite and insoluble silica, is added 4 c.c. nitric acid, 1·2 sp. gr., and evaporated to dryness. The further proceedings are the same as previously described for the determination of silicon. If it is intended to determine only the silica, the whole solution is evaporated to dryness immediately after boiling. When the silica is red, strong hydrochloric acid is added, as previously described. If the silica is contaminated with titanic acid, vanadic acid, ar tungstic acid, it is operated upon with fluoride of ammonium or hydrofluoric acid, as previously mentioned, whereby the silica is evaporated and calculated by loss. By the above method of dissolving iron in hydrochloric acid, the silicon changes, without evaporation, for the most part, to insoluble silica, which may be filtered and determined. Sometimes a very unimportant part is dissolved, especially if the boiling has been short.

When iron is dissolved in hydrochloric acid without heating (white cast iron is very difficult to dissolve in this way), a still less portion is dissolved, and generally so little that it may be neglected for practical purposes. The washing is performed with hot water containing nitric acid, as previously described.

When the iron is dissolved in nitric acid, a great deal of silica enters into solution.

The different sorts of cast iron appear to be slightly different in this respect. In dissolving cast iron with heat, in very diluted sulphuric acid, a great deal of silica is dissolved, but very little when the water is the least possible; as the water evaporates, the silica settles and becomes insoluble. The method given below rests upon these circumstances, and has proved very satisfactory, and by this the taking away of the acid is avoided, which is both necessary and troublesome when using hydrochloric acid with heat. The amount of silicon has, according to both methods, turned out alike. Traces of silica are always found left in the solution and wash-water. Regarding the determination of silicon in iron, it should be observed that only such vessels may be employed as are unacted upon by the reagents used in the analysis, as otherwise an undue proportion of silicon may be obtained.

Two grammes of cast iron which have passed a sieve of 0·2 of a line, are shaken by small portions at a time into a beaker of 100 c.c. capacity. In this beaker has been previously put 18 c.c. of water with 3 c.c. pure sulphuric acid of 1·83, or 15 c.c. sulphuric acid of 1·23 sp. gr. with 6 c.c. of water.

The beaker is covered with a watch-glass, and placed on a water-bath; if the graphite rises on the side of the beaker, it is pushed down into the liquid by a glass rod. When the iron is dissolved, the watch-glass is changed, after being washed, for a paper cover, and the solution evaporated on a water-bath until no condensation occurs on a watch-glass held over the beaker; 30 c.c. of water are then added, and it is frequently stirred with a glass rod, whilst on the water-bath, until the white iron salt has completely dissolved. The insoluble mass is then thrown on a filter, washed with hot water containing 5 per cent. nitric acid, 1·2 sp. gr. (in order to dissolve all compounds of iron) as long as an iron reaction is given with ferrocyanide of potassium. The filter, with its contents, is placed in a carefully tarred porcelain crucible; it is then cautiously dried, ignited, and weighed. The silica contains 48 per cent. of silicon, and its purity is examined by the method previously mentioned, when such is considered necessary. If the cast iron contains vanadium, this is obtained for the most part as a yellow-brown vanadic acid with the silica, from which it may be extracted by warm hydrochloric acid or ammonia.

When intending to determine at the same time the amount of graphite in the cast iron, the solution is treated, after the separation of the chemically combined carbon, by boiling, as previously described when dissolving the iron in hydrochloric acid. In determination of graphite the use of hydrochloric acid is preferable.

The greatest amount of silicon which has been found here in grey charcoal pig iron was 2·7 per cent., and in white (spiegeleisen) 0·8 per cent. The amount of silicon in pig from coke blast-furnaces is rarely more than 4 per cent. The least quantity of silicon in grey cast iron has been 0·2 per cent., and in white (spiegeleisen) it has not been less than 0·01 per cent. The amount has usually been from 1 to 2 per cent. in cast iron suitable for the Bessemer process, about 1 per cent. in good Franche Comté, and in pig iron for puddling about 0·5 per cent.

From many iron works has been obtained pig iron suitable for refining on the charcoal hearth, which contained about 0·2 per cent. silicon; but from others a greater amount of silicon has been found in the same sort of pig iron, and it is generally presumed that different quantities of silicon require a different construction of the furnace, and a different method of working the refinery. It has been clearly proved by numerous experiments what a great influence the amount of silicon has upon the nature of cast iron, in being more or less easily refined, etc., and at the same time the great importance of paying more attention to its manufacture than has hitherto been done, in order to obtain cast iron, which, with regard to its silicon, may be suitable to the purposes for which it is to be employed.

The amount of silicon in iron of different degrees of hardness from the same charge of the blast-furnace ought to be pretty well valued by the fracture, after some determinations have been made by analysis.

ESTIMATION OF NITRITES IN WATERS.

BY PHILIP HOLLAND.

Dr. Miller, in his paper, "On some Points in the Analysis of Potable Waters,"[*] alludes to a reaction for the detection of nitrites, viz., the property these salts have of liberating iodine from an acidified solution of iodide of potassium.

Dr. Angus Smith[†] asserts than an amount of nitrous acid so small as 1 in 3½ millions of water, may easily be discovered in this manner. In spite of its extreme delicacy, I am not aware than any process for the quantitative estimation of nitrous acid has been founded on the reaction. Most, if not all water analysts, unless they be recent converts, are inclined to be satisfied with permanganate indications, the presence or absence of nitrous acid being assumed according as the permanganate is quickly or slowly decolourised.

The process I am about to describe is one in which the colouration imparted by the free iodine is taken as the measure of the nitrous acid present. For a "colorimetric" standard, I know of nothing better than a solution of iodine in iodide of potassium; about 4 grms. is dissolved in excess of iodide, and made up to the volume of a litre.

In the next place it is necessary to prepare a pure salt of nitrous acid; for this purpose, commercial nitrite of potassium is precipitated with $AgNO_3$, the resultant silver salt washed by decantation, re-crystallized, and dried in vacuo.

To ·3276 grm. of the silver salt, dissolved by heat in water, is added a slight excess of pure NaCl, and the liquid, when cold, made up to the volume of 1000 c.c.

$$10 \text{ c.c.} = 1 \text{ mlgrm. } HNO_2.$$

The iodine solution is "titrated" as follows:—A permanganate burette divided in $\frac{1}{10}$ths of a c.c., and fitted with a float, is filled with it. Two narrow white glass jars are placed on a white slab; on each is marked the point at which a volume of 200 c.c. of water stands. Into one, A, is put an amount of the standard nitrite equal to 1 mlgrm. of HNO_2, together with 6 c.c of iodide of potassium (1 to 10 of water), then distilled water nearly to the mark, and lastly dilute H_2SO_4. The whole is to be mixed and allowed to stand until the colour is fully developed; when that point is reached the second jar, containing an amount of iodide of potassium and acid equal to that in A, is filled to within a short distance of the volume mark with water, and placed under the burette; the iodine solution is then cautiously delivered into it, until the depth of colour is judged to be equal in intensity to that in A. The iodine solution should be of such a strength that 10 c.c. have a colouring power equal to that possessed by 1 mlgrm. of HNO_2 in the presence of iodide of potassium in a volume of 200 c.c. of water. It is unadvisable, when making the comparison, to add the standard nitrite from a burette, to an acidified solution of iodide of potassium, for an obvious reason. It may, however, be suggested that a definite quantity of nitrite should be added together with iodide to the water in the jar, and lastly the acid. Such a method is tedious, in that it would be necessary to make several assays before attaining the desired shade.

The following determinations of nitrous acid were made:—An amount equal to 1 mlgrm. was evaporated

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[*] Journal Chem. Society, May, 1865.

[†] "Estimation of Organic Matter in Waters, with reference especially to sanitary purposes," page 20.

added, then distilled water to within ¼ inch of the mark, and lastly dilute H_2SO_4. After thoroughly mixing, the contents of the cylinder were left undisturbed, for the colour to become fully developed; when that stage arrived it was found that 11·5 was the number of c.c. of iodine requisite to impart the same colour to an equal volume of water. 10 c.c. should only have been required; the excess, therefore, of 1·5 c.c. is the measure of the HNO_2 in the water employed. In order to justify this assumption I evaporated two separate quantities of a litre.

	Iodine c.c.
No 1	1·2
" 2	1·4

These figures give ·12 mlgrm. per litre as the amount of HNO_2 in the spring water. Artificial waters were made by adding known amounts of HNO_2 to common water; the quantity of HNO_2 already existent therein being deducted.

Amount HNO_2 added in mlgrms.	Iodine required in c. c.	Mlgrms. HNO_2 found.
·43	4·5	·45
·86	8·4	·84
1·52	14·8	1·48

The following natural waters were examined:—

A. A well water.

B. A well water, in which nitrates were found in some quantity.

C. A brook water containing some sewage matter.

A.	·23	mlgrm. HNO_2 per litre.
B.	·27	"
C.	·63	"

The process is not suitable when the quantity of nitrous acid is large; whilst it ranges below and up to 1 mlgrm. corresponding results can be obtained.

Some precautions are necessary in certain cases. H_2S and sulphides if present must be removed; the former escapes during the evaporation of the water; the latter may be decomposed by a metallic oxide.

Organic colouring matter can be precipitated by means of chloride of calcium, carbonate of sodium, and a few drops of hydrate of potassium, as suggested by Dr. Frankland. Kaolin could perhaps be employed for the purpose.

Chorley, March 4, 1868.

ON THE DETERMINATION OF TARTARIC ACID.

BY GEORGE H. MANN,

POLYTECHNIC INSTITUTE, TROY, NEW YORK, UNITED STATES.

Ten grammes of the crude tartar are mixed with a sufficient quantity of pure hydrate of potassa, free from carbonate, in order to neutralise the free tartaric acid present; the mixture is evaporated to dryness in a water bath, and heated to redness in a closed porcelain crucible. The tartaric acid is thus decomposed into carbonic acid, which is determined in the following manner: place the warm mixture of the carbonates and carbon in any of the various forms of the apparatus designed for this purpose, and illustrated in Fresenius' "Quantitative Analysis," fill the upper bulb with strong sulphuric acid, tare the apparatus on the balance, admit the acid into the lower part, when carbonic acid will be evolved, and the amount present may be estimated from the loss of weight. Then we shall have the pro-

pressed in numbers, 22 : 150 : : a grammes of carbonic acid : x grammes of crystallisable tartaric acid.

ON THE VENTILATION OF SEWERS.*

BY DR. W. ALLEN MILLER, V.P.R.S.

Determination whether the charcoal injuriously impedes the ventilation.

This could be determined by two methods, viz.—*a.*, by ascertaining the ordinary draught of air in the sewer when there was no charcoal, and then ascertaining the amount of draught after the charcoal had been introduced; or *b.*, by analysing the air before and after the introduction of the charcoal, and ascertaining whether any serious diminution of oxygen or increase of carbonic acid had occurred.

a. The variation of the draught of air in the sewer was examined, as follows:—A puff of smoke was produced by firing a little gunpowder, and ascertaining the time occupied by the smoke in travelling up or down the sewer for a known distance. This method answered its object sufficiently well, but it was found that such slight causes interfered with the strength of the draught, that much less information of value was obtained than had been anticipated, since it was found that the direction of the draught and its amount were liable to be interfered with by local accidents, such as the imperfect closure of a trap, the variable force of the wind, and the opening or shutting of a side entrance: so that the act of entering or leaving the sewer for the purposes of experiment, was more than once found to reverse the direction of the current of air in the body of the sewer during the observations.

So far as the observations go they, however, show that the introduction of the charcoal into the boxes produces a sensible retardation of the current of air. Indeed any other result would be impossible.

Three sets of experiments were made upon this plan; one set before the charcoal was introduced, and two other sets after the boxes had been charged with charcoal.

The average rate deduced from six experiments *without the charcoal* showed a current moving at the rate of 4,254 feet per hour.

The first set of trials, consisting of three experiments, *after the introduction of the charcoal*, gave a current moving at the average rate of 3,263 feet per hour, and the second set of trials, also, an average of three experiments, showed a current of 2,005 feet per hour, in the same part of the sewer.

b. Effect of the charcoal on the chemical composition of the air.

It was ascertained by direct trial that air passed freely through the charcoal in the trays, but no sewer odour was ever perceived in the escaping air; though if the box of charcoal were purposely removed from the ventilating shaft, an immediate and powerful odour of sewage was perceived. The charcoal, therefore, did its work in absorbing the offensive products. It had, however, no direct action upon the atmosphere in the body of the sewer; but, indirectly it might be expected to impair its quality by detaining it for a longer period within the sewer, thereby causing it to lose a larger portion of its oxygen than if a freer current of

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* Abstract from the Report to the Metropolitan Board of Works.

air was maintained. The oxygen during its detention would combine with part of the decomposing refuse, whilst an increased amount of carbonic acid was to be looked for as one of the products of decomposition; besides which a small quantity of carburetted, and occasionally of sulphuretted, hydrogen might occur in the more stagnant parts.

Samples of air were therefore collected for analysis both before and after the introduction of the charcoal.

From an average of eighteen experiments upon the quantity of carbonic acid, made during the month of May, before the charcoal was introduced, and before the lateral sewers had been provided with flaps, the average quantity amounted to 0·106 parts per cent, while in the open air, the average may be taken at 0·040 parts. The mean temperature of the air within the sewer during this period was 50°·8, ranging between 48° as a minimum and 56° as a maximum.

The mean amount of oxygen in the air of the sewer was for the same interval, from an average of six experiments, 20·71 per cent., the proportion in the open air being 20·96.

After the introduction of the charcoal, the quantity of carbonic acid was found to have risen to 0·132 parts per 100 of air, the mean temperature in the sewer having risen to 56°·2, with a minimum of 52°, and a maximum of 61°·5.

The average amount of oxygen was 20·79. No sulphuretted hydrogen was present.

As a general rule it was found that the quantity of carbonic acid in the air within the sewer increased in proportion as the temperature rose in the sewer, and it declined again as the temperature fell.

No connection between the fluctuation in the amount of carbonic acid and the variation of the barometer could be traced.

On the whole, the air of this sewer was not seriously altered by the obstruction occasioned by the charcoal.

But though this sewer in the Avenue Road offered great mechanical facilities for carrying out experiments of this nature, there were circumstances which detracted from the value of the conclusions to be drawn from the results obtained there. The flow of water was rapid (four feet per second), the sewer itself a clean one, and the openings for ventilation numerous; so that, though it was quite certain the use of charcoal in such a case would occasion no difficulty, it did not indicate whether, in a fouler sewer with fewer air outlets, it would still be proper to use the charcoal ventilators.

It was therefore arranged in July that a second sewer should be placed under experiment; and for this purpose the Great Smith Street sewer was judged by Mr. Lovick to be particularly suitable.

The register thermometers were suspended in this sewer in the Brompton Road on the 3rd of October, and samples of the air in its ordinary condition were taken on the 4th and 5th of the month. On the 6th of October charcoal was placed in all the boxes in the ventilating openings, and samples of the air of the sewer collected as before, for analysis.

An average of six samples of air before the charcoal was introduced showed that the air in its normal state contained the large proportion of 0·307 per cent. of carbonic acid, with a temperature ranging in the sewer between 57° and 60°, with a mean of 58°·2.

During the months of October and November, twenty-four samples of the air were examined after the introduction of the charcoal, giving a mean of 0·251 per cent. of carbonic acid, with a mean temperature of
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OF ELEMENTS.*

BY HENRY E. ROSCOE, B.A., F.R.S.

THE metal vanadium (so-called from Vanadis, a cognomen of the Scandinavian goddess Freia) was discovered in 1830 by Sefström in the celebrated Swedish bar-iron made from the Taberg ore. From this source, even when using many pounds of the iron, Sefström obtained only minute quantities of the new substance, but he found it in somewhat larger amount in the slag or cinder produced in the reduction of the iron ore. Sefström ascertained some of the most peculiar characters of the substance, proved it to be a new element, and prepared some of its compounds in the pure state. The reactions by which vanadium can be separated and distinguished from all the other elements are: (1) The formation of a soluble sodium vanadate when the vanadium compounds are fused with sodium carbonate; (2) the formation of an insoluble ammonium vanadate when sal-ammoniac is added to the solution of a soluble vanadate; (3) the production of a splendid blue solution when this ammonium salt, dissolved in hydrochloric acid, is warmed with reducing agents such as oxalic acid.

Sefström not having leisure to prosecute the full examination of the properties of the new metal, handed over his preparations to Berzelius; and it is to the investigations of the great Swede (1831) that we owe almost all our acquaintance with the chemistry of vanadium.

Since Berzelius's time vanadium has been discovered in many minerals, of which a lead ore containing lead vanadate, and called by the mineralogists vanadinite, is the most important. It has also been found in many iron ores, in clay, bricks, and even in caustic soda. Still the quantity of the substance found in all these various sources has been extremely small; so much so, that the vanadium compounds must be reckoned amongst the chemical rarities, and we find them quoted in the price list of dealers in chemicals at 1s. 6*d*. per grain, or £35 per ounce! It is clear that our knowledge of the chemical properties of a substance so rare must necessarily be but incomplete, as the difficulties of obtaining exact or satisfactory results with small quantities of material are evident; and, in fact, the statements of the only persons who have worked upon the subject recently (Schafarik Czudnowicz), instead of giving us any more reliable information respecting the character of vanadium, have only served to throw doubt upon some of the conclusions of Berzelius, and thus to render our knowledge even less complete than it appeared to be.

Hence it was with much satisfaction that, in February, 1865, the speaker came into possession of a plentiful source of vanadium in a by-product obtained in the preparation of cobalt from the copper-bearing beds of the lower Keuper-sandstone of the Trias at Alderley Edge, in Cheshire. The manager of the works was puzzled to know why a blue solution, supposed by him to contain copper, did not deposit the red metal upon a strip of zinc; the speaker recognised this reaction as due to the presence of vanadium, and secured the whole of the by-product, which he found to contain about 2 per cent of the rare metal. The exact position of the vanadium mineral in the sandstone beds cannot

and cannot be entered. The general characters of the deposit are, however, well known, and exhibit points of great interest; they have been well described by Mr. Hull as follows:—

"The 'edge' or escarpment of Alderley rises from the eastern side of the plain of Cheshire gradually towards the east, but with a steep and abrupt ridge towards the north. This northern bank is richly wooded, and has a very beautiful aspect when viewed from a distance, as it contrasts strongly with the almost level plain which sweeps away to the northward and westward from its base. The ridge has here been upheaved along the line of a large fault, bearing east and west, throwing down at its base the Red Marl; and on the other side bringing up the soft sandstone of the Bunter, capped by a mural cliff of Lower Keuper conglomerate, which often breaks out in conspicuous masses through the foliage. The beds rise from the plain towards the east at an angle of about from 5° to 10°, and the escarpment is continued southward for some distance facing the east."

SUCCESSION OF BEDS IN DESCENDING ORDER.—(*Hull.*)

Red Marl....................		Red & grey laminated marls.
Waterstones...... Freestones........ Copper-bearing sandstone...... Conglomerate	Lower Keuper sandstone, 500 feet.	Brownish flaggy sandstones and marls. White and brown freestone. Soft white, yellow, and variegated sandstone. Hard quartzose conglomerate, underlain by bands of Marl, forming the base of the Keuper sandstone.
Upper red and mottled sandstone..........	Bunter.	Soft fine-grained yellow and red sandstone, being the uppermost member of the Bunter sandstone.

The beds in the above series which claim the greatest share of our attention are those at the base of the Keuper series, for in these occur the copper and other minerals. The copper, as both blue and green carbonate, occurs disseminated throughout the sand, the ore coating the outside of the grains of sand and the pebbles of quartz. In addition to copper, bands containing lead both as carbonate and sulphide (galena) occur, also bands and veins of cobalt ochre, oxide of manganese, and iron ochre in workable quantity. The copper is extracted from the ore by solution in hydrochloric acid and precipitation as metal by scrap iron. The ordinary copper liquor, as well as the oxide of iron precipitated by lime from the solution of the chloride, does not contain any trace of vanadium, nor was the speaker able to detect any of this metal in the ore as at present worked.

Following, in the main, the process of preparation adopted by Sefström, the speaker obtained from the above-mentioned lime precipitate several pounds of pure ammonium vanadate, from which all the other compounds of vanadium can be prepared.

What now were the conclusions to which Berzelius

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* Read before the Royal Institution of Great Britain, Friday, February 14th, 1868.

arrived from his experiments concerning the constitution of the vanadium compounds? He corroborated Sefström's statement, that the most characteristic feature of the substance is the existence of an acid-forming oxide, termed vanadic acid, produced whenever any of the oxides are heated in the air. Berzelius also discovered two other oxides of vanadium, of which he ascertained the composition; and likewise a volatile chloride. To the highest oxide he gave the formula VO_3, to the second VO_2, and to the lowest (or suboxide) VO; whilst the chloride was represented by VCl_3. The atomic weight of the metal he ascertained to be V = 68·5. Berzelius came to this conclusion from the following experimentally ascertained facts: (1) That on passing hydrogen over heated vanadic acid a constant loss of weight occurred, and the suboxide was formed; (2) that when dry chlorine is passed over the suboxide thus prepared the volatile chloride was formed, and a residue of vanadic acid remained, which was exactly equal in weight to one-third of the acid originally taken for reduction. Hence assuming that the lowest oxide contains one atom of oxygen (an assumption borne out by the analysis of the chloride), the acid must contain three atoms of oxygen,* and the following formulæ represent the composition of these compounds according to Berzelius:—

$$VO \quad VO_2 \quad VO_3 \quad VCl_3 \quad (V = 68{\cdot}5).$$

The interest attaching to the conclusions which Berzelius fairly drew from his experiments was much heightened by an observation made by Rammelsberg in 1856, as to the exact crystalline form of the mineral vanadinite, a double salt of lead vanadate and lead chloride.

So long ago as 1780 Werner had observed the identity of crystalline form of two minerals, viz., apatite, a phosphato-fluoride of calcium, and pyromorphite, a phosphato-chloride of lead; to which may be added, mimetesite, an arsenato-chloride of lead. These minerals all have an analogous composition, being represented by the formulæ:—

Apatite..........$3\,(Ca_3P_2O_8)+CaFl_2$
Pyromorphite.....$3\,(Pb_3P_2O_8)+PbCl_2$
† Mimetesite.....$3\,(Pb_3As_2\,O_8)+PbCl_2$.

They are truly isomorphous, crystallising in hexagonal prisms, terminated with hexagonal pyramids, having the same angles and the same length of axes. Rammelsberg added to this list the mineral vanadinite, which he ascertained by measurement to be strictly isomorphous with the foregoing, and to be as follows. The angle P on P was in

1. Vanadinite......................142°30′
2. Apatite.........................142°20′
3. Pyromorphite....................142°15′
4. Mimetesite......................142°7′

and the relation of the length of the axis:—

1. 1 : 0·727	3. 1 : 0·736.
2. 1 : 0·732	4. 1 : 0·739.

So far, indeed, has the identity of crystalline form been traced, that crystals have been found which at one end consisted of
pyromorphite (Heddle
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	Dioxide.	Trioxide
V = 51·3	V_2O_2	V_2O_3

Each of the four oxide
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mixed with hydrogen c

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* Berzelius concludes that the acid does not contain two atoms of metal, inasmuch as no alum could be formed with potassium sulphate corresponding to those formed by well-known sesquioxides.

† This group of minerals may be considered as calcium triphosphofluorhydrine, etc., thus:—

$$3\left.\begin{matrix}(PO''')\\ Ca_5\\ F\end{matrix}\right\}O_9 \qquad \left.\begin{matrix}3PO'''\\ Ca_5\\ Cl\end{matrix}\right\}O_9$$

Apatite. Pyromorphite. (*Wurtz.*)

* Or lead trivanadochlorhydr

† In his paper on Vanadium,
1867), the author ventured to
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oxide is obtained by the reduction of vanadic acid in a current of hydrogen, and the tetroxide is formed by the slow oxidation of the trioxide.

The lowest or dioxide of vanadium (V_2O_2) is obtained in solution by the reducing action of nascent hydrogen evolved from zinc, cadmium, or sodium amalgam upon the sulphuric acid solution of vanadic acid, which, passing through all stages of blue and green colour, ultimately assumes a permanent lavender tint. This solution of V_2O_2 in sulphuric acid acts as a most powerful reducing agent, bleaching indigo solution and other vegetable colouring matters as rapidly as chlorine; it also absorbs oxygen with avidity from the air, forming a deep brown solution. The other oxides of vanadium may be obtained in solution by the action of various reducing agents on the sulphuric solution of vanadic acid. Thus, by the action of nascent hydrogen evolved from magnesium a permanent *green* tint is obtained, and the vanadium is contained in solution as the trioxide, V_2O_3; whilst if moderate reducing agents, such as sulphurous acid, sulphuretted hydrogen, or oxalic acid are employed, the colour of the liquid does not pass beyond the *blue* stage, and the vanadium is contained in solution as tetroxide, V_2O_4.[*] The different colours of solutions containing these oxides were exhibited by means of the magnesium light.

The fact that the lemon-coloured chloride (the terchloride of Berzelius) contains oxygen was clearly demonstrated during the discourse by passing the vapour from a few grammes of the substance, together with perfectly pure hydrogen gas, over red-hot carbon. A portion of the oxygen of the oxychloride unites with the carbon to form carbonic acid, and the presence of this gas was shown by the precipitation of barium carbonate in clear baryta water contained in two test-tubes placed one before the other. At the commencement of the experiment, the carbonic acid was entirely absorbed by the small quantity of baryta water contained in the first test-tube; but after some time the hydrochloric acid gas simultaneously produced by the decomposition of the chloride saturated this liquid, expelling the carbonic acid gas, which being carried forward into the second test-tube, threw down a bulky precipitate of barium carbonate, thus showing that the turbidity cannot possibly be due to the presence of any vanadium compound. It was found quite unnecessary to place a tube containing heated copper oxide after the red-hot carbon, for the purpose of oxidising any carbonic oxide gas which might be formed, inasmuch as carbonic acid was always left in sufficient quantity to give a considerable precipitate. No method has been found for separating the whole of the oxygen from the oxychloride, and hence it has been impossible to make the above experiment quantitatively. Solid oxychlorides are obtained by the action of hydrogen upon the oxytrichloride, one of which resembles mosaic gold, possessing a bright metallic bronze-like lustre, and having been taken for the metal by Schafarik.

The atomic weight of vanadium was determined (1) by reducing the pentoxide to trioxide in a current of hydrogen. (2) By the analysis of the oxytrichloride. The atomic weight obtained as the mean of a large number of well-agreeing experiments is 51·3.

The metal itself has not yet been obtained, but a compound of vanadium and nitrogen has been prepared, shown by direct analysis to contain 14 parts by weight of nitrogen to 51·3 parts by weight of vanadium, corresponding to the formula VN. The existence of this compound is proof positive of the true atomic weight of the metal, and the nitride serves as the point of departure from which to seek for the metal and the true chlorides of vanadium, one of which, VCl_3, has already been prepared by the action of chlorine upon the nitride. It is a dark brown liquid, which decomposes when thrown into water, forming a green solution containing V_2O_3. The speaker demonstrated the fact that the oxychloride, $VOCl_3$, when thrown into water decomposes with formation of a *yellow* solution of vanadium pentoxide, V_2O_5, whilst the trichloride, VCl_3, on being similarly treated yields a *green* solution containing the metal in solution as trioxide, V_2O_3. He then compared these reactions with the decomposition of the corresponding phosphorus compounds, $POCl_3$ and PCl_3, forming P_2O_5 and P_2O_3, and rendered these reactions visible by obtaining a precipitate of yellow silver phosphate in the first case, and of black metallic silver in the second.

The characters of the vanadates themselves bear out the analogy of the highest oxide with the corresponding oxides of phosphorus and arsenic. In the first place, all the naturally occurring vanadates are tribasic; secondly, the true character of vanadic acid is shown to be tribasic, by the fact that, when the pentoxide is fused with sodium carbonate, three atoms of CO_2 are liberated, and the normal or orthovanadate, $Na_6V_2O_8$ (corresponding to $Na_6P_2O_8$), is formed; thirdly, the so-called monovanadates are monobasic salts, corresponding to the monobasic phosphates, and may be termed metavanadates, thus, $Na\,V\,O_3$ and $Ba\,2\,V\,O_3$, whilst the so-called bi-vanadates are anhydro-salts.

All the reactions by which Berzelius explained the facts he discovered, can equally well be represented according to the new atomic weight and constitution; thus:—

BERZELIUS' FORMULÆ.

$$V = 68{\cdot}5 \quad O = 8$$

$$1)\ VO_3 + H_2 = VO + H_2O_2$$

$$2)\ 3\,(VO) + Cl_6 = VO_3 + 2\,(VCl_3)$$

NEW FORMULÆ.

$$V = 51{\cdot}3 \quad O = 16$$

$$V_2O_5 + 2H_2 = V_2O_3 + 2\,(H_2O)$$

$$3\,(V_2O_3) + 6\,Cl_2 = V_2O_5 + 4\,(VOCl_3)$$

The speaker stated that the foregoing facts clearly pointed out that vanadium, hitherto standing in no definite relation to other elements, must be regarded as a member of the well-known trivalent or triad class of elementary substances, comprising nitrogen, phosphorus, boron, arsenic, antimony, and bismuth.

It is true that we are still but imperfectly acquainted with many of the characters of vanadium, but the more its nature is studied, the more points of family resemblance will be discovered, and the more close will the ties be found, which bind it to the great triad family.

The following tabular statement of the compounds of the most important members of this group clearly shows their common relations:—

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[*] In his communication to the Royal Society (Bakerian Lecture, Proc. Royal Soc., xvi. 220), the author gave the empirical formula VO and VO_2 to the 1st and 3rd oxides of vanadium, as the molecular weights of these oxides have not been determined, and it is uncertain whether they obey the law of even atomicities, or, like the only corresponding compounds, the nitrogen oxides, are exceptions to this law. On consideration, the author has, however, thought it best to adopt the doubled formula as urged by Sir Benjamin Brodie on the occasion above referred to.

TRIVALENT GROUP OF ELEMENTS.

	Nitrogen.	Phosphorus.	Vanadium.	Arsenic.	Antimony.
	N = 14	P = 31	V = 51·3	As = 75	Sb = 122
Trihydrides...	NH_3	PH_3	—	AsH_3	SbH_3
Trichlorides ..	NCl_3 (?)	PCl_3	VCl_3	$AsCl_3$	$SbCl_3$
Pentachlorides	—	PCl_5	—	—	$SbCl_5$
Oxychlorides	—	$POCl_3$	$VOCl_3$	—	—
Monoxides....	N_2O	—	—	—	—
Dioxides......	N_2O_2	—	V_2O_2	—	—
Trioxides.....	N_2O_3	P_2O_3	V_2O_3	As_2O_3	Sb_2O_3
Tetroxides....	N_2O_4	—	V_2O_4	—	Sb_2O_4
Pentoxides....	N_2O_5	P_2O_5	V_2O_5	As_2O_5	Sb_2O_5

In conclusion, the speaker remarked that vanadium was the fourth substance, supposed by its discoverer to be a metal, which had in recent years been shown to be a compound body.

Titanium.	Uranium.	Niobium.
Wollaston, 1823.	Klaproth, 1789.	Hatchett, 1801.
Wöhler, 1849.	Peligot, 1849.	Rose, 1842-64.
		Marignac, 1865.

Vanadium.
Sefström and Berzelius, 1831.

CONTRIBUTIONS TO OUR KNOWLEDGE OF THALLIUM.*

BY PROF. DR. J. W. GUNNING.

ONE of my former pupils, M. Serrurier, now managing director of the Amsterdam soda manufactory, had the kindness to make me a present of the flue dust obtained at the works where the pyrites used for making sulphuric acid is derived from the neighbourhood of Suhrort. I found this flue dust to yield about 1 per cent. of chloride of thallium; the bulk of the dust is made up of arsenious and arsenic acids, some iron and lead, but hardly any sulphuric acid. It is usual in order to obtain thallium from this dust to boil it (the dust) with dilute sulphuric acid, to strain, and to precipitate the thallium by means of hydrochloric acid; the chloride of thallium so obtained is washed, and afterwards dissolved in strong sulphuric acid, yielding the well crystallising sulphate of thallium. Another plan is to digest the flue dust with a solution of carbonate of soda, and to precipitate the thallium by means of hydro-sulphuret of ammonium. It has struck me while engaged with this matter that neither of these methods answer the purpose well; the sulphate and carbonate of thallium are not very readily soluble, and unless therefore one is prepared to lose a portion of thallium, there is no end of boiling the flue dust with solvents. One must, moreover, bear in mind that the flue dust contains a portion of the thallium as peroxide, insoluble in soda, and indifferently soluble only in dilute sulphuric acid. The presence of Tl_2O_3 in flue dust is proved in this way: after long treatment with soda solution there is a brownish muddy mass left, which, when acted upon by sulphurous acid dissolved in water, becomes partly discoloured and yields a large proportion of sulphate of thallium.

I have applied phosphoric acid to extract thallium from the flue dust, and I find it answer admirably well. The phosphates of thallium, and especially so the acid phosphate, are among the most soluble of the salts of thallium. Since phosphoric acid itself is rather too expensive to be thus applied, I have substituted therefor a mixture of bone ash and sulphuric acid, which answered the purpose splendidly; it is only required
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* Translated by A. Adriani, M.D., Ph.D., etc.

has been asserted by some chemists that there exists a reddish, or brown-reddish coloured higher sulphuret of thallium insoluble in dilute acids, but a rather instable compound. The substance just alluded to is no doubt that sulphuret, of the existence of which one may easily satisfy one's self by treating dry sulphate of thallium with *aqua regia*, or by boiling the sulphate of thallium with sulphuric acid and peroxide of lead, or of manganese; the solution of the thus partly-oxidised sulphate yields, on being submitted to a current of H_2S, in the first instance, a brownish-red precipitate, which, however, gets soon after dissolved, while sulphur is deposited; H_2S reduces also the thallic salts to thallous salts. I have not succeeded in obtaining this sulphuret under conditions of greater stability.

Böttger, *Ann. der Chem. u. Pharm.*, cxxviii., p. 249, speaks of this sulphuret, which, according to his researches, should be a higher sulphuret of thallium mixed with sulphide of arsenic and free sulphur; he also mentions that the said compound may be obtained in pure state by treating an acid thallic salt with a rather small quantity of hyposulphite of soda. I consider this last assertion to be highly improbable, since even sulphurous acid so readily reduces thallic salts to thallous salts. On repeating the experiment with a solution of the chloride or sulphate of thallium, and addition of some hyposulphite of soda, I have seen nothing but a separation of sulphur. I cannot, however, consent to agree to the opinion that the sulphuret in question should just be a higher sulphuret, for the following reason:—I found that the solution of the crude chloride of thallium in sulphuric acid, after having been treated with a current of sulphurous acid in excess, and afterwards with H_2S, just again yields the same reddish-brown precipitate. The following reactions justify the opinion that the brownish-red precipitate is a peculiar sulphide of thallium; strong bases readily convert it into black sulphide of thallium TlS_2, while sulphuret of arsenic becomes dissolved in the alkaline lye, and is re-precipitated therefrom on addition of an acid, as yellow As_2S_3, without simultaneous precipitation of sulphur, and also without disengagement of H_2S, which latter occurrence could not have failed to take place if one had had to deal in this instance with a higher degree of sulphuration than As_2S_3, or Tl_2S; its behaviour on being heated, whereby a sublimate is obtained partly of As_2S_3, partly of As_2O_3, while at the same time black sulphuret of thallium remains as a molten mass; it is easy to obtain at will the very self-same precipitate from every thallous solution by simply adding to it, first, some arsenious acid, then to pass through a current of H_2S, while it does not in the least matter whether the fluid is acid or has been rendered alkaline by ammonia; the said precipitate also occurs when an ammoniacal solution of As_2S_3 is precipitated with an ammoniacal solution of a thallous salt; from these results I draw the inference that I had simply to deal in this case with a mixture of As_2S_3 and Tl_2S. Analysis has proved this to be quite correct; the substance does not contain anything else than arsenic, thallium, and sulphur, while the latter is present in the proportion required to form Tl_2S and As_2S_3. My assistant, Mr. Adriaanz, performed the analysis in different ways: 1st. The substance weighed in a small flask was treated with a solution of pure caustic potassa, quite free from any sulphuric acid [sulphate], chlorine gas was passed through, and after a while the matter was entirely dissolved; from this solution pure carbonate of potassa free from sulphate, precipitated Tl_2O_3, this, after having been well washed upon a filter was dissolved in sulphurous acid, next evaporated to dryness, again re-dissolved and precipitated with iodide of potassium as insoluble iodide of thallium, then dried and weighed. In the filtrate from Tl_2O_3 the sulphuric acid was determined in the usual way, and the arsenic As_2O_5 as ammonio-magnesian arseneate. This mode of proceeding always gave the arsenic too low. 2nd. The same method was applied, with this difference: that in order to prevent loss of arsenic, iodine was used instead of chlorine to effect the oxidation; the excess of iodine was eliminated by evaporation along with alcohol; by this process, however, it was found that a portion of the sulphide of thallium was not properly oxidised, in consequence whereof the result for S was found too low. 3rd. The substance was oxidised with pure nitric acid, or pure *aqua regia;* the sulphur which partly escaped oxidation, was weighed in that state, after having eliminated the excess of acid by evaporation; the thallic salt was, after addition of some potassa, precipitated by means of iodide of potassium; the iodine which hereby separated was eliminated by evaporation along with alcohol, the iodide of thallium separated by filtration, and As and S estimated as just described. While executing these operations, my assistant experienced great difficulty in obtaining a complete reduction of the thallic salt, in consequence whereof the iodide of thallium did not present its usually bright, splendid yellow colour, but was always somewhat blackish; it was, moreover, very difficult to remove from the filter, whereupon the sulphur had been collected, some thallic salt tenaciously adhering to both sulphur and filter. As a sequel of these difficulties, the estimation of the different compounds did not take place from one and the same weighed quantity, but care was taken to estimate all compounds in samples obtained by one and the same preparation. After it had been duly ascertained that on being heated above 100°C., the substance did not lose any more water, the samples submitted to analysis were dried at 100°C. Dried at that temperature the substance is found to be a readily mobile very light powder which, on being rubbed, becomes electrified. The following are the results obtained on analysis:—Tl = 204.

	No. 1.	No. 2.	No. 3.	No. 4.	No. 5.	No. 6.
Sulphur...	18·82	19·56	21·27	26·75	18·14	24·66
Arsenic...	21·40	21·56	20·08	—	—	26·00
Thallium..	60·57	57·07	58·65	41·52	67·33	46·27
	100·79	98·19	100·00			96·93

Nos. 1–3 have been prepared by passing a current of H_2S through a solution of arsenious acid and sulphate of thallium in excess, acidified with sulphuric acid. The wash water ought to be mixed with an aqueous solution of H_2S, since otherwise the filtrate becomes somewhat colored.

No. 4, prepared by mixing an ammoniacal solution of As_2S_3 with excess of an ammoniacal solution of sulphate of thallium, and afterwards addition of sulphuric acid until acid reaction ensues.

No. 5, prepared by adding to an excess of an ammoniacal solution of As_2S_3 an ammoniacal solution of sulphate of thallium.

No. 6, prepared as No. 5, but in reversed order, *i.e.*, thallium in excess. I am quite aware that these analyses exhibit some imperfections due especially to the difficulty quite correctly to estimate As, but more so yet Tl; this, however, is demonstrated that from acid solutions containing thallium in excess, is obtained a combination made up of equivalent proportions of sulphide of arsenic and sulphide of thallium. The calculated result of this compound As_2S_3, $Tl_2 S$, is the following:

Sulphur	18·7
Arsenic	21·9
Thallium	59·4

Ammoniacal solutions do not yield a compound of constant composition; even while being prepared the washings run off coloured, while the precipitate itself is evidently decomposed by ammonia. When a current of H_2S is passed through a solution containing equivalent proportions of As_2O_3 and Tl_2So_4, only a small proportion of thallium is thrown down along with an excess of As_2S_3. If the quantity of As_2O_3 is increased above that of Tl_2SO_4, yet thallium remains in the solution, even a large excess of arsenic is incapable of assisting in throwing down the whole of the thallium: this, notwithstanding all these different precipitates, exhibits the same bright reddish-brown colour. In consequence of this somewhat singular behaviour, I, for a moment, suspected that some peculiar variety of the thallium salt might perhaps exist. I soon found, however, that if a solution of a salt of thallium is treated with As_2O_3 and H_2S, and this operation is repeated with the same solution of thallium, a second time a loss of metal is experienced, and that by repeating the same operation with the same sample over and over again, at last all the thallium is eliminated. I do not think it is possible to assign to this red substance a place among the well defined compounds. I do not know of another instance of similar composition and origin. It appears to me that the most probable explanation may be the following: To assume that beside the black sulphuret of thallium there exists another sulphuret of the same composition, but of different colour (akin to what is well known to be the case for mercury and antimony), and that this modification of the sulphuret of thallium is perhaps crystalline, and hence more apt to resist the action of acids, and that, furthermore, this compound formed as has been explained, can unite with sulphuret of arsenic, and form a molecular compound therewith. But even this explanation leaves unanswered the question which can be asked, How is it that the sulphuret of arsenic, the presence of which, and aptitude to combine with this red sulphide of thallium, is the cause of the origin of the latter, does not act in proportion of its quantity present in a given solution, but only transforms a comparatively small proportion of the thallium present into that compound?

The chemical history of this red substance teaches us that thallium cannot be separated from arsenic by means of H_2S, a point of some importance in quantitative analysis, as well as while operating on the flue dust from sulphuric acid works. It also deserves notice that the orange-yellow colour often exhibited by sulphur obtained from pyrites, and which is usually accounted for by ascribing that colour to the presence of selenium, may be equally well due to such sulphur containing thallium I beg leave to state in connection herewith, that Mr. Wm. Crookes, the discoverer of thallium, found in crude sulphur obtained from Spanish pyrites 0·29 per cent. of thallium. Mr. Crookes, however, did not record what colour this sulphur exhibited.*

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* The colour varies between orange, red, and dark grey, according to the quantity of thallium present.—W. C.

NATURE AND EX
GANIC MATTER

BY CHARLES R. C

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* Read in the Physiological
the British Medical Associatio

the gases dissolved in the water, are all points connected with the state of the organic matters, but call for no particular remarks upon my part.

Another very important point in connection with water intended for drinking purposes, is the colour. Every kind of water, whether from a spring, river, or reservoir, possesses a certain tinge, however faint that tinge may be. An instrument, which I will call the chromiometer, is particularly suited to this purpose. In a paper, which I wrote some years ago, "On the Urinary Pigment" (CHEMICAL NEWS, March, 1862, *Eng. Ed.*), I described this instrument, and suggested its use for the determination of the relative amount of pigment voided. A similar instrument is now used by Dr. Letheby for the examination of water. I shall describe my own arrangement of this instrument. To observe the colour, a pencil of light passing through a considerable body of the water is viewed by means of a tube. The apparatus consists of this tube, a yard in length, well closed at one end by a stopper. The stopper is preferable to a permanently fixed end, as it enables the operator to clean the tube perfectly—a matter of some importance. On the bottom is fixed a piece of white porcelain—a sight, if I may use the term. The tube is graduated into convenient divisions for the relative examination of different samples. By filling the tube with the water to be examined, and looking through the water at the white disc at the bottom, a faint colouration is at once perceived. Thus an experienced eye will, after a chromatic examination, prognosticate the character of his microscopic examination. The green chlorophyll tinge is almost invariably produced by the presence of desmidiaceæ and other algæ, or similar bodies. A white opacity is frequently indicative of fungoid growths. The finely suspended basic persalts of iron frequently found in waters rich in organic matter, which pass through iron-pipes, etc., are instantly recognised by a peculiar ochrey colour. True chalybeate springs also deposit a similar precipitate when exposed to atmospheric oxygen. Another important application of this tube is the determination of the state of oxidation of the iron-salts in the water, if present. This is a matter of some importance, particularly in connection with the organic matter; and I shall perhaps be excused if I dwell a little upon this point.

Iron is almost always present in well-waters, and frequently so in river or reservoir water. If present in very minute quantities, the primitive state of oxidation can only be recognised with the aid of the chromiometer.

As the microscope magnifies the form, so does the chromiometer magnify the colour so as to be recognisable by the eye. We are enabled to extend the range of our ordinary chromatic tests, and to recognise minute quantities of the proto- and per-salts of iron. Any attempt to concentrate the water for examination would alter the state of oxidation, and the results would be fallacious. Thus, on adding to separate portions of the water in the tube a few drops of the ordinary reagents, the following results are obtained. Sulphocyanide of ammonium developes a pink tinge, indicative of the per-salts of iron. A few drops of nitric acid are added to another specimen, and tested with sulphocyanide of ammonium. A darker shade is produced than in the first experiment. This is indicative of the presence both of per- and proto-salts of iron. Colouration, only produced after the addition of nitric acid and sulphocyanide, indicates that the iron is present as a proto-salt. When the iron bears a considerable proportion to the organic matter in the water, little or no organic matter, if we except ammonia, will be found in solution, providing the iron is in the state of a per-salt. If, however, the iron be present as a proto-salt, it will exert very little influence on the organic matter, and frequently such waters (except deep chalybeate springs) will be found to contain large quantities of soluble organic matter. Thus it is, that a water containing proto-salts of iron and organic matter will frequently, although quite clear on drawing from the well, become cloudy and deposit a mass of red flakes. They consist of a basic salt of iron, which retains the whole, or nearly the whole, of the organic matter. The trace left in solution is of an unfermentable nature.

The estimation of nitrites is one of importance. It is easily and readily performed in the following manner. I am induced to insert it here, as it differs from all the methods in use. It might at first sight appear strange that I should advocate the use of a volumetric solution of permanganate of potassium, when, in another part of my paper, I heartily condemn that reagent; but it will be also seen that there are two exceptions to the uncertainty of the action of permanganate of potassium; viz., oxalic acid and the nitrites. There are many operations in vogue for estimating nitrates; two of which the author proposed some years ago. ("On the Estimation of Nitrites in the presence of Nitrates," *Proceedings of the Pharmaceutical Conference*, Birmingham, 1865.) All of these methods require more or less care, combined with a considerable amount of manipulation. The following process is particularly applicable to potable waters as the nitrites therein are in a very diluted state. The method is based upon the reaction that nitrite of ammonium is on heating split up into nitrogen gas and water ($NO_2NH_4 = 2N + 2H_2O$). Eight ounces of the water to be examined are boiled, and, after cooling, are made up to the original bulk with pure water. The readily oxidisable matter in this measured portion is then estimated with volumetric solution of permanganate of potassium according to the directions given by Dr. Miller ("Observations on some Points in the Analysis of Potable Waters," by Professor Miller—*Journal of the Chemical Society*, vol. 3, new series, p. 119), *i.e.*, the volumetric solution is added a few degrees at a time, at short intervals, until a permanent pink tinge remains. We will suppose that the sample of water under examination used up 30^c of the permanganate solution. The thirty degrees were consumed in the oxidation of all the nitrites, *plus* some of the more readily oxidisable products. To a separate eight ounces of the water is added a few drops of a solution of pure sulphate of ammonium, and the whole is evaporated to dryness at a temperature below 100°C. The residue is again dissolved in 8 oz. of pure water, and estimated under exactly similar circumstances with the volumetric solution of permanganate of potassium. I will suppose that only 10° are now decomposed. These 10° represent readily oxidisable matter—the difference in the two estimations—*i.e.*, the 20° represent the nitrous acid present, which has been entirely destroyed and dissipated as nitrogen and water.

It must be self-evident to chemists, that to look for a specific test for miasma in water is absurd; that there are certain subtle substances of intense power which are physically unrecognisable—substances that, so far as we have gone, no balance can weigh, no microscope can see. Some time ago, there appeared an elaborate paper from M. Stas. M. Stas has made a great name for himself; he is deservedly looked upon as one of our best chemists, and he has made his name by the exquisite care with which he conducts his experiments. He found out what most chemists have found out—how very difficult it is to get pure water. Simple distillation will not do it. He therefore proposed to digest the water upon manganate of potassium—to destroy the organic matter. But I have here a water which has been treated as he directs; and I have even gone further with these experiments, for one of these specimens is now standing upon manganic acid itself—the most powerful oxidiser we have. One is water which had originally been distilled from a few grains of musk, and the other from fusel oil. It will be seen from the odours that one of these substances has escaped the oxidation entirely, and that the other has merely absorbed oxygen to be converted into another substance even more disagreeable than that originally contained therein, namely, valerianic acid. The late researches upon limited oxidation by Messrs. Wanklyn and Chapman show that the results of the action of oxidisers upon organic matter in solution are complicated, and entirely differ in their character from a perfect combustion. Therefore M. Stas's assumption, that nothing is present in water distilled from manganate of potassium, must be fallacious. This naturally brings me to the consideration of the utility of permanganate of potassium, which has been fashionable until lately for determining the amount of organic matter in water. It has been the opinion of most practical chemists who have had much experience with water, that that test was worthless as a measure of organic matter. A chemical friend of mine remarked to me, "It is the colour that does it. If it were not for its beautiful colour and the prettiness of the reaction, no one would use it." And I really think there is something in the remark.

Dr. Frankland was, I believe, the first to enter his protest (in type) against the use of this reagent as a measure of organic matter "Lectures on Water" (Royal Institution, 1867); although, as far back as 1866, I, in the presence of Dr. Mapother, practically illustrated its worthlessness at a public meeting in this city, when the experiments were entered upon the minutes of the meeting. I merely mention this fact in case the experiments which I give below might be considered as a babbling echo of what has, perhaps, been so much better put to the public; but I insert them here from two reasons. The first of these is, that many of the substances upon which I have experimented, although they are not given by Dr. Frankland, are yet typical of the organic matter likely to be met with in potable waters; secondly, that some curious points are illustrated by my experiments, which would not be foreseen by the scientific man.

A large number of experiments were instituted under exactly similar circumstances, for the purpose of observing what class of organic substances were readily oxidised, and how far their state of combination might affect the change. To effect this, the process recommended by Dr. Miller was fixed upon as the most suitable for watching the relative action. A volumetric solution of permanganate of potassium was used, which corresponded to ·22 oxalic acid. The different solutions were made by dissolving in each case ·2 of a *gramme* of the organic matter in (568 c.c.) an imperial pint of pure water. A degree of the permanganate solution was added at intervals of a quarter of an hour as long as any decolouration took place, half an hour being the crucial test
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No.	Substance.
1	Oxalic acid
2	Nitrate of potassium
3	Nitrate of ammonium
4	Chloride of ammonium
5	Ammonia
6	Trimethylamine
7	Nicotine
8	Conium
9	Aniline
10	Urea
11	Uric acid
12	Urinary pigment
13	Hydrocyanic acid
14	Strychnine
15	Sugar
16	Albumen (partially de composed)
17	Albumen (coagulated)
18	Oxalic and urea
19	Lactate of lime = *gramme* ·2 Lactic aci
20	Butyrate of lime = *gramme* ·2 Butyric aci

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ed as water supersaturated with oxygen, the extra equivalent of oxygen being held by the faintest phase of attachment coming within the term of chemical attraction; so that the least disturbing agency imaginable would decompose it into oxygen, ozone, and water. Peroxide of hydrogen oxidises organic matter, and stops fermentation: therefore, providing that we had a pure article, we could oxidise the organic matter, without introducing anything but pure water. But at present peroxide of hydrogen is very dear, and, as found in commerce, is much too impure to be used in drinking water. Another doubtful point is, that this curious substance sometimes seems to play the part of a reducing agent, as well as an oxidiser. If so, sulphuretted hydrogen might be generated by its action upon sulpho-compounds, such as albumen.

It had been lately stated that charcoal will not, after a little time, purify drinking water; and that, so far from taking from the water organic matter, it gives up again a certain amount. Thus: that water, on analysis before and after passing through charcoal which had been some time in use, was more contaminated with organic matter after having been passed through the said filtering medium. If it were not for the fact that the paper containing the above statement had received a considerable amount of commendation, and, *ergo*, publicity, I should have passed it by. But the author of that paper evidently misunderstood the position. The *modus operandi* by which charcoal acts on oxidisable organic substances is not so much by virtue of any attractive or selective power that it possesses; but, as a carrier of oxygen in a concentrated form, it is one of the most powerful oxidisers we possess. The oxygen condensed within the charcoal acting more energetically than the available oxygen, we can apply in the form of permanganate of potassium. The original experiments of Dr. Stenhouse in connection with the action of charcoal have received ample confirmation from the late investigation of Professor Calvert (*Journal of the Chemical Society*, June, 1867).

It is true that charcoal, after a certain time, becomes effete—its activity destroyed; but it is equally wonderful to observe the length of time a charcoal-filter with the following provisions will retain its vitality, if I may use the expression. 1. That the water passed through it does not contain a very large percentage of organic matter; 2. That it is drained from the water the better part of each day, so that the atmospheric oxygen (with as little of the dust as possible) may have access to it. Charcoal, under these circumstances, will be found to do its work well. You must give it its food quietly, so that it can digest it.

I can hardly believe the statement made, that charcoal will, after a certain period, transfer again organic matter to water. I have examined water before and after passing through a charcoal filter which had been in daily use in my own house over a month. There was not much difference, it is true; but the water which had passed through the filter had most decidedly the advantage. I can well imagine that, as the charcoal acts as a mechanical recipient to the insoluble organic matter, that substance may at last accumulate to such an extent as to enter itself into a state of fermentative change. The activity of the charcoal, being by this time exhausted, or at least only sufficient to supply a minimum of oxygen, would only assist such a decomposition. This state of the case would be simply the putrefaction of a mass of solid organic matter independent of the charcoal—not the rendering back from the charcoal of something it had absorbed from the water. I would suggest that a well-constructed water-filter should have an arrangement by which the insoluble organic matter should be separated before the water comes into contact with the filter. In such a filter, the organic matter could never accumulate.

In conclusion, I may point out that the most valuable method of examining water is that which I believe was first used by Dr. Frankland—that is to say, to examine the relative amount of oxygen and nitrogen found in water. In absorbing atmospheric air, the oxygen is dissolved with a little greater avidity, so that that gas is found in water in a larger relative proportion than in atmospheric air; viz., the relative proportion of oxygen to nitrogen is about 21 to 79; but as found in water it is about 32 to 68. Now, if the enclosed air is found to contain less oxygen, it shows that that element is being consumed by chemical changes going on within the water; or, in other words, such a water is not in its normal state, and therefore is unfit for general use.

The aqueducts of antiquity show how important the blessings of pure water were considered from time immemorial, and how necessary to the welfare of all communities. But I cannot see that we have improved upon our forefathers; for, whilst they spent enormous sums to produce stupendous supplies, we seem to me to spend our money in merely building immense reservoirs. Anything is, however, better than that "like a dog we should return to our own vomit."

> "Most blessed water, neither tongue can tell
> The blessedness thereof; no heart can think,
> Save only those to whom it has been given,
> To taste of that divinest gift of heaven."

A NEW ASPIRATOR.

BY J. LANDAUER.

Though a number of aspirators of different construction are proposed and used in chemical analysis, the following may be added to the number as being distinguished by great simplicity. This new aspirator is based on the principle of the siphon. A capacious flask is hermetically closed by a cork provided with two holes. One of the latter receives the siphon, and the other a glass tube for connecting the apparatus, through which the passing of a current of air is desired. After having made the connections and filled the flask with water, the latter is made to run out of the flask by sucking the outer leg of the siphon, the end of which must, of course, be lower than the level of the water. The current of air is thus effected.

The efflux of water is regulated by joining more or less width to the tubes between apparatus and aspirator. Two aspirators are connected with the apparatus, and used alternately, in order to enable a refilling of the flasks without interruption of the process. For this purpose an intermediate apparatus, consisting of a glass tube about two inches long, and one inch wide, is required, which is connected on one side, with the apparatus intended for receiving the current of air, and on the other side with the two aspirators. The latter connection may be effected by india-rubber tubes, each provided with Mohr's pinchcock. Such arrangement will enable the alternate working of the two aspirators. It is of course understood that all the connections must be effected hermetically. One of the advantages of this vessels.

ON THE FORMATION OF

A SERIES OF DOUBLE SULPHOCYANIDES OF CERTAIN OF THE ALKALOIDS

WITH THE METALS

ZINC, TIN, MERCURY, AND MOLYBDENUM.

BY WILLIAM SKEY,

ANALYST TO THE GEOLOGICAL SURVEY OF NEW ZEALAND.

WHILE engaged in testing some of the chemical properties of the alkaloids, in relation to those of the inorganic bases, a new set of reactions have just discovered themselves, which I will briefly note preparatory to a further communication thereon. These reactions consist in the formation of precipitates, when an acid solution of certain of the alkaloids is brought in contact with a solution of a salt of zinc, tin, mercury, or molybdenum, in presence of hydro-sulphocyanic acid.

To avoid the formation of single sulphocyanides, it is best to employ solutions of the metals and alkaloids of such strength that a sulphocyanide makes no precipitate in either solution separately.

The precipitate formed by nicotina, mercury, and sulphocyanogen, in presence of each other, by quina, sulphocyanogen, and zinc, or mercury, and by strychnia, zinc, and sulphocyanogen, was found to yield these several substances respectively; the remaining precipitates I have not yet had time to examine, but pending this and the quantitative analysis of some, I will for the present suppose all these precipitates to be compound sulphocyanides.

Generally these compound sulphocyanides are very insoluble in cold water, more soluble in hot water, and freely soluble in alcohol; they are but little affected by hydrochloric or sulphuric acids,—but are decomposed by alkalies,—while their physical properties are in some instances very characteristic.

The following are details respecting the more interesting of them:—

Sulphocyanide of Strychnia and Zinc separates as a gelatinous mass, but gradually assumes the form of long acicular crystals. Sulphocyanide of strychnia and mercury is crystalline (it is soluble in sulphocyanide of potassium).

Sulphocyanide of Quina and Zinc is solid and brittle at 70°, soft and plastic at 90°, and changing to a fluid, or semi-fluid substance at about 200°, which appears to crystallise on cooling.

Sulphocyanide of Nicotina and Zinc is crystalline, while the tin, mercury, and molybdenum salts are oils at common temperatures; they are nearly colourless, with the exception of that of molybdenum, which is of a rich purplish red colour.

The Sulphocyanide of Atropia and Tin is a semi-solid fat at 60°, while the zinc, mercury, and molybdena salts are oils; the last is of a dark red colour.

Sulphocyanide of Morphia and Zinc or Tin was obtained in amorphous forms; they fuse at a very slight elevation of temperature; the mercury compound is an oil.

Sulphocyanide of Narcotine and Mercury is crystalline, and easily fusible.

Sulphocyanide of Veratria and Zinc, Tin, Mercury,

Sulphocyanide of Conia and Mercury is a green crystalline substance.

The remaining natural alkaloids have not yet been tested, but sufficient is shown to make it probable that this property of forming double sulphocyanides with certain of the metals, is possessed by all.

With regard to the metals thus connected together by these reactions, I would observe, the only character common, and at the same time, to some extent, peculiar to all, is that of the ready fusibility and volatility of their chlorides. I may state that aniline does not give any precipitate when substituted for the alkaloids.

The feebly basic nitrogenous substances, gelatine and isinglass, behave like veratria.

For the detection, separation, and determination of certain of these alkaloids, it is possible these reactions may be advantageously employed.

ON THE USE OF METHYLATED SPIRIT IN PHARMACY.

THE following paper is a condensed translation of a report made to the Medical Council of the province of North Holland, by six of its members, among whom was Prof. Dr. J. Gunning. In accordance with the excellent laws which, since January, 1866, regulate in the Netherlands all matters relating to medicine in its more extended sense, there is in every province [county] of the kingdom a medical council composed of medical men, pharmaceutists, and lawyers. As regards the free use of methylated spirit by pharmaceutists, the committee just alluded to is of opinion that it would be manifestly unfair to pharmaceutical chemists who are in the habit of preparing, or manufacturing, for instance, such substances as quinine and other alkaloids, to require that such articles should, as far as such is required, be made by them with alcohol, whereas the wholesale maker would use, and quite justly so, methylated spirit. The committee, however, distinctly desire it to be understood that the use of methylated spirit cannot be allowed in the preparation of medicinal tinctures, for although it is true that the methyl-alcohol, as it is met with in ordinary wood-spirit, bears the greatest analogy to ethyl-alcohol, there occur beside in wood-spirit, acetate of methyl and aceton, both of which, in their solvent power, more resemble ether, and, consequently, influence and alter the real constituents of tinctures to be prepared with alcohol; the same, of course, applies to alcoholic extracts. The committee also disapproves of the manufacture of ether and chloroform for medicinal use from methylated spirit. Since the inspection of chemists' shops in the Netherlands, and the testing of the divers pharmaceutical preparations is a duty of the Medical Council's, it was necessary to find ready tests to ascertain whether or not methylated spirits have been unlawfully applied. The following are the results of some experiments instituted on purpose by the members of the above-named Committee. It is quite possible to recognise even in tinctures which contain strongly-scented substances, the wood-spirit, if methylated spirit was used in the preparation thereof; the smell is even detected three months after the tinctures have been made, but if a doubt arises, it is best to mix the tincture in question with double its bulk of boiling water. Tinctures containing free ammonia beside must be first rendered neutral. Another test is the following:—The alcholic fluid in question is mixed with twice its bulk of strong ammonia.

Next there is added, while the fluid under examination is well stirred up, a few drops of a solution of 10 grs. of iodine and 20 grs. of iodide of potassium, in half a fluid ounce of distilled water. In case the fluid under examination does *not* contain methylated spirit, there will soon be observed a finely-divided precipitate of a black substance (iodine?), giving to the fluid a dark bluish appearance; if, on the other hand, wood-spirit or methylated spirit is present, the fluid remains clear, assumes a brownish yellow, but rapidly again vanishing hue; after the fluid has become quite colourless again, there will distinctly be perceived, on smelling it, an odour of saffron, while shortly after, also very frequently, crystals of iodoform are deposited. This test and reaction are discovered by Mr. J. Polak. Tincture of iodine made with methylated spirit may be detected, since on addition of liquid ammonia it becomes readily and without application of heat, discoloured, the saffronaceous odour will be smelt, and crystals of iodoform deposited. The above-named test is not disturbed by the presence of essential oils, camphor, compound ethers, &c. It is best, however, that, as regards the application of this test to tinctures, the latter should be submitted to distillation, and the distillate tried by the reagent. From a series of interesting experiments instituted by the committee, in order to test in how far methylated spirits might change the constitution of alcoholic extracts made with methylated spirit instead of with pure alcohol, it appears that methylated spirit dissolves out from 2 to 7 per cent. more from vegetables than alcohol does, while in the case of extracts of cicuta and belladonna the amount was from 13 to 14 per cent. more if methylated spirit instead of pure alcohol was applied. The following are the results of experiments instituted with ether, æther muriaticus alcoholicus, æther aceticus, æther nitricus, alcoholicus, and chloroform made with pure alcohol and methylated spirit.

Ether from methylated spirit cannot be detected by the smell, but easily by the following test:—Pour carefully some strong sulphuric acid in a test tube, hold it then as slantingly as possible, and then pour in as carefully as possible some of the ether; if the latter is obtained from methylated spirit, it will be seen that at the place of contact of the two fluids, a dark brownish yellow colouration ensues, which, if the ether were obtained from pure alcohol, and submitted to a similar experiment, will be found absent, or at least hardly perceptible. Æther muriaticus, alcoholicus, and æther nitricus alcoholicus can at once be detected by the iodine test spoken of before if they have been prepared with methylated spirit.

Æther aceticus, prepared either with pure alcohol or with methylated spirit, is not recognised by the smell, but the iodine test detects the origin from methylated spirit at once, and it hence follows that acetic ether obtained from methylated spirit contains acetate of methyl and also aceton.

Chloroform, if prepared with methylated spirit, may be recognised by the smell, which is different from that of chloroform obtained from pure alcohol; beside this, the discolouration with sulphuric acid takes place with chloroform as with ether made from methylated spirit.

ON A

NEW GENERAL METHOD OF VOLUMETRIC ANALYSIS.

BY WOLCOTT GIBBS, M.D.,

RUMFORD PROFESSOR IN HARVARD UNIVERSITY.

In a memoir on the quantitative determination of nitric acid, H. Rose* suggested that in particular cases the metal in the nitrate might be precipitated by means of sulphydric acid, and the nitric acid set free determined in the filtrate by volumetric methods. So far as this application of the volumetric analysis is concerned, Rose's method appears not to have been carried out in practice or even supported by actual experiment. It occurred to me that the method might be generalised so as to form the basis of a new application of the processes of acidimetry, and the following analyses will serve to show the degree of accuracy which may be attained. When the salt to be analysed contains a fixed acid which does not act upon sulphydric acid gas, a weighed portion is to be dissolved in water, the solution brought to a boiling heat, and a current of sulphydric acid gas passed through until the metal is completely precipitated. When quantities of about 5 grammes are employed the precipitation is usually complete in half an hour. The precipitate may then be allowed to settle, and a drop of the supernatant liquid taken out with a glass rod, and tested upon a white porcelain plate, with a drop of a saturated solution of sulphydric acid in water, or with any other reagent which may be specially adapted to the metal in the salt examined. The precipitation being complete the liquid is filtered upon a ribbed filter, the filtrate and the washings allowed to flow into a half litre or litre measure, and the washing with hot water continued until a drop of the filtrate no longer exhibits an acid reaction. The liquid is then allowed to cool, and the volume made up to exactly a half litre or litre by the addition of water. After thoroughly mixing the contents of the measure, fifty or one hundred cubic centimeters are to be taken out, a few drops of a solution of cochineal or logwood added, and the free acid determined by means of one-tenth normal ammonia in the usual manner. The first determination is to be used simply as a guide. Two or more successive portions of the acid liquid may then be taken out and determined successively, and the mean of several determinations obtained. With very little practice the results will be found to correspond to one-tenth c.c., when a burette with Erdmann's swimmer is employed. From the quantity of ammonia required to neutralise the acid, the quantity of acid, and in many cases also of base, in the salt may be readily calculated.

With crystallised sulphate of copper the following results were obtained.

Grs. sulphate.		Per cent. acid.	Per cent. base.	
1·8435	gave	31·89	31·70	(Sharples)
3·7183	"	32·14	31·93	"
8·3955	"	32·10	31·89	(Tower)

The formula $\text{ЄuSΘ}_4 + 5\text{aq}$. requires ($\text{Єu} = 63{\cdot}50$).

		I.	II.	III.
ЄuΘ,	31·86	31·70	31·93	31·89
SΘ_3,	32·07	31·89	32·14	32·10

The first analysis was made with a commercial sulphate, the other, with a pure salt prepared from elec-

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* Pogg. Ann., B. cxvi., p. 125.

trotype copper. In crystallised sulphate of copper and potassium, ($\Theta uS\Theta_4 + K_2S\Theta_4 + 6aq.$) 2·6601 gr. gave 18·23 per cent. acid and 18·11 per cent. of oxide of copper. The formula requires

		Found.
$\Theta u\Theta$,	18·00	18·11
$S\Theta_3$,	18·12	18·23

In the memoir already referred to, Rose points out the necessity of diluting the solutions of metallic nitrates to such a degree that the nitric acid set free shall not act sensibly upon the sulphydric acid.

In the experiments made in my laboratory to test the method this precaution was not found to be sufficient. Thus with crystallised nitrate of lead, Mr. Sharples obtained the following results.

I. 2·147 grammes of salt were dissolved in 200 c.c. water, and the lead precipitated from the boiling solution by sulphydric acid gas. The filtrate was made up to 500 c.c., of which three portions containing each 75 c.c. were taken for titration, and each required 18·2 c.c. of ammonia. This gives 30·51 per cent. of nitric acid, while the formula $Pb(N\Theta_3)_2$ requires 32·61 per cent.

II. 2·4992 of the nitrate were dissolved in 500 c.c. of water and treated as above, only the lead was thrown down in the cold by sulphuric acid, and the excess of the latter expelled from the filtrate by boiling. The acid found corresponded to 32·12 per cent. in place of 32·61. From this it is clear that even dilute nitric acid acts too powerfully upon sulphydric acid to permit a very accurate determination of the former under the circumstances of the experiment. Precipitation from a boiling solution is necessary because the filtrate is then at once free from sulphydric acid.

To obviate the difficulty arising in the case of nitric acid it occurred to me to add to the solution of the nitrate a portion of a neutral salt containing a fixed organic acid, an equivalent quantity of which would be set free by the combination of the free nitric acid with the base contained in the salt. This method was found to give perfectly satisfactory results, as the following analyses by Mr. S. P. Sharples will show.

4·409 grms. of nitrate of lead were dissolved in 200 c.c. of water, five or six grammes of pure Rochelle salt added, and the lead precipitated as above. The quantity of acid found corresponded to 32·58 per cent. The formula $Pb(N\Theta_3)_2$ requires

		Found.
$N_2\Theta_5$	32·63	32·58
$Pb\Theta$	67·37	67·27

0·9380 grm. of nitrate of bismuth were treated as above, Rochelle salt being added. The nitric acid found, corresponded to 33·40 per cent. and the equivalent quantity of oxide of bismuth to 47·82 per cent. The formula $Bi(N\Theta_3)_3 + 10$ aq. requires

		Found.
$N_2\Theta_5$	33·47	33·40
$Bi_2\Theta_3$	47·94	47·82

5·6553 gr. of chloride of mercury were treated as above, 6 or 8 grm. of Rochelle salt being added to the solution. The free acid corresponded to

	Calc.	Found.
Cl_2	26·20	26·10
Hg	73·80	73·90

When chlorine is separated in the form of chlorhydric acid the volatilisation of the acid in the process of boiling is completely avoided by the addition of the organic salt. The same remark applies to nitric acid,

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* Bull. de la Socié

ON HEAT AND COLD; A COURSE OF SIX LECTURES (ADAPTED TO A JUVENILE AUDITORY), DELIVERED AT THE ROYAL INSTITUTION OF GREAT BRITAIN (CHRISTMAS, 1867–8.)

BY JOHN TYNDALL, ESQ., LL.D., F.R.S.

LECTURE VI.

(Concluded from Am. Repr., April, 1868, page 181.)

Reflection, refraction, and absorption of radiant heat.—The heat of the sun.—Visible and invisible rays.—Extraction of light from the rays of heat.

I HAVE had occasion to say to you once or twice in these lectures that no body in nature is absolutely cold. All bodies are more or less hot. Even ice itself is a hot body compared with solid carbonic acid. In fact, ice would be quite competent to make a mixture of solid carbonic acid and ether boil, it being hot in comparison. All bodies are warm, and all bodies are emitting rays of heat Here is a platinum wire in front of the table, such as we have already operated upon. At the present time that platinum wire is emitting rays of heat of a perfectly definite character. If I connect this wire with our battery you will observe our old experiment. You see the wire is heated to redness; it emits rays of heat, and also to some extent rays of light. Before the electric current passes the wire emits rays of heat which are incompetent to excite vision; but when I raise the temperature of the wire thus, by sending the electric current through it, what becomes of its old rays of heat which it emitted in this invisible state? They still maintain themselves, and they become much stronger, but they are still obscure. We mix, with the luminous rays of that wire, the obscure radiation that issued from it before the current made it incandescent. If I go on shortening the wire, as in an experiment we made in an early portion of these lectures, we find it gets brighter and brighter, but the rays it emitted before it became red hot at all are still mingled with the visible radiation. They exist, but they exist greatly intensified; so that the rays which issued from that wire before it became incandescent, are present, as well as the visible rays, but they are raised to a thousand times the intensity which they first possessed. They are still obscure, and have no power to excite vision, but they are, nevertheless, there with a thousand-fold their first intensity. Now I must try to separate before you these luminous rays from the obscure rays; and I must endeavour to operate upon the obscure rays so as to show you some effects that they can produce. I think you will understand the process by which this can be done. I have here a small concave mirror, and this I will place behind the electric lamp. We shall have an image of the carbon points of the lamp produced in that way, and I will throw that image upon the screen. We have now thrown upon the screen an image of the carbon points, whence issues the electric light. If I take another mirror, and converge the rays by it, I can give you a larger image, which, perhaps, will be better seen. Here is now a larger image of the carbon points produced in that way. The image is inverted. You see a considerable amount of light there, but Mr. Cottrell will now fill a vessel with an opaque liquid The liquid which we use to obtain the opaque solution is called bisulphide of carbon: it is perfectly transparent; and here is the substance called iodine—very well known to many people. This bisulphide of carbon dissolves the iodine with great freedom, and the consequence is the production of this dark liquid, which is so wonderfully opaque that it would cut off the light of the sun at noonday. Strange to say, it is the quality and property of this wonderful substance to entirely cut away the luminous or visible rays upon which depend the colours you saw on the screen, whereas it allows all the rays of heat to pass through. beam or cone of light tracking its way through the dust of the room towards the thermo-electric pile. Mr. Chapman will, when I tell him, place the cell containing this opaque liquid in front of the electric light That will cut off bodily all the light, but still the spot where the pile will be placed will remain very hot. [The cell and pile were then placed in position.] You see that all the light is cut away; but you observe that the needle at once marches away, thus proving that although the light is cut off, the heat rays are left behind.

I want now to try and make these heat rays more evident to you still, and for that purpose I have placed within this camera an electric lamp similar to what I have just used; and behind the electric lamp I have placed a silvered mirror. This mirror will reflect the rays of light from the electric lamp, and will cause them to issue through this window which you see in front. This window is formed of rock salt. Rock salt is exceedingly transparent to the rays of heat, and also to the rays of light; and it is for that reason that I use that substance. I now obtain a convergent beam from the electric lamp. You see a brilliant cone of rays. Mr. Cottrell will now place the opaque solution in front. There it is, cutting off all the light, so that you see nothing. But now I bring this piece of platinum opposite the dark liquid; and observe what occurs. The platinum is raised to a red heat, in perfectly dark air. If, instead of platinum, I take some dry paper, and hold it in the focus of the dark rays, you see I can ignite that paper. The paper is set on fire. This ignition is caused by the invisible rays of heat issuing from the electric lamp. I now take a thick piece of metal, and hold it in the dark rays of heat: you see it is melted by the radiant heat, and drops down in a liquid state. I will now burn a piece of zinc here. There, you see the zinc is actually set on fire in a place where there was perfect darkness. The air where this zinc is set on fire is perfectly unwarmed. Nothing would be easier than to ignite a cigar in this way in perfect darkness. For instance, here is one which I will ignite. You see it is instantly set alight in a place where there is absolutely no light. You might put your eye where that platinum was raised to red heat. I have cautiously approached my eye to that burning focus that you saw there, and allowed the rays bodily to enter the eye, and could neither see light nor feel heat. The retina was perfectly dead to those very powerful rays. Sometimes we obtain the combustion of magnesium by these rays. Here you see we have that beautiful metal set on fire in a place where there was no light whatever—a space of utter darkness. I might set London on fire by means of these dark rays I have here a glass jar containing oxygen gas, and into this jar I dip a piece of charcoal. I now bring the charcoal into the focus of the invisible rays of heat, and you see the charcoal is ignited by these dark rays, and burns brilliantly in this gas.

I want now to make one or two more experiments in connection with this subject. For this purpose I will take the same mirror which I have just used, and employ another camera which is at the end of the table. The mirror will be placed behind the light, and will reflect a beam of light along the table. Instead of allowing this beam to fall upon the audience and annoying you, I will catch it upon another mirror just as I caught the ray of light by the mirror near the ceiling in an experiment early in the lecture. I dare say many of you see the intense reflection here. There is a focus which would burn your fingers most fearfully if you put them there. I dare say we shall be able to inflame paper at that focus. There you see the paper instantly set in a blaze; and this blaze is produced not by the luminous rays, but by the dark ones. You might put a sensitive thermometer there, and have no result. It is only when the heat falls upon this paper that the heat is produced. We can burn zinc here as I did in the dark rays. You see the zinc is set on fire, and blazes up almost like a piece of paper. Here is a small vessel containing water, and I will place that in the focus of the rays. I now place another vessel of water in such a way that the light has to pass through it. This will intercept the dark rays which give the heat, though it does not sensibly interrupt the rays of light. At the present time the focus of rays falls upon the former vessel of water, without any effect whatever being produced upon it. I will now withdraw the vessel of water through which the beam passes before it reaches the mirror, and so allow the heat rays to pass, and you see the water in the vessel as the focus of the rays immediately begins to boil. After a time this water will be thrown into a state of violent ebullition. It is already boiling. This action is due not to the rays of light, but entirely to the dark invisible rays of heat of which I have been speaking.

I make these experiments for the purpose of bringing home to your minds the fact that we owe all our rivers, all our glaciers, and all our snow, entirely, or almost entirely, to these dark rays. The luminous or bright rays of the sun fall upon the tropical ocean, and pierce it to great depths: they are not absorbed; but the non-luminous rays —the heat rays of the sun—strike upon the tropical ocean, and they are absorbed very near its surface. It was by the absorption of the dark rays that the water was boiled in the last experiment. These dark rays of the sun which strike upon the tropical ocean, and are then absorbed, heat the surface of the ocean, and thus it is that all the moisture or evaporation is produced.

And, now, I am sorry to say, we have come to the end of our task. I told you in the beginning that I wished very much to transfer the task of giving these lectures to somebody else, as I was so occupied that I could not make them what I wished to make them, and still I am not sorry that I undertook them. I am glad that I have come here, for it has given me great pleasure to meet you from day to day. You have made up by your attention for my defects in lecturing; and I have only to add that I thank you for that attention, and wish you from my heart a happy new year.

FOREIGN SCIENCE.

PARIS, MARCH 3, 1868.

Estimation of Titanic and Niobic Acids.—Process for bleaching palm Oil by Chromic Acid.—Solubility of Ether in solutions of Sugar.—Alcoholate of Baryta.

THE processes proposed up to the present time for the estimation of titanic and niobic acids in mixtures have not succeeded very perfectly. M. Marignac, after testing the capabilities of the processes already known, has adopted the following. He takes 5 grammes of the metallic acid, and fuses it with 1·5 gramme of hydrofluorate of fluoride of potassium, first heating this salt gently until it is in a state of fusion (this fusion might almost be called aqueous), and then adding the niobic and titanic acids. By allowing the salt to fuse first, loss by decrepitation is avoided; afterwards a powerful heat is applied, and in a minute there is complete fusion, and the metallic acid is completely dissolved. It is not well to prolong the operation, as the mass has a tendency to creep up the sides of the crucible; for this reason a deep crucible should be chosen. The cooled mass is dissolved out by digesting with warm dilute hydrochloric acid, employing for this purpose 20 c. c. of pure concentrated hydrochloric acid and 230 c. c. of water, and the solution collected in a flask. When it is quite cold, a bar of distilled zinc is introduced, sufficiently long to reach the bottom of the flask, and then to protrude at the neck. The flask is closed by a cork carrying a bent glass tube, by which the hydrogen is disengaged under water; the temperature of the solution must never be allowed to rise sensibly. At the end of twenty-four hours the reduction is terminated, and it is only necessary to remove the cork, and withdraw the bar of zinc, and then to proceed immediately to determine the

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tion to the Société d'Encouragement, by M. Schützenberger. This communication has led the way to results of great interest, whether taken in a scientific aspect, or a pecuniary one. The existence of five pigments in this colouring matter, he had remarked, may be recognised—they are alizarine, purpurine, orange madder, pseudo-purpurine, and xanthopurpurine. These bodies are distinguished by their composition, their physical and chemical properties, and especially by their comportment in dyeing operations. Different samples of madder give, then, different results in dyeing, according to the predominance of one or other of the principles, and, as is well known, different tints according to the mordants used. Pseudo-purpurine is distinguished by its fugitiveness under the influence of soap; and the want of stability in the tinctures made from the madder of Alsace, is due to the presence of this principle in large proportion. M. Martin, observing these points in M. Schützenberger's investigation, and knowing alizarine to be the only one of the five colouring matters yielding an unalterable dyeing material, essayed the transformation of the other principles into alizarine. The result has been that M. Martin has discovered a process by which purpurine, pseudo-purpurine, xantho-purpurine (yellow), and the orange-colouring matter can be transformed into alizarine: the process he has patented. This transformation is easily effected by the combined action of dehydrating and reducing agents. The colouring matters, either separately or mixed, are dissolved in concentrated sulphuric acid, and when dissolved, zinc is added. Elevation of temperature and a finely divided condition of the metal, facilitate the reaction. As soon as the transformation is considered complete, the mass is diluted with water, which gives rise to an abundant precipitate of alizarine; washing with water will render the matter ready for dyeing purposes. It will be interesting to compare the composition of the five principles occurring in madder, as given by M. Schützenberger:—

Alizarine	$C_{40}H_{12}O_{12}$
Purpurine	$C_{40}H_{12}O_{14}$
Orange madder	$C_{40}H_{16}O_{16}$
Pseudo-purpurine	$C_{40}H_{12}O_{18}$
Xantho-purpurine	$C_{40}H_{12}O_{12}$

The process of M. Martin would seem to be one of great importance.

Dr. Löwe has indicated the following method of preparing large quantities of uric acid from Peruvian guano. The guano is pulverised and dried at 100° C.; 1 part by weight is added in small quantities at a time to 1 part of sulphuric acid contained in a capsule, heated by a water bath, and stirred with a glass rod. The mixture is allowed to remain on the water bath as long as hydrochloric acid continues to be evolved When the odor of hydrochloric acid is only slight, and the mixture seems homogeneous, 12 or 15 volumes of distilled water are added; this dilution causes a yellow precipitate, for the subsidence of which time is allowed. Afterwards the supernatant fluid is decanted off, the precipitate is washed with fresh quantities of water, thrown upon a filter, and the greater part of the sulphuric acid washed out. Small portions of this precipitate are now boiled in a weak solution of an alkali, the solution filtered and acidified with dilute hydrochloric acid to precipitate the uric acid, which appears as a yellow cloud. When cold, the precipitate is thrown on to a filter, washed, and dried. The yellow colour may be removed by heating with sulphuric acid at the temperature of a water bath, and repeating the process described: it is necessary to avoid adding more water in precipitating than is absolutely necessary, since the product is always yellow when excess of water has been used.

The extraction of oils by means of bisulphide of carbon is now carried on at Moabit, near Berlin, upon a very great scale. In the manufactory of M. Heyl, 2,570 kilog. of oil, of sufficiently good quality to be employed in lubricating machinery, are manufactured daily. Colza and linseed are the materials chiefly operated upon: the residues serve very well to feed cattle with. The seeds are first crushed and dried by heating. For the daily fabrication of 2,570 kilog. of oil, only six men are required. Analysis has shown the residues to contain only 2 per cent. of oil and 7 per cent. of water, while the residues of the ordinary pressure process contain 9 per cent. of oil and 15 per cent. of water. In the extraction of the oil, 7,000 kilog of bisulphide of carbon are used daily, and the amount lost is 28 kilog.

M. Nickles has published a paper on the sesquifluoferrates. The sesquifluoride of iron forms combinations with fluoride of sodium and ammonium; they are obtained in two ways, either by direct union of the sesquifluoride of iron with the alkaline fluoride, or by decomposing the latter with sesquichloride of iron. The alkaloids also unite with this fluoride: combinations have been prepared with quinine and brucine. When the solutions of the ammonic and potassic fluo-salts are boiled, they are decomposed, depositing yellow flakes charged with iron. Ammonia separates sesquioxide of iron from these solutions; ferrocyanide of potassium causes a blue colouration, unless the solution contain an excess of alkaline fluoride. The colouration which is usually manifested is of a fine violet tint, quite different from the blue precipitate of prussian blue; it is capable of utilisation, M. Nickles thinks, in the preparation of colours.

PARIS, MARCH 24, 1868.

Metallurgy of Iron.—Stannate of Sodium.—Oxidation of Butyric Acid.—Preservation of Meat.—Common Salt as a Manurial Agent.

M. GILLOT'S experiments on the metallurgy of iron have placed in evidence a certain number of points:—1. The theory of the reduction of silica, and the combination of the silicium with iron in the blast furnace; (2) a maximum limit which does not attain 1000° C., for the decomposition of the carbonate of lime; (3) the condition necessary for the progress of the furnace operations, viz., that each charge should furnish alone the total amount of heat required in its treatment; (4) the maximum and minimum limits; *a*, the temperature of complete combustion of the carbon at the tuyere; *b*, the mean temperature of the escaping products which result from this; *c*, the temperature of the body of the gaseous column, accruing to a charge after the conversion of the carbonic acid into carbonic oxide; finally, the temperatures and the modifications of the charge, and of the gaseous column at all stages. (5) The general cause of the transformation of bodies, of which cause cementation, oxidation, and reduction are only particular effects. (6) The principles which regulate the employment of one or of several tuyeres; (7) the theory of the employment of warm air in the blast furnace; the fact of a greater consumption of fuel with warm air than with cold; (8) the respective consumptions of heat by the pig-iron, and by the slag in blast furnaces, and in reverberatory furnaces; (9) the insufficiency of analysis of an aliquot part of the gaseous column, to determine either the composition of this gaseous column, or the reactions which take place in the furnace. Finally, M. Gillot compares the losses sustained in working the old process and the new, thus:—The losses in the carbonisation of the wood, and in the smelting of the ores, taken together, entail in the old process a minimum loss of 90 per cent. of the combustible employed and a consumption of 779 kilos. for every 100 kilos. of iron or steel manufactured. In the new processes, there are only those losses of combustible, common to all systems—loss by radiation, and by the absorption of sensible heat by the products.

MM. Moberg and Rammelsberg have shown that when containing 5 molecules of water are obtained. Since then, M. Haeffely, in re-dissolving these crystals in their mother liquor, has obtained crystals containing 8 molecules; others containing 10 molecules of water have more lately been obtained by M. Scheurer-Kestner, by submitting a weak solution of pure stannate of sodium to a low temperature. The presence of foreign salts, and especially of an excess of hydrate of sodium, prevents the crystallisation.

M. Berthelot has made some experiments upon the oxidation of butyric acid. He performed the oxidation in an alkaline solution, so as to prevent, as far as possible, the destruction of the bibasic acids. For this purpose he dissolved 10 parts of butyric acid in 1200 parts of water, in the presence of 60 parts of potassa. This liquor gradually decolorised permanganate either in the cold, or better at 100°. An experiment was made at 100°, the temperature being maintained during several days, until the butyric acid had destroyed a little more than its own weight of permanganate. The liquor then contained a considerable quantity of carbonate, oxalate, and a small quantity of succinate, not to mention acetate and propionate. To isolate the various acids, this process was adopted. First of all, the liquor was rendered acid with hydrochloric acid; it was then boiled a moment, a drop of ammonia added, and precipitated by chloride of calcium. The precipitate contained oxalate of lime, and a small quantity of an analogous salt more carbonated, probably malonate. The filtrate was evaporated on the water-bath, and the chloride of potassium eliminated by repeated crystallisations. The last residue was rendered strongly acid by hydrochloric acid, and agitated at several intervals with a considerable volume of purified ether. Evaporated to dryness, the ether left a crystalline acid, possessing the properties of, and reacting like, succinic acid. This fact has been verified by the solubility of the lime-salt in water, and by the precipitate occasioned upon the addition of alcohol; the properties of the magnesian salt, the precipitation of the neutral perchloride of iron by the solution of succinate of magnesia, etc., have also lent confirmation. The amount of succinic acid formed is very inconsiderable.

M. Martin has made some experiments upon the preservation of meat by means of ether. He placed in six tin boxes uncooked beef, surrounded by little tufts of cotton wool soaked in sulphuric ether; the boxes were then soldered tight, and exposed to the rays of the sun. Every three months a box was opened. Each piece of meat weighed a kilogramme. At the end of three months there was no alteration either in weight or form. The meat thus preserved does not undergo the putrid fermentation; it is strongly impregnated with ether, and the odour remains after numerous washings with cold water. When cooked the meat possesses a peculiar savour, probably due, M. Martin says, to the formation of a new ether; the fibre is disintegrated. The process is not applicable to the preservation of food, but other animal matters might perhaps be advantageously treated by it.

M. Jean has proposed the following as the mode of action of common salt employed as a manurial agent (considering all the salts likely to be present in the soil). The carbonate of lime decomposes the ammoniacal salts, transforming them into carbonate of ammonia; this carbonate meets in the subterraneous atmosphere with carbonic acid gas produced by the decomposition of organic matters, and forms bicarbonate; then if this salt finds chloride of sodium in the soil, a double decomposition is established, giving rise to chloride of ammonium, and carbonate of soda. The chloride of ammonium, in its turn, is decomposed by carbonate of lime, yielding chloride of calcium, which passes into the sub-soil, and carbonate of ammonia. The carbonate of soda thus produced

matter, and facilitates nitrification. The transformation of chloride of sodium into carbonate of soda takes place so readily that MM. Truck and Schlœsing have applied this reaction in the manufacture of carbonate of soda.

REPORTS OF SOCIETIES.

MANCHESTER LITERARY AND PHILOSOPHICAL SOCIETY.

Ordinary Meeting, January 21st, 1868.

Edward Schunck, Ph.D., F.R.S., &c., *President, in the Chair.*

"*Some Remarks on Crystals containing Fluid*," by J. B. **Dancer**, F.R.A.S.

The author gave a brief history of the discovery of fluids in crystals, including Sir H. Davy's chemical experiments on the fluids and gases obtained from the cavities in quartz crystals; Sir David Brewster's discovery of the pressure cavities in the diamond, ruby, emerald, amethyst, chrysoberyl, &c.; the existence of minute crystals in these cavities and the two new and remarkable fluids, which are immiscible, but sometimes found together in the same cavity—one a liquid hydrocarbon, named Brewstoline, the other Cryptoline; his experiments and examination of artificial crystals deposited from aqueous solutions; his examination of the Koh-i-noor diamond and others in the East India Company's museum; and the geological speculations to which these discoveries gave rise. Mr. Dancer mentioned the experiments of his late father and others in producing artificial gems by intense heat, and stated that his own attention was drawn to this subject some twenty-four years since, by Sir David Brewster presenting him with a specimen of topaz containing fluid. Since that time he had examined a large number of crystals of various kinds, from the collections of friends; and had found fluid in quartz from South America, Norway, the Alps, Ireland, Snowdon, and the Isle of Man; and in fluor spar from Derbyshire; this latter specimen contained a considerable quantity of fluid, which burst the crystal at 180° temperature.* He suggested the employment of the microscope as a valuable assistance in detecting spurious from real gems; very few of the latter are perfect, and the flaws and cavities are so distinct in character from those which are so abundant generally in artificial gems that very little experience is sufficient for the purpose. This mode of testing of course is limited to transparent crystals, but might be employed when the usual methods are not practicable. He also mentioned Mr. Sorby's (F.R.S.) discovery of fluid cavities in the quartz of granite, in the quartz of volcanic rocks, and also in the feldspar ejected from the crater of Vesuvius, and Mr. Sorby's method of determining the temperature at which various rocks and minerals are formed. At the conclusion of the meeting, crystals containing fluid were exhibited under the microscope, and the expansion of the fluid by elevating the temperature of the crystal whilst under examination.

Ordinary Meeting, February 4th, 1868.

Edward Schunck, Ph.D., F.R.S., &c., *President, in the Chair.*

Among the donations announced were several bottles of chemical products for the Society's collection, from Dr. F. Crace Calvert, F.R.S., &c.

The thanks of the Society were voted to Dr. Calvert for his valuable donation.

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* After this Paper was written, Sir David Brewster informed the author that the fluid contained in crystals of fluor spar was water, and that the cavities burst at a temperature of 150°.

[English Edition, Vol. XVII., No. 434, page 184; No. 431, page 118.]

"*On some Constituents of Cotton Fibre*," by E. Schunck, Ph.D., F.R.S., &c., *President.*

It is generally supposed that cotton, when quite pure, consists entirely of woody fibre or cellulose, and that its composition is consequently represented by the formula $C_{12}H_{10}O_{10}$. It is certain, however, that in the raw state, as furnished by commerce, it contains a number of other ingredients, some of which occur so constantly that they may be considered essential constituents of cotton, viewed as a vegetable product. The object of the bleaching process to which most cotton fabrics are subjected is to deprive the fibre of these other ingredients and leave the cellulose behind in a state of purity. Notwithstanding the importance of an accurate knowledge of everything relating to cotton from an industrial point of view, the substances contained in it along with cellulose have never been subjected to a special chemical examination, and very little is consequently known about them. Persoz, in his *Traité de l'Impression des Tissus*, says that the woody fibre constituting the tissues of cotton, hemp, linen, &c., is not pure; it contains—1st, a certain quantity of colouring matter, which is more or less shielded from the action of decolourising agents by the bodies which accompany it, naturally or accidentally; 2ndly, a peculiar resin, natural to the fibre, insoluble in water and soluble with difficulty in alkalies, which plays the part of a reserve and protects the colouring matters inherent in the fibre from the action of the agents which ought to destroy and remove them; 3rdly, a certain quantity of fatty matter, of which a very small portion is peculiar to the fibre, the greatest part being derived from the operations of spinning and weaving; 4thly, a neutral substance, either flour, starch, or glue, which has been introduced by the weaver in sizing his warp; 5thly, inorganic saline matters, some of which belong to the fibre, while the others are derived from the water and the matters employed in the dressing of the warp. In the excellent article on Bleaching in the new edition of Ure's *Dictionary of Arts*, there is a full account of these and other impurities of cotton fabrics, comprising all that was known at the time when the author commenced his examination.

The object which the author had in view in undertaking his investigation was to endeavour to throw a little more light on the nature of those substances which are contained in or attached to the framework of cellulose, of which cotton fibre mainly consists, and which are, together with the latter, produced by the plant. All foreign and extraneous matter introduced during the process of manufacture was therefore left entirely out of consideration. The author has further confined his attention to those constituents of the fibre which are insoluble in water but soluble in alkaline ley, and are afterwards precipitated by acid from the alkaline solution. Whether cotton contains naturally any substance soluble in water, or which being originally insoluble is rendered soluble therein by the prolonged action of alkalies, is a question on which the author pronounces no decided opinion.

For the purpose of obtaining the substances which he proposed to examine, the author employed cotton yarn, which he preferred to unspun cotton for several reasons, the principal being that yarn is comparatively free from mechanical impurities, such as fragments of seed-vessels, &c., while on the other hand, if proper care be taken, no impurity is added to those previously existing during the process of spinning. The yarn was boiled in an ordinary bleacher's kier for several hours with a dilute solution of soda ash. The resulting dark brown liquor, after the yarn had been taken out, drained, and slightly washed, was removed from the kier into appropriate vessels, and mixed with an excess of sulphuric acid, which produced a copious, light brown, flocculent precipitate, while the liquid became colourless. This precipitate was allowed to settle, the liquid was poured off, and after being washed with cold water to remove the sulphate of soda and excess of acid it was put on calico strainers and allowed to drain. A thick pulp was thus obtained, which when dried assumed the appearance of a brown, brittle, horn-like substance, translucent at the edges. In one experiment 450 lbs. of yarn, made from East Indian cotton, of the variety called "Dhollerah," yielded 0·33 per cent. of the dried precipitate. In another experiment made with 500 lbs. of yarn, spun from American cotton, of the kind called in commerce "middling Orleans," 0·48 per cent. was obtained. The total loss sustained by yarn during the bleaching process amounts to about 5 per cent. of its weight. Only a small portion of the matter lost is therefore recovered by precipitation of the alkaline extract with acid.

This precipitate formed more especially the subject of the author's investigation. It was found to consist almost entirely of organic substances, and of these the following were distinctly recognised:—

1. A species of vegetable wax.
2. A fatty acid.

3, 4. Colouring matters.

5. Pectic acid.
6. A trace of albuminous matter.

The author described the method employed by him for separating these substances from one another and obtaining them in a state of purity; and he then gave an account of their properties and composition.

The waxy matter is by far the most interesting of these substances. It is insoluble in water, but soluble in alcohol and ether. If a concentrated solution in boiling alcohol be allowed to cool, the greatest part is deposited, causing the liquid to assume the appearance of a thick white jelly, consisting of microscopic needles or scales. When this jelly is filtered off and dried it shrinks very much, and is converted into a coherent cake, which has a waxy lustre and is translucent, friable, and lighter than water. Its melting point is between 83° and 84° C. At a higher temperature it is volatilised. When heated on platinum it burns with a very bright flame. The author thinks it probable that this substance covers the cotton fibres with a thin waxy film, and thus imparts to them their well-known property of resisting water. In its properties and composition it approaches very nearly the better known vegetable waxes, such as that obtained by Avequin from the leaves of the sugar-cane, and that which is found on the leaves of the Carnauba palm. The author thinks that the name *cotton wax* is sufficient to distinguish it from these and other nearly allied bodies.

The fatty acid has the properties and composition of margaric acid. It is white and crystalline, fuses at 53° C, and gives with alkalies compounds soluble in water, which are true soaps. It is, however, probably not a natural constituent of cotton fibre, but rather an impurity derived from the oil of the seed which escapes and diffuses itself among the cotton before or during the process of ginning. It might also have had its source in the oil and fat used for greasing the cotton spinning machinery, since the author employed yarn in all his experiments. Persons practically conversant with cotton spinning affirm, however, that if ordinary care be taken, it is impossible that the cotton can become contaminated with anything of a fatty nature during its conversion into yarn.

The colouring matters obtained in these experiments are without doubt the substances to which raw cotton owes its yellowish or brownish colour. The author was able to distinguish two bodies of a dark brown colour, which occurred in all kinds of cotton examined by him. Of these one is easily soluble in cold alcohol, and is left, on evaporation of the solution, as a dark brown, shining, brittle, amorphous resin, which is transparent in thin layers. In boiling water it softens and melts to a pasty mass, which becomes hard and brittle again on cooling. When heated on platinum foil it burns with a bright flame, leaving a very voluminous coal. It is nearly insoluble in ether. It dissolves easily in concentrated sulphuric acid and glacial acetic acid, with a brown colour. It also dissolves with ease in caustic and carbonated alkalies, giving dark, yellowish-brown solutions, from

Cold alcohol, indeed, dissolves only a trace, but in boiling alcohol it dissolves with tolerable facility, being re-deposited, on the solution cooling, in the form of a brown powder. This powder, when filtered off and dried, forms coherent masses of a colour varying from light to dark brown, which are easily broken, showing a dull earthy fracture. Both colouring matters contain nitrogen, and they differ therefore in constitution from true resins, which they resemble in many of their properties. The peculiar colour of the so-called "Nankin cotton" is probably due to a great excess of these colouring matters existing in the fibre. It is certainly not caused by oxide of iron.

The purification of the pectic acid contained in the brown precipitate produced by sulphuric acid was not effected without difficulty. The best method, according to the author, consists in submitting it to a simple process of bleaching with chloride of lime, by which means the impurity, consisting of brown colouring matter, which adheres to it with great pertinacity, is destroyed. When pure it has the properties and composition ascribed to pectic acid by Fremy. The cotton itself probably contains pectose or pectine, which is converted into pectic acid by the action of the alkaline ley. About three-fifths of the brown precipitate consists of pectic acid. Of the remaining two-fifths the colouring matters constitute by far the largest part, the wax and fatty acid being present in very minute quantities. The albuminous matter was not isolated, but its presence was indicated by the formation of a small quantity of leucine, which took place when the brown precipitate was submitted to the action of hydrate of soda. A large quantity of oxalic acid was formed at the same time, no doubt from the pectic acid.

In conclusion, the author makes some remarks in regard to the part which these bodies may be supposed to play during the process of manufacturing gun-cotton. It has been asserted that the instability occasionally observed in gun-cotton is to be attributed to the impurities in the raw fibre, forming, by the action of nitro-sulphuric acid, bodies which decompose spontaneously at the ordinary or a slightly elevated temperature. The author's experiments do not support this view, since the substances described by him, when submitted to the action of the mixed nitric and sulphuric acid of the strength employed for making gun-cotton, do not yield explosive compounds.

ACADEMY OF SCIENCES.

FEBRUARY 10, 1868.

On the heat set in motion during chemical combinations.—A new body analogous to diastase.—Nitrous fermentations.—Influence of light on vegetation.—Electro-capillary actions.—On the part played by electricity in muscular contraction.

AT the meeting held on the 10th of February, M. Favre contributed a memoir on researches on the heat set in motion during chemical combinations and decompositions.

Three memoirs were sent by M. Dubrunfaut; the first related to a new body analogous to diastase; nitrous fermentations, lately the subject of examination by M. Reiset, formed the subject of the second, and the influence of light upon vegetation the third.

M. Becquerel communicated a further account of his researches in capillary chemistry, and M. Marey brought before the Academy his researches on the part played by electricity in muscular contraction.

M. Favre has studied the compounds formed by the union of sulphuric acid, considered as a hydrogen salt, with zinc, iron, copper, and cadmium. He measured first the heat produced by the oxidation of the metal, then that produced by the combination of the acid with the oxide, and finally that caused by the hydration of the salt. He found that a result already announced by him,—that the heat produced by the combustion of a body, or absorbed during reduction, does not represent the whole of the heat put in motion. A certain amount of heat is necessary to prepare the body for combination or decomposition. The general result of M. Favre's work is that in the case of the salts with which he has experimented, these are more correctly represented as direct combinations of SO_4 with the metal, than as sulphates of oxides.

The matter analogous to diastase, described by M. Dubrunfaut in his first memoir, possesses the same property as diastase, but it is distinguished from it in having less saccharifying power, and by the property of rendering fluid 1,000 or 2,000 times more starch. The second memoir was chiefly a criticism of M. Reiset's recent communication on nitrous fermentations. M. Dubrunfaut has obtained in recent experiments confirmation of the theory formerly proposed by him to explain these phenomena. The following is the theory referred to:—Lactic acid is developed, and this decomposes the nitrate of potash of the saccharine juice, liberating a certain quantity of free nitric acid, which, by contact with organic matters, is reduced to the state of binoxide of nitrogen; then the binoxide of nitrogen, by contact with the atmosphere, becomes nitrous acid. In M. Dubrunfaut's last memoir, a study of the influence of light on vegetation, he indicates a method of valuing the mechanical work performed by the light in the decomposition of carbonic acid.

PARIS, MARCH 10, 1868.

Death of M. Foucault.—Products of the distillation of beet-root.—Oxygenated water, not the cause of colouration in ozone test-papers.—Employment of salts of potash in agriculture.—Examination of flour.

ON the 17th of February the Academy was informed, by the President, of the loss it had sustained in the death of two distinguished savans, Sir David Brewster and M. Leon Foucault.

M. Pasteur presented a pamphlet, entitled "A Study of Vinegar: its fabrication, accidents, and the means of preventing them. New observations upon the preservation of wines by subjecting them to heat."

MM. Pierre and Puchot presented a memoir, entitled "Experimental researches on the products of the distillation of beet root."

M. Chacornac addressed a note concerning the intimate constitution of light and the formation of nebulæ.

M. Phipson sent a note on some luminous phenomena which accompany meteoric showers.

M. Houzeau sent a note on oxygenated water considered as not being the cause of the alterations induced by the atmosphere in litmus, and iodide of potassium and starch paper as ozone tests.

Experimental researches on the employment of salts of potash in agriculture was the subject of a note by M. Deherain; and there was a note on the qualitative and quantitative examination of rye flour; and of alcoholic liquids by means of chloroform, from M. Rakowitsch. These are all the communications relating to chemistry brought before the Academy at this *séance*; there were several physiological papers, among others one upon the nature of vaccine virus, which elicited considerable discussion.

The memoir of MM. Pierre and Puchot, communicated by M. Wurtz, is an account of a research which has occupied them more than three years. It relates, as has already been mentioned, to the products of the distillation of beet root. When the ordinary rectification of the fermented liquor is observed with attention, a disagreeable penetrating odour is perceived in the first portions which come over. These

[English Edition, Vol. XVII, No. 431, pages 119, 120; No. 432, page 130.]

products often cause liquids with which they are mixed to colour spontaneously. The disagreeable odour of the products manifests itself a considerable time, to the annoyance of the distillers, since they are obliged to keep the portion contaminated separate from the alcohol of good flavour which follows, and sell it at a reduced price. MM. Pierre and Puchot have observed the presence of avinic aldehyd, of which they have separated, though not without considerable trouble, several litres. The aldehyd thus separated, boils a little below 22°; a specimen made three months ago is still perfectly clear.

If, towards the end of the distillation, a sample be collected and examined, the odour of amylic alcohol will be perceived; at the end of the operation, this alcohol distils over almost free from other alcoholic products. Examined more closely at this stage, the distillate is found to contain other definite substances, such as butylic and propylic alcohols. To test the nature and purity of these two alcohols, MM. Pierre and Puchot have prepared the corresponding iodides and acetates. The butylic alcohol boiled at 107·5°, its iodide at 122·5°, its acetate (isomeric with ethylic butyrate) at about 116°. The propylic alcohol boiled at about 98·50, its iodide about 104·50, and its acetate (isomeric with methylic butyrate) about 105°. In conclusion, they remark upon the deceitful nature of the appearances manifested when investigations of bodies such as they have been working upon are made with restricted quantities of material. It is not easy, they say, to distinguish always mixtures possessed of a relative stability from definite and true compounds. Elementary analysis, too, cannot always solve the point, and many examples are cited to show how mixtures of two alcohols may yield the same percentage results as a third pure alcohol.

Some of the other memoirs deserve more than a passing notice, and your correspondent will introduce accounts of them hereafter.

M. Dehérain has arrived at a certain number of conclusions regarding the employment of salts of potash in the cultivation of wheat, potatoes, and beet-root. He finds the salts of potash have generally augmented the wheat crops, which have been augmented still more when ammoniacal salts and phosphatic manure have been also added. Pure potash manures have not increased potato crops; when ammoniacal salts and phosphates have been added as well, a slightly greater yield has been obtained, but not sufficiently to make the employment of these manurial agents profitable. In the cultivation of the beet-root the facts are precisely the same. The experiments upon which these conclusions are based were made upon a large scale in a part of the domain of l'Ecole de Grignon.

M. Rakowitsch proposes a method of examining flour by means of chloroform. The following are the results which he says may be gathered from an experiment capable of being made in a few minutes:—The amounts of bran, the moisture between 10 and 25 per cent., the damaged flour, the mineral matters, the ergot of rye, and other impurities. The whole of these are determined by the relative specific gravities of the different substances in chloroform. The flour is simply placed in a tube and mixed with chloroform; the chloroform is enabled to hold, in very thorough suspension, the pure flour, while the other materials are not thus suspended. By adding spirits of wine of 95°, the flour is precipitated to the bottom of the tube. The more humid the flour, the more spirits of wine must be added, and thus the amount of humidity in the flour is arrived at.

FEBRUARY 24, 1868.

Production of Chlorine and Oxygen.—Products of the slow Oxidation of Phosphorus.—Diffusion and Endosmose.—Mode of Action of Common Salt as a Manurial Agent.—Antiputrescent Properties of Sulphuric Ether.

AMONG the memoirs brought before the Academy at the séance of the 24th of February, were the following:—A memoir on the production of chlorine and oxygen, by M. Mallett; on ozone and phosphoric acid, the result of the slow oxidation of phosphorus, from M. Blondlot; a memoir relating to diffusion, endosmose, molecular movement, &c., from M. Dubrunfaut; on the mode of action of common salt employed as a manure, by M. Jean; a note on the antiputrescent properties of sulphuric ether, from M. Martin; analyses of some waters from the thermal springs of Ischia, near Naples, by MM. Mène and Rocca Tagliato. The section of rural economy has presented the following list of candidates for the place vacant in it from the death of M. Rayer:—(1) M. Reiset; (2) MM. Bouley, Dubrunfaut, and Hervé Mangon; (3) M. Richard.

M. Mallett's memoir was an explanation of a process to which he called the attention of the Academy last year. He remarked that the fixation of the atmospheric oxygen upon protochloride of copper, permitted either of making the latter yield the oxygen, or yield chlorine upon addition of hydrochloric acid. The absorption of oxygen by protochloride of copper is spontaneous; the air being ordinarily moist, it will be complete in a few hours, if fresh surfaces be renewed But elevation of temperature, and this is a main point, induces a much more rapid absorption; at temperatures between 100° and 200°, as well as at higher temperatures in the presence of water, this absorption may be considered as almost instantaneous. By this process 100 kilog. of chloride of copper (cuprous chloride), usually mixed with inert matter for convenience, will yield 3 to 3½ cubic metres of oxygen, or 6 to 7 cubic metres of chlorine, and as four or five operations may be made in four-and-twenty hours, this quantity, 100 kilog., would yield 15 to 18 cubic metres of oxygen, or 200 to 300 kilog. of chloride of lime, during the same time: the price of the chloride of copper does not exceed 1 franc the kilogramme.

When phosphorus undergoes slow combustion in air, it is generally considered, M. Blondlot says, that ozone and phosphorous acid are produced; these two bodies being incompatible, he thought the matter worthy of investigation. For this purpose, he took a flask of several litres capacity, closed with a cork carrying two tubes. One descending to the bottom of the vessel, communicated at the upper extremity, by means of a caoutchouc tube, with a reservoir of water furnished with a tap; the other was simply a curved tube for the delivery of the gas. In the ascending portion of this tube he placed a thin cylinder of phosphorus about 15 centimetres in length. This arrangement made, a fine jet of water was made to issue into the flask, when the air was expelled, bubble by bubble, between the phosphorus and the sides of the tube. The resulting gas, collected in the usual way, was washed at several intervals with water, until white vapours had completely disappeared. Two important facts have been demonstrated by the experiment. The first is, that if the ambient air should not attain vigorously 12°, when it leaves the apparatus, it will have acquired distinctly the characteristic odour of ozone, but it will not affect iodide of potassium and starch paper; while if the air of the apparatus is at 12° to 13°, these same papers, suspended in the receiving flasks, become as distinctly blue as if the temperature had been much higher. The second is that, whatever temperature one operates at, the white vapours which escape from the apparatus are composed exclusively of phosphoric acid, without admixture of phosphorous acid. There is no difference in the product after the greater part of the oxygen has been withdrawn from the air, this condition being attained by collecting the escaping gases in a flask over water, and then making the phosphorus undergo slow combustion in these collected gases. To be enabled to prove that phosphoric acid is the only product of combustion, it suffices to pass the escaping gases into distilled water. The solution, very distinctly acid, being exactly neutralised with potash, causes in nitrate of silver a yellow precipitate; it does not decolourize permanganate of potash, and introduced into a Marsh's apparatus, produces no green flame. This being so, it would seem curious why, in the class ex-phosphoric acid, to which a few pieces of phosphorus have been added, and corked, a portion of the phosphoric acid is speedily converted into phosphorous acid according to the equation $3PO_5 + 2P = 5PO_3$. From this it follows that when small sticks of phosphorus are exposed in narrow tubes to the action of moist air, the phosphorous acid produced is the result of a deoxidation of the higher oxide first produced. In conclusion, M. Blondlot states that, whatever be the rapidity of the combustion, when phosphorus undergoes combustion in air, phosphoric acid is the only product.

M. Dubrunfaut placed before the Academy a number of theorems relating to diffusion and endosmose. Among others, these:—Diffusion is always a molecular property, whether manifested with or without diaphragms (the first condition being considered endosmose, the second, diffusion). It is always accompanied by the double current, observed by Priestley and Dutrochet, and it is the result of an attractive force which is developed by the juxtaposition of molecules of matter of different kinds, or of molecules of matter in a different physical state. The effects of diffusion and endosmose are referable to one force—diffusion, which, though exerted at insensible distances, cannot be said to result from contact, since these distances are really great, and depend upon the finite dimensions of the molecules of the matter operated upon. The force of diffusion is always exerted in one direction, which is normal at the surface of contact of the fluids. It varies in intensity with different fluids, and with fluids of different densities, wherefore, also, with the temperature and pressure. Mechanical work in commensurable degree is performed. In fact, fluids which mix with great perfection offer with the displacement of their centres of gravity useful data wherewith to calculate the work performed in producing the mixtures.

MARCH 2, 1868.

Election of M. Bouley.—M. Le Verrier's Assistants.—Physical Properties and Calorific Power of Petroleum.—Analysis of Vegetable Tissues.—Corresponding Term to Benzoic Acid in the Naphthalic Series.—Diastase.

AT the meeting of the Academy of Sciences held on the 2nd of March, the election of a member in the place vacant in the section of rural economy, was proceeded with. M. Bouley was the successful candidate. The meeting was essentially stormy. M. Deville had at the previous meeting defended in very energetic terms M. Foucault against some imputations M. Le Verrier had cast upon his memory. M. Le Verrier denied at this meeting that he had any other purpose than to do the amplest justice to, and exalt the fame of, the illustrious deceased. Afterwards another question became involved. The president, M. Delaunay, considered it right that the name of the assistant who discovered the 96th little planet should be made known; he said: "I have the honour to inform the Academy that the young man to whom the discovery of the 96th little planet is due, is M. Coggia." M. Le Verrier replied that only one result could follow from M. Delaunay's remarks—disorder in the observatory of Marseilles. He said that the young gentlemen who discovered planets at Marseilles did not deserve to be cited; the work they did was simply manual, and required no knowledge of astronomy. They were very well satisfied, he also said, with their condition; they received for every planet discovered an increase in salary of 250 fr., and a gold medal. A general protest from the members followed this statement.

M. Deville brought before the Academy at the *séance* of the 9th March a memoir on the physical properties and the calorific power of petroleum and mineral oils. A large number of samples were experimented with. The mineral oil was submitted to distillation in a copper alembic furnished

below 140°. The same experimental fact represents as well the loss which must be sustained to remove the explosive property of the oil. Another danger is encountered when the oils are enclosed in air-tight vessels—explosion by dilation. The amount of space necessary to be left above a mineral oil is calculated from the coefficient of dilation. The data M. Deville has obtained from each sample are drawn generally from the following determinations. Loss by heating to 100°, to 120°, and so on, by intervals of 20° up to 200°; this is expressed in percentages. Composition of the oil, *i.e.*, percentages of carbon, hydrogen, and oxygen, obtained by combustion. Density at zero, and at 50°, and coefficient of dilation. Composition and density of the oil obtained by distillation, and density of the residue. In some cases the specific heat has been determined, and the latent heat at the mean temperature. M. Deville's memoir contained tables giving an immense number of experimental results; it is, however, only a first memoir; more upon the subject will be brought before the Academy shortly. Perhaps it will not be without interest to tell your readers that M. Deville has undertaken this research by command of the Emperor, to report upon the most advantageous arrangements to adopt for the economic and safe employment of mineral oils, with especial reference to its use in transports. M. Elie de Beaumont drew M Deville's attention to a blackish schist which occurs at Vassy, near Avallon, as a source of oily matter; this schistous substance extends over a very large area.

MM. E. Fremy and Terreil contributed, at the same meeting, a memoir upon a general method for the immediate analysis of vegetable tissues. In wood they recognise the existence of three principles. The first component cannot be easily mistaken; it is insoluble in sulphuric acid, containing two equivalents of water; it is further transformed by chlorine water into a yellow substance, and afterwards dissolved, nitric acts in the same way as chlorine; it is not soluble in potash solution, either dilute or concentrated. The substance is distinguished by the name of the ligneous cuticle; it has also been designated by M. Hartig by the name of *eustathe*, in consequence of its great stability. The second component of ligneous tissue has been studied by M. Payen, under the name of *incrusting* substance: qualitatively, they recognise its existence by its solubility in sulphuric acid, which becomes blackened, and afterwards by its insolubility in alkaline solutions and in chlorine water. The third component is cellulose. When pure, cellulose dissolves without giving rise to colouration in concentrated sulphuric acid, and water does not precipitate it from the solution; it is difficultly attackable by chlorine water, and by nitric acid. They detail in their memoir the actual process. 1 grm. of the sawdust dried at 130° is introduced into a flask, with about a litre of chlorine water, and allowed to remain here for thirty-six hours. The chlorine water dissolves the ligneous cuticle and certain parts of the incrusting matter; it leaves in an insoluble state, cellulose mixed with a part
the incrusting matter, which has been transformed into
acid completely soluble in potash. This residue left by
chlorine water is, therefore, treated with an alkaline
tion, afterwards washed with acid, then with wat
finally dried at 130°. Cellulose is thus obtained i
of absolute purity. Determinations gave 40 per ce
substance for oak wood and 39 per cent. for the w
ash. For the determination of the ligneous
take again 1 grm. of sawdust, and submit it to
sulphuric acid, containing 4 equivalents of w
six hours; the portion which it is required
remains insoluble; in some cases they re
other containing only 2 equivalents of w
is washed with water and with an alkali
washings are no longer coloured; it

mined by difference. But this part of the wood is composed of several substances, and they separate them thus: (*a*) matter soluble in boiling water; (*b*) bodies, probably of the nature of pectose, which dissolve in dilute solutions of alkali; (*c*) matter rendered soluble in alkaline solutions, by treatment with moist chlorine. For instance, in the ligneous tissue of oak wood, they estimated the incrusting matter by difference, at 40 per cent.; then they found *a* 10 per cent, *b* 15 per cent., and *c* 15 per cent.

Dr. A. W. Hofmann contributed a memoir on the corresponding term to benzoic acid in the naphthalic series, M. Payen, a memoir entitled "extraction and properties of diastase;" there were also several other very interesting papers, accounts of which will probably be introduced in your correspondent's next letter.

CHEMICAL SOCIETY.

Thursday, March 5, 1868.

Dr. Warren de la Rue, F.R.S., &c., *President, in the Chair.*

The minutes of the two previous meetings were read and confirmed, and a long list of donations to the library announced. Dr. Schenk, Mr. Vosper, and Mr. Gilbert W. Child, were formally admitted as Fellows of the Society, and the following gentlemen were duly elected, viz., Dr. Benjamin H. Paul, 8, Gray's Inn Square; Mr. Thomas W. White, Ifield, near Crawley, Sussex; Edward Dowson, M.D., 117, Park Street, London. Mr. Reinhold Richter, of the Rothamstead Laboratory, was also elected as an Associate. The candidates proposed for admission were John Tyndall, LL.D., F.R.S., Fullerian Professor of Chemistry in the Royal Institution of Great Britain; Frederick Guthrie, Ph.D., F.R.S. Edin., Lecturer on Chemistry in the College of Mauritius; William Brantingham Giles, Chemist at the Borax Works, Old Swan, Liverpool. For the second time were read the names of Mr. R. Calvert Clapham, Walker Alkali Company's Works, Newcastle-upon-Tyne; Rustomjee Byramjee, M.D., Assistant-Surgeon in Her Majesty's Bombay Army; and Edward Menzel, Ph.D., recommended by the Council to become an Associate.

The names of officers and other members of Council proposed for election at the anniversary meeting on the 30th of March were announced. For President, Dr. Warren de la Rue, F.R.S., F.R.A.S, &c. For Vice-Presidents, Dr. E. Frankland, F.R.S., and Dr. J. H. Gilbert, F.R.S. For Foreign Secretary, Prof. F. A. Abel, F.R.S. New Members of Council:—Dr. E. Atkinson, Dr. E. J. Mills, Mr. W. H. Perkin, F.R.S., and Mr. John Williams.

Professor J. A. Wanklyn read a paper "*On the Action of Oxidising Agents on Organic Compounds in Presence of Excess of Alkali,*" of which Mr. E. T. Chapman and himself were joint authors. Part I. "*Ammonia evolved by Alkaline Permanganates acting on Organic Nitrogenous Compounds.*" It has already been shown in previous communications that albumen evolves ammonia when submitted to the action of alkaline permanganates, and, further, it is asserted that this ammonia is perfectly constant in quantity, and always proportional to the amount of albumen employed, although the whole of the nitrogen does not take this form. The authors have now extended this inquiry to organic nitrogenous substances in general, and find the action to be definite, yielding proportions of ammonia varying with the nature of the body acted upon, and sometimes the entire quantity was obtained. A number of typical substances were selected for examination with the following results:—

Class I. Bodies furnishing the *whole* of the nitrogen in the form of ammonia: amylamine, di-amylamine, asparagine, piperine, piperidine (sulphate), narcotine, diphenyltetramide, and hippuric acid.

(sulphate), cinchonine (sulphate), nicotine, naphthylamine, toluidine, and rosaniline (acetate).

Class III. Creatine, which gives off *one-third* of its contained nitrogen in the form of ammonia upon distillation with the alkaline permanganate, is conceived to contain the remaining two-thirds in the form of urea, which by previous experiment was found to give only nitrogen and nitric acid. It is, therefore, concluded that sarcosine, which, together with the elements of urea, make up the original creatine, will furnish the whole of its nitrogen as ammonia,—a supposition which awaits the confirmation of direct trial.

Class IV. Theine, gives off *one-fourth* of the nitrogen.

Class V. includes bodies which evolve various proportions of nitrogen in the form of ammonia. 100 parts of uric acid give about 7 parts of ammonia; caseine 7·6 parts, dry albumen about 10 parts, and gelatine 12·7 parts of ammonia. Picric acid, as the type of a nitro-compound, gives no ammonia on treatment with alkaline permanganate.

The authors finally conclude that nitro-nitrogen does not give ammonia; that amidogen, imidogen, and perhaps nitrogen, are evolved in the form of ammonia from organic bodies derived from marsh gas and its homologues. Compounds derived from hydrocarbons below marsh gas do not give up the whole of the nitrogen as ammonia. The anomalous results obtained in the case of urea are explained by the circumstance, that no oxidation of this substance is possible without destruction of the amidogen. The authors reserve the full consideration of the residual nitrogen (in cases where there is any), and of other complementary products of the oxidation.

A short discussion then took place, having reference to the means of purification of distilled water and the application of the Nessler test, in which Messrs. Dugald Campbell, Wanklyn, Chapman, and Thorp took part.

The President exhibited some interesting examples of phosphorescent salts, arranged in series so as to imitate the colours in the solar spectrum. A butterfly also, with gorgeous wings extended, was constructed by placing the various salts in patches against a glass plate. These illustrations were the work of M. Gaiffe, and were said to have been prepared from the sulphates of barium, calcium, &c., reduced by heating with carbon to the state of sulphides. To start the phosphorescent activity of these salts the frames were exposed to the intense light given out during the combustion of about six inches of magnesium ribbon. [The phenomenon was exhibited with good effect in the meeting-room after the gas-lights had been lowered.]

A "*Note on Dr. Frankland's Process of Water Analysis*" was read by Mr. E. T. Chapman. The author calls in question the accuracy of the method lately proposed for the destruction of nitrates in natural waters, by evaporating with aqueous sulphurous acid, even when perchloride of iron or phosphoric acid is added to the portion of liquid under treatment. The objections raised were, firstly, that the decomposition was incomplete; and, secondly, that the sulphuric acid formed, or other free acids present in solution, would decompose a portion of the organic matter and entail a loss of carbon.

Mr. Chapman then proceeded to read a "*Note on the Estimation of Nitric Acid in Potable Waters*" This analytical method depends upon the reduction of the nitric acid to ammonia by the action of nascent hydrogen—a condition practically ensured by distilling the water with pure caustic soda, to expel, in the first instance, any ready-formed ammonia, then cooling the contents of the retort and introducing aluminium foil, which, during solution, effects the reduction of the nitrates to ammonia. After standing for several hours heat is again applied, and the distillation proceeded with until no more ammonia comes over. The amount of the latter is ascertained either by the Nessler test, or by the method of titration with standard acid. The possible occurrence of nitrates in the caustic soda employed is to be guarded against, and some special precautions are taken for the purpose of avoiding loss of ammonia by diffusion of air into the apparatus.

Dr. WILLIAMSON inquired whether, in proceeding to effect the destruction of nitrates by sulphurous acid in the presence of proto-salts of iron, Mr. Chapman had exactly adhered to the proportions of these re-agents prescribed by Dr. Frankland?

Mr. CHAPMAN said he had used even double the quantity of sulphurous acid without completely decomposing the nitric acid.

Dr. ODLING, in rising, said that he was not about to offer any testimony as to the truth or inaccuracy of either of the proposed methods of determining nitric acid; that point would be ascertained, not by discussion, but by patient investigation. It appeared to him possible that both methods of operating would give successful results if all the prescribed conditions were fulfilled. On referring to Dr. Frankland's published paper, which he held in his hand, he found recorded a specific experiment in which a water containing nitrate was treated with sulphurous acid, evaporated to dryness in vacuo, and the residue afterwards burnt. As the result of the combustion, no trace of nitrogen was obtained, and this evidence seemed to be perfectly conclusive. He admitted that apparently trifling modifications in the mode of conducting an experiment might sometimes influence the result, and this showed the necessity of thoroughly working out a process before submitting it to public criticism. For his own part he could not help saying that the new methods lately proposed by Messrs. Wanklyn and Chapman would have met with a more welcome reception at the hands of chemists, if the results, instead of being published piecemeal, had been kept back until a complete and thoroughly verified system of analysis had been worked out.

Professor WANKLYN repudiated the accusation of having brought forward his method of analysis prematurely, by asserting that all the details published in June last had been confirmed by evidence since obtained. He used now, instead of a litre of water, only half a litre for the distillation, and in other trifling respects had improved the process. He would, however, insist upon the truth of the leading axiom, that the amount of ammonia obtained was always proportional to the "badness" of the water, and the method was, therefore, in all cases, applicable.

Professor ABEL said that, upon the first appearance of Messrs. Wanklyn and Chapman's method of analysis, he had felt it his duty to inquire into the applicability of the new process; but he soon found that modifications were to be introduced, and that statements made in one paper were contradicted in the next. He had failed in arriving at such definite results as would enable him to adopt the process in its present form, and he entirely agreed with Dr. Odling in considering that the authors had laid themselves open to the accusation of having published their paper somewhat prematurely.

After a few words from Mr. CHAPMAN, the President invited Mr. W. H. PERKIN to read his paper "*On the Hydride of Aceto-Salicyl.*" In the present communication, the author gives a fuller account of the hydride of aceto-salicyl, the formation of which was mentioned in a previous paper when treating of the artificial production of coumarine. The body in question was prepared by acting with acetic anhydride upon the hydride of sodium-salicyl in the state of fine powder suspended in pure ether. After twenty-four hours' contact the etherial fluid was evaporated, and furnished a white satin-like crystalline mass upon cooling. It is possessed of aldehydic properties, and has the following composition:—

$$C_9H_8O_3 = \left\{\begin{matrix} COH \\ C_6H_4 \\ C_2H_3O \end{matrix}\right. \left.\begin{matrix} \\ \\ \end{matrix}\right\} O.$$

By digesting this product with more of the acetic anhydride, another crystalline body is formed, which fuses at 100°—101°C., and contains the elements of the two substances which give rise to its production. Its formula is then—

$$C_{13}H_{14}O_6 = C_9H_8O_3, C_4H_6O_3.$$

The rest of Mr. Perkin's paper is devoted to an account of the formation of coumarine, from which it appears that this body is only formed when acetate of sodium is present, together with the other ingredients used in its production; this apparent anomaly is explained by assuming the formation, in the first instance, of the sodium-salt corresponding to Gerhardt's "anhydrous biacetate of potassium."

A paper "*On the Absorption of Vapours by Charcoal,*" by John Hunter, M.A., of Queen's College, Belfast, was read by the Secretary. This communication resumes the subject of a previous experimental inquiry, and describes the amount of mixed vapours, as well as of a number of new substances in a state of vapour absorbed by a known volume of cocoa-nut charcoal, under varying circumstances of temperature and pressure. Among the substances examined were the following:—Ethylamine, iodide of ethyl, acetate of methyl, oxalic ether, hydride of salicyl, salicylic acid, iodide of amyl, naphthaline, camphor, nitro-benzol, bisulphide of carbon, alcohol, acetone, and methylic alcohol.

The next paper read was "*On the Occurrence of Prismatic Arsenious Acid,*" by Mr. FREDERICK CLAUDET. The dimorphism of arsenious acid was discovered by Wöhler, who found in the flue of a reverberatory furnace crystals of this substance, produced by sublimation, which were not octohedral. The author exhibited a fine sample of a product naturally formed in fissures of an arsenical pyrites ore, occurring in the San Domingos mines, Portugal. This proved on analysis to be pure arsenious acid in the form of thin plates, similar in appearance and structure to the mineral selenite, and having a beautiful pearly lustre. The specific gravity was found to be 3·85, and hardness 2·5. An analysis of the ore is given, from which it appears that arsenic occurs to the extent of 0·47 per cent., and copper about 3 per cent., with a host of other metals in smaller quantity, including a trace of thallium. There are indications in the mine of a slow process of oxidation having been going on for many years, and the mineral from which the arsenious acid was taken remained quite hot to the touch after it had been raised from the workings. The substance in question is believed to be the result of a very slow process of sublimation, and the form is probably modified by the sulphurous atmosphere pervading those parts of the mine.

Professor WARINGTON SMYTH, having been invited to speak, said he was not conversant with the locality where these crystals were found, but from the full description given by the author he thought it probable that his speculations regarding the origin of the substance, were correct. It was interesting as furnishing evidence of the dimorphism of arsenious acid, since two kinds of oxide of antimony were previously known.

The next paper was by Dr. Stenhouse, on the "*Action of Nitric Acid on Picramic Acid.*" The author reconciles the conflicting statements regarding the nature of the products of this action by showing that, according to the degrees of concentration of the acid employed and temperature reached, the products are a mixture in variable proportions of picric acid, and the diazodinitrophenol of Greiss. When much nitrous fume is evolved the latter substance is the principal product.

An important communication "*On Chloranil.*—Part I.," by the same author, was then read. In following the best method described for the preparation of chloranil from phenol, that of A. W. Hofmann,—the substance is never produced in the theoretical proportion, but by superadding a treatment with chloride of iodine, the red oil and terchlorquinone become almost completely converted into chloranil, which can then be purified by solution and crystallisation from warm benzol. The action of hydrochloric acid and chlorate of potassium upon picric acid gave but a small proportion,—one-eighth of the theoretical quantity—of chloranil, the chief product being chloropicrin. For the preparation of Städeler's chlorhydranil Dr. Stenhouse directs that the chloranil should be treated with moderately strong hydriodic acid, and about one-tenth by weight of ordinary phosphorus for about half an hour, the product being washed with cold water, and crystallised from boiling alcohol. By the action of sulphurous acid on chloranil suspended in boiling water, the author remarked that other products besides chlorhydranil were formed. Free acids (sulphuric and hydrochloric) remained in solution, which were separated or neutralised with carbonate of lead, and sulphuretted hydrogen passed through the liquid precipitated the heavy metal, and left still in solution an organic compound, which was recovered by evaporation to dryness and sublimation from a paraffin bath heated to 120° C. Lustrous crystals were thus obtained which proved on analysis to be terchlorhydroquinone $C_6Cl_3H_3O_2$. This treated with nitric acid furnished terchlorquinone $C_6Cl_3HO_2$. A modification of this process will, it is expected give the bichlor- and chlor-quinone in a state of purity. The author concludes with some observations on terchlorquinone and the bromo-derivative, which, not yet analysed, is believed to be $C_6Cl_2BrO_2$.

Time did not permit of the reading of the next paper, but Mr. Chapman made a short verbal statement of its contents. It is entitled, "*Action of Zinc Ethyl on Nitrous and Nitric Ethers*," by E. T. Chapman and Miles H. Smith. The authors compared the action of zinc ethyl to that of metals, and show that when operating upon nitrites the residual products are nitrogen, nitric oxide, or the intermediate oxide N_2O. When acting with sodium upon nitrite of amyl a black substance is formed, which has but a transitory existence, and is believed to be the nitride of sodium, NNa_3. Undiluted zinc-ethyl attacks nitrite of amyl with great violence, bursting into flame when placed in contact with it, but if previously diluted with ether its action may be controlled, and gives rise to the production of nitric oxide gas and a honey-like solid mass believed to have the following composition:—

$$\left.\begin{matrix} ZnC_2H_5 \\ C_5H_{11} \end{matrix}\right\} O + \left.\begin{matrix} C_2H_5 \\ C_5H_{11} \end{matrix}\right\} O.$$

This decomposed by water gives hydrated oxide of zinc, hydride of ethyl, and amylic alcohol. If the zinc ethyl be used in excess, there is formation of Frankland's dinitroethylate of zinc. In proof of this the barium, and subsequently the copper salt, were prepared and analysed. When only a small proportion of ether was employed to moderate the action, the following change occurred:—

$$N''' \left\{\begin{matrix} O'' \\ OC_5H_{11} \end{matrix}\right. + 3\left(\frac{Zn}{2}C_2H_5\right) = \frac{ZnO}{\frac{Zn}{2}OC_5H_{11}} + \\ + N(C_2H_5)_3.$$

The triethylamine was recognised by its power of neutralising acids, and of giving by limited oxidation only acetic acid, with consumption of the proper amount of oxygen. The reaction between pure zinc ethyl and nitrite of amyl was characterised as giving a mixture, which, when heated only to 40°C., suddenly exploded with a degree of violence unsurpassed by any fulminating compound of which the authors had previous experience.

The Secretary gave notice that on the next occasion, 19th instant, Mr. Chance will deliver a lecture "*On the Manufacture of Glass;*" and that Professor Kolbe, who will then be in London, will give a short account of "*The Direct Transformation of Carbonate of Ammonia into Urea.*"

A vote of thanks was passed to the authors of the several communications read, and at an unusually late hour the meeting was adjourned.

Thursday, March 19th.

DR. WARREN DE LA RUE, F.R.S., &c., *President, in the Chair.*

THE minutes of the previous meeting were read and confirmed, and the donations to the library were announced.

The following gentlemen signed the statute book, and were formally admitted as Fellows of the Society, viz., Professor Hermann Kolbe, of Leipsig; Dr. B. H. Paul, Messrs. Herbert M'Leod, J. R. Carulla, and Peter Griess.

Lieutenant Francis C. H. Clarke, Royal Artillery, Staff College, Farnborough, was proposed for election, and the names of the following gentlemen were read for the second time:—John Tyndall, LL.D., F.R.S., &c., Royal Institution of Great Britain; Frederick Guthrie, Ph.D., F.R.S.E., Lecturer on Chemistry at the Royal College of Mauritius; William Brantingham Giles, Old Swan Borax Works, Liverpool. The ballot was taken for the following gentlemen, all of whom were declared unanimously elected: Mr. R. Calvert Clapham, Walker Alkali Company's Works, Newcastle-upon-Tyne; Dr. Rustomjee Byramjee, Assistant-Surgeon in Her Majesty's Bombay Army; and Dr. Meuzel, who was elected an associate.

The names of officers and members of Council proposed for election at the anniversary meeting on Monday, 30th inst., were suspended in the meeting-room.

Professor Kolbe was then invited by the President to favour the Society by giving an account of his experiments on the "*Conversion of Carbonate of Ammonia into Urea.*" The statement made by Professor Kolbe had reference to the production of artificial urea by heating in sealed tubes dry carbonate of ammonia to a degree of temperature a little lower than that at which the urea formed would be again destroyed. The speaker likewise referred to the electrolysis of acetic acid which furnished a new acid isomeric with glycolic acid, but of which the properties were as yet but imperfectly known.

Dr. FRANKLAND briefly interpreted Professor Kolbe's remarks, and alluded to the interest attaching to the investigation of the new isomer of glycolic acid, which might possibly throw light upon the question of the equality of the value of the four bonds of carbon in these bodies.

Mr. HENRY CHANCE, M.A., then delivered a most interesting lecture "*On the Manufacture of Glass,*" an account of which is unavoidably postponed until next week. The author briefly sketched the history of this manufacture, and quoted several analyses of various kinds of glass. The action of heat in causing devitrification, and of sunlight as affecting the colour, besides other considerations having reference to permanence, were discussed. Mr. Chance appended to his remarks upon glass a statement of his mode of treating the Rowley Rag basaltic rock of South Staffordshire. This material gives by fusion a black obsidian-like glass, which again devitrified furnishes a material suitable for building purposes, and capable of ornamental application. The formation of soluble silicate of soda by Gossage's process was described, and some of the corroded flints exhibited. An excellent series of samples illustrative of the manufacture of glass, and of the two materials producible from Rowley Rag, were laid on the table for inspection.

A short discussion followed, in which Drs. Frankland, Williamson, Miller, Hugo Muller, and Messrs. Dallmayer, Church, and David Forbes, took part. The accommodation afforded by the meeting-room was as usual on this lecture evening taxed to the utmost. After votes of thanks had been passed both to Professor Kolbe and Mr. Chance, and Messrs. Duppa and Mills had been appointed auditors, the meeting was adjourned until the anniversary on Monday, 30th inst., when the President and Treasurer would present their reports.

At the next ordinary meeting, on Thursday, April 2nd, Professor Church will read a paper entitled, "*Chemical Researches on some New and Rare Cornish Minerals;*" and there will be other papers, by Messrs. Perkin and Duppa, "*On the*

[English Edition, Vol. XVII., No. 432, page 129; No. 434, pages 152, 153.]

Constitution of Glyoxylic Acid," and by Dr. Odling, "*On Glyoxylic Amide.*" On the second ordinary meeting in April, Professor Guthrie will read a paper "*On Graphic Formulæ.*"

ROYAL SOCIETY OF EDINBURGH.

Relation of the Chemical Constitution and Physiological Action of Medicine.—Addition of Iodide of Methyl to Vegetable Alkaloids.

At one of the recent meetings of the Royal Society of Edinburgh, a very interesting paper was read by Drs. Crum Brown and T. R. Fraser, upon the influence of direct chemical addition upon the physiological action of substances. This paper is the first of a series which may be expected to throw great light upon one of the most interesting questions which can suggest themselves; viz., the relation existing between the chemical constitution and the physiological action of medicinal and poisonous substances. That such a relation must exist, we can have no doubt; and, indeed, attempts have been made by some to establish the relation in certain cases. Hitherto, however, the subject has not received that systematic investigation which it is now receiving at the hands of the authors of the paper.

In order to arrive at any accurate knowledge as to the influence which chemical constitution exerts upon physiological action, it would appear to be desirable to take substances having a very definite and energetic physiological action, and then to perform upon them a chemical operation, having for its object the promotion of a definite change in the constitution, and to examine the modification which the physiological action has undergone. Such has been the plan which the authors have pursued; the bodies which they have chosen for examination are the more active of the vegetable alkaloids, and the chemical operations, of which they have studied the effect, has been the direct addition of iodide of methyl. It was shown by How that, when iodide of methyl acts upon strychnia, brucia, morphia, and other alkaloids, it adds itself to them, and beautiful crystalline bodies are produced which differ considerably in character from the salts of the alkaloids. The authors have already examined the physiological action of the bodies produced by the addition of iodide of methyl to strychnia, brucia, morphia, thebaia, codeia, and nicotia.

The iodide of methyl-strychnium is prepared by first treating finely pulverised strychnia with a solution of carbonate of potash in dilute alcohol, and then adding an excess of iodide of methyl mixed with about its own volume of rectified spirit, and digesting in a flask for twenty-four hours. The spirit is thereafter distilled off, the residue dissolved in water, and crystallised. It is well known that doses of strychnia, varying from one-twentieth to one-thirtieth of a grain, rapidly produce in rabbits most violent convulsions, and in a few minutes kill the animal; the phenomena produced being due to a localisation of its action on the cord. It was found that twelve grains of iodide of methyl-strychnium, when administered (by subcutaneous injection) to rabbits weighing three pounds, produced no effect whatever. Fifteen grains produced symptoms, and twenty killed; but the animal died with symptoms altogether different from those produced by strychnia. In place of violent and spasmodic convulsions and muscular rigidity, the appearances were those of paralysis with complete general flaccidity. The spinal motor nerves were either paralysed, or speedily became so; and, instead of the speedy occurrence of muscular rigidity, the muscles remained flaccid, contractile, and alkaline for several hours. In short, by the addition of iodide of methyl to strychnia, the toxic properties of the latter are diminished about 140 times; and the body produced possesses the physiological action of curare; viz., paralysis of the end-organs of the motor nerves.

Similarly, Brown and Fraser have discovered that the toxic properties of brucia, thebaia, and codeia are immensely diminished by the addition of methyl; and that the bodies produced, instead of being, as all three of these alkaloids are, strongly convulsent, possess, on the contrary, the physiological action of curare. Morphia, as is well known, possesses both soporific and convulsent properties; its toxic action is much diminished by the addition of iodide of methyl; its convulsent action is destroyed, but its soporific action remains. The above are amongst the chief results which have been obtained by the authors, and appeared to possess such interest as to warrant my drawing the attention of your readers to them.—*British Medical Journal.*

PHARMACEUTICAL SOCIETY.

Wednesday, March 4th.

H. Sugden Evans, Esq., *Vice-President, in the Chair.*

The minutes of the preceding meeting were read and confirmed.

Specimens of crab oil, cayenne pepper in olive oil, and other interesting drugs from British Guiana and New South Wales, were presented to the museum by P. L. Simmonds, Esq.

The **Chairman** directed attention to a spurious jalap, from New York, 16 bales of which had been offered at a public sale, but was not purchased. He thought it was the rose-scented jalap, and previous to the meeting he had shown it to Professor Bentley, who thought it bore a great resemblance to the specimen of rose-scented jalap in the museum.

Mr. D. **Hanbury**, F.R.S., who read a paper a short time since "*On the Cultivation of Medicinal Plants*," mentioned that he very recently dug up a root of jalap from his father's garden, at Clapham, which was planted last June twelvemonth. It had remained in the open ground during the winters of 1866 and 1867. One tuber had produced six large tubers and twenty-four small ones. He thought it grew better in the open air than under glass. Unfortunately the flowering was too late for the seeds to ripen. This circumstance goes far to prove that jalap might be cultivated in Europe with ordinary attention.

In reply to the Chairman and Mr. Morson, Mr. Hanbury said that he was not prepared to speak of the properties of the jalap, not having tested it sufficiently.

Mr. **Umney** had examined the specimen of jalap from New York, on the table, and thought it differed from the rose-scented jalap in the museum: it possessed a peaty odour which the other did not.

Professor **Attfield** then read a paper "*On the Analysis of the Water of a Remarkable Medicinal Spring in Jamaica*," which he had received for analysis in May, 1867. It had been sent from Jamaica with the statement that thousands of the negroes had for weeks flocked to the spring, thinking it was a cure for all diseases. It was clear, inodorous, and strongly alkaline to the taste, its specific gravity being 1026·6. An imperial gallon contained 2493½ grns. of solid matter, which is about the average amount of saline compounds in sea water, but the author thought that spring water containing so much mineral matter had never been known. The constituents in one gallon were:—

Chloride of calcium	1510·00 grs.
Chloride of sodium	981·00 "
Chloride of ammonium	2·43 "
Water	69368·57 "

So that a gallon contained about 3½ oz. of chloride of calcium, 2 oz. of salt, and 2½ grs. of chloride of ammonium. Dr. Attfield had tested it for sulphates, nitrates, carbonates, potassium and magnesium salts, bromides, iodides, fluorides, sulphides, phosphates, nitrites, silicates, borates, and a number of other salts, but found none, and animal and vegetable matter were also absent. The proportion of chloride of calcium he believed to be unprecedented. There was another spring in Jamaica of a thermal character, which contains 105 grs. of chloride of calcium to the gallon; the saline and chalybeate water of Harrowgate contains between 120 and 130 grs. of chloride of calcium; and the water of the Dead Sea, 25 per cent. of which was stated to be solid matter, also contained a little chloride of calcium. The gases dissolved in the water were small in amount—one gallon contained 3·33 cubic inches of nitrogen, 1·55 of oxygen, and ·50 of carbonic acid. In sending the result of the analysis to Jamaica, Dr. Attfield asked the proprietors of the estate to send him some information on the history of the spring and topography of the district, thinking it would be of geological as well as chemical and physiological interest. From the report he had received it would appear that the water had been used for medicinal purposes upwards of forty years. The negroes believed it a cure for every disorder, but it was chiefly used for scrofulous affections, glandular swellings, &c. The author quoted from Pereira's statement of the therapeutic action of chloride of calcium, which shows that it is most useful in those diseases for which the negroes have recourse to the medicinal water. The spring is 68 ft. above the sea level, and 76 chains from it; temperature, 82°F.; and it makes its appearance in the diluvial gravel that nearly fills a small brook known as the Saint Ann's Great River. Dr. Attfield presumed it was of volcanic origin.

The **Chairman** said they were very much obliged to Dr. Attfield for his paper, which contained a number of points of great interest.

Mr. C. H. **Wood** suggested that it might be used to water the road, as an artificial water was manufactured for that purpose.

Mr. C. H. **Wood** read a paper "*On the Syrup of Hypophosphite of Iron*," in which he referred to the disadvantages of the processes given in the *Pharmaceutical Journal* vol. vii. p. 440) and *Parrish's American Pharmacy*. He had made a good syrup, containing 2 grs. of hypophosphite of iron in 1 dram, by dissolving 480 grs. of granulated sulphate of iron in 1 ounce of dilute phosphoric acid and 1¼ ounce of distilled water, then reducing 326 grs. of pure hypophosphite of lime to fine powder, adding to it the solution of sulphate of iron, and triturating before transferring to calico. The liquid was then pressed out, filtered, and mixed with seven times its volume of simple syrup.

The **Chairman** thanked Mr. Wood for his practical paper, and

Mr. **Umney** referred to a process for the preparation of the syrup which contained 1 gr. of hypophosphite of iron to the dram, but he thought Mr. Wood's process was an improvement.

Dr. **Attfield** asked Mr. Wood if he had experienced any danger in preparing the hypophosphite of lime. He had heard of an explosion occurring, even when it was dried at a low temperature, 130 to 140°.

Mr. **Wood** had not attempted to make any quantity, on account of phosphoretted hydrogen being evolved, which would prove an annoyance to the neighbourhood.

Professor **Redwood** had never heard of an explosion taking place in the preparation of hypophosphite of lime, but in preparing hypophosphite of soda, it was necessary to dry at a very low temperature, to prevent an explosion.

Professor **Attfield** described a laboratory experiment relating to "*Magnetic Hydrate of Iron*." He had added an alkali to a solution of ferrous and ferric sulphate, in molecular proportions, and obtained the usual black hydrate of iron having the well-known property of being attracted by a magnet, even when the latter was simply immersed in the mixture. He then precipitated appropriate quantities of ferric hydrate and ferrous hydrate in separate vessels; neither of the precipitates was attracted by a magnet. The contents of the vessels were then well mixed, when a hydrate resulted, which at first was not at all magnetic, feebly attracted after ten minutes, its attractability slowly increasing until, after twenty-four hours, it appeared to be more strongly attracted than the black hydrate made in the usual way.

This experiment showed, first, that in making magnetic oxide of iron for use in medicine, fuel need not be wasted in obtaining ferric sulphate entirely free from the nitric acid used in its preparation, for the ferric solution could be poured into the alkali before the ferrous, any nitric acid thus becoming neutralised and prevented from oxidising the ferrous salt; second, it afforded confirmation, were any needed, of the view that magnetic hydrate was a compound and not a mere mixture of ferrous and ferric hydrates; third, it was a good illustration of the influence of time in chemical change.

Dr. **Redwood** thought that by boiling the solution, one would obtain a more certain and definite preparation.

The **Chairman**, after thanking Dr. Attfield for his communication, announced that the next meeting would be held on the 1st of April, when a paper would be read by Mr. C. H. Wood, and several laboratory experiments would be described.

The meeting was adjourned at an early hour.

GLASGOW CHEMICAL SOCIETY.

On Monday evening last, a meeting, embracing a large representation of the chemists of Glasgow, was held in the Philosophical Society's hall, for the purpose of forming a local Chemical Society. In the absence, from illness, of Dr. Anderson, Professor of Chemistry in the University,

Wm. McAdam, Esq., of Hyde Park Pottery and Bottle Works, occupied the chair, and briefly stated the object of the meeting, and then called upon

Mr. E. C. C. **Stanford**, F.C.S., manager of the British Seaweed Company, who explained at some length the character of the proposed Chemical Society. He stated that the want of such a Society had long been felt among the chemists of Glasgow, and that as the Philosophical Society embraced in its membership many chemists, it had been suggested that a chemical section should be formed in the Society in accordance with the Society's rules. To this proposal the council of the Philosophical Society had readily agreed, and in order that the meetings and other privileges of the members might be open to other persons pursuing chemical studies, or engaged in chemical manufactures, they had resolved that associates should be admitted on payment of an annual subscription of five shillings, and that they should thereby be entitled to consult the Society's valuable reference library. Mr. Stanford also mentioned that he had received replies from about eighty members of the Philosophical Society, expressing approval of the projected chemical section. It was then resolved that such an organisation of the chemists of Glasgow should be formed, and a batch of eighteen associates were formally proposed and admitted, on the motion of Mr. James Mactear, F.C.S., of St. Rollox Chemical Works.

The night of meeting having been resolved upon, and Monday, the 6th of April, being fixed for the first formal meeting of the Society,

Mr. **John Mayer**, F.C.S., Government Science Teacher, proposed, and Mr. St. **John Vincent Day**, C.E., seconded, a motion—"That a council of eighteen members be elected to conduct the business of the Society, and that the council consist of a president, two vice-presidents, a secretary, and a treasurer, and thirteen ordinary members." The motion was unanimously agreed to, and in accordance with it the election was then proceeded with. The following are the names of the office-bearers:—

President: Dr. Thomas Anderson, F.R.S.E., Professor of Chemistry. *Vice-Presidents*, Dr. Wallace, F.R.S.E., Analytical Chemist; Mr. E. C. C. Stanford, F.C.S., Manager British Seaweed Company. *Treasurer:* Mr. Alexander Whitelaw, F.C.S., Soap Manufacturer. *Secretary:* Mr. R. Tatlock, F.C.S., Analytical Chemist. *Some of the other Members of Council:* Messrs. Ex. Baillie Harvey, Govanhaugh Dyeworks; James Napier, F.C.S., Chemical Manufacturer; William McAdam, Hyde Park Pottery; James Mactear, F.C.S., Alkali Department, St. Rollox Chemical Works; St. John Vincent Day, C.E.; John Mayer, F.C.S., Government Science Teacher: John Jex. Long, Lucifer Match Manufacturer; John E. Poynter, Chemical Manufacturer.

We have every reason to expect that this new Chemical Society will be of much service in extending a knowledge of theoretical and practical chemistry. It would indeed be strange were it to prove otherwise, considering the chemical antecedents of the City of Glasgow, and the manufacturing district of which it is the centre. The class-rooms and the laboratories of Glasgow are indissolubly associated with the names of Drs. Joseph Black, Thomas and Dundas Thomson, Thomas Clark, Birkbeck, Ure, Anderson, Penny, Professor Graham, Lyon Playfair, James Young, Greville Williams and many others; and the chemical manufacturers of the Glasgow district furnish names not less famous in the history of chemistry. As evidence, there are the late Walter Crum; J. B. Neilson, of the hot-blast; Charles Tennant, the first manufacturer of bleaching-powder; Mushet, of the black-band ironstone; Macintosh, of waterproofing fame, &c.

We shall keep our readers apprised of anything that is novel, interesting, and important in the communications that may be made from time to time to the members of the Society, whose birth we have now put on record.

CHEMICAL NOTICES FROM FOREIGN SOURCES.

Carbonic Oxysulphide.—C. Tran. This gaseous compound is formed; 1. By direct union of carbonic oxide with sulphur, at a low red heat; 2. By the action of dilute acids upon sulphocyanhydric acid:

$$\Theta SNH + H_2\Theta = NH_3 + \Theta S\Theta.$$

By making use of the latter reaction large quantities of the new compound may easily be prepared. Powdered potassic sulphocyanide is added to a mixture, by volume, of 5 sulphuric acid and 4 of water (in such proportions that the mass remains liquid), and the evolution of gas which sets in at once kept constant by alternately heating and cooling. The gas is purified from traces of cyanhydric acid (and formic acid), carbonic disulphide, and aqueous vapour, by letting it pass successively over mercuric oxide, non-vulcanised india-rubber, and calcic chloride. It may be collected over mercury when dry, without suffering change; water dissolves an equal volume, and gradually decomposes it. Its temperature of ignition is very low; it burns with blue flame; the products of combustion being carbonic anhydride, and sulphurous acid. Alkalic hydrates absorb it with formation of carbonate and sulphide. Its sp. gr. was found = 2·10.—(*Ann. Chem. Pharm.* 5 suppl. 236.)

Isomeric Compounds derived from Benzoic Acid.—H. Hübner and F. Mecker. The bromnitrobenzoic acids obtained from brombenzoic acid by the action of very strong nitric acid, at a moderate temperature, are best separated by extracting the mixture of acids repeatedly with insufficient quantities of boiling water, until the residue has become insoluble and infusible under water. (Those acids, derived from the insoluble portion, fusing at 248°C., are named *a*; those from the soluble, fusing at 140° *β* compounds.) *β* bromamidobenzoic acid,

$$\Theta_7H_4Br(NH_2)\Theta_2,$$

obtained by reduction of the corresponding nitro-compound with tin and chlorhydric acid in the proportions shown in the following equation:—

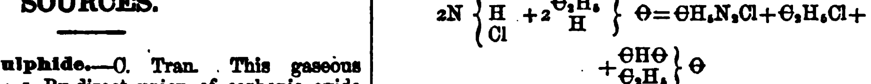
$$\Theta_7H_4Br(N\Theta_2)\Theta_2 + 6Sn + 6HCl = 6SnCl + 2H_2\Theta \\ + \Theta_7H_4Br(NH_2)\Theta_2$$

crystallises in small needles, and fuses at 171° to 172°. *β* bromamidobenzoic acid, $\Theta_7H_3Br(NH_2)\Theta(\Theta H)$, crystallises in long needles, which are sparingly soluble, and fuse at 202° to 204°. *a* and *β* amidobenzoic acid, $\Theta_7H_5(NH_2)\Theta_2$ and $\Theta_7H_4(NH_2)\Theta(\Theta H)$, which are obtained from their respective bromnitro-compounds by employing the following proportions:—

$$\Theta_7H_4Br(N\Theta_2)\Theta_2 + 8Sn + 7ClH = \Theta_7H_5(NH_2)\Theta_2 + 7SnCl \\ + SnBr + 2H_2\Theta$$

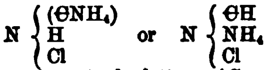
have been found identical so far as experiments at present went, but isomeric with ordinary amidobenzoic acid.—(*Zeitschr. Chem.* N. F. iii. 564.)

Chlorhydrate of Cyanhydric Acid.—Arm. Gautier. Dry cyanhydric acid is saturated with chlorhydric acid gas at −10° C., and then heated in a closed vessel to 35°—40°. On again cooling the liquid solidifies to a white crystalline mass which is cyanhydric chlorhydrate $\Theta NH + ClH$. This compound decomposes very readily, is soluble in water, alcohol, and glacial acetic acid, insoluble in ether. Sulphuric acid expels chlorhydric acid with formation of sulphate. When heated with glacial acetic acid to 150° or 160° complete decomposition takes place, amongst the products of which formylamide and acetamide were found. When decomposing in presence of alcohol the chlorhydrate of a new base is formed together with ethylic chloride and formic ethide.



$$2N\left\{\begin{matrix}\Theta H\\ H\\ Cl\end{matrix}\right. + 2\left.\begin{matrix}\Theta_2H_5\\ H\end{matrix}\right\}\Theta = \Theta H_5N_2Cl + \Theta_2H_5Cl + \\ +\left.\begin{matrix}\Theta H\Theta\\ \Theta_2H_5\end{matrix}\right\}\Theta$$

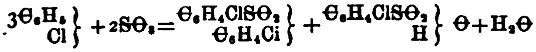
The base cannot be isolated with potassic hydrate, on account of its splitting up into ammonia and formic acid the moment it is set free. The chloroplatinate has the formula $2(\Theta H_5N_2Cl)PtCl_4$. This chlorhydrate is isomeric or perhaps identical with the compound obtained by the addition of chlorhydric acid to ammonic cyanide, and its constitution is represented by the formula—



$$N\left\{\begin{matrix}(\Theta NH_4)\\ H\\ Cl\end{matrix}\right. \quad \text{or} \quad N\left\{\begin{matrix}\Theta H\\ NH_4\\ Cl\end{matrix}\right.$$

preference being given to the latter.—(*Comptes R.* lxv. 410 and 472).

Dichlorsulphobenzid.—R. Otto has not been able to confirm Gerike's statements as to the formation of dichlorsulphobenzid, by the action of chlorine or phosphoric chloride upon sulphobenzid, the products of this reaction being, according to the author, chlorbenzol and sulphobenzolic chloride. He, however, succeeded by acting upon monochlorbenzol with sulphuric anhydride at a low temperature. Dichlorsulphobenzid is formed according to the following equation:—

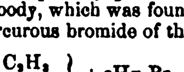


$$3\left.\begin{matrix}\Theta_6H_5\\ Cl\end{matrix}\right\} + 2S\Theta_3 = \left.\begin{matrix}\Theta_6H_4ClS\Theta_2\\ \Theta_6H_4Ci\end{matrix}\right\} + \left.\begin{matrix}\Theta_6H_4ClS\Theta_2\\ H\end{matrix}\right\}\Theta + H_2\Theta$$

This compound crystallises in white needles, fusing at 140°—141° C., insoluble in water, readily soluble in hot alcohol or ether. It is not decomposed by an alcoholic solution of potassic hydrate. Sodium-amalgam reduces it principally to benzol and sulphobenzolic acid.—(*Zeitschr. Chem.* N. F. iii., 609.)

Trimethylamine in Wine.—White Austrian wine, according to E. Ludwig, contains trimethylamine, which the author isolated in the following way:—The wine was freed from alcohol by distillation, then again distilled with a solution of sodic hydrate, the alkaline distillate neutralised with sulphuric acid, and evaporated. The residual salts, containing much ammonic sulphate were extracted with absolute alcohol, the alcoholic solution evaporated, and the residue again distilled with sodic hydrate. From this distillate, the platino-chloride was prepared and identified as that of trimethylamine.—(*Akad. z. Wien.*, 56, 1857.)

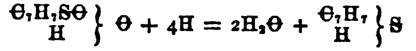
Derivatives of Vinyl-Compounds.—Glinsky has studied the reaction between vinylic bromide and mercuric oxide, or hypochlorous acid (suggested by an analogous investigation by Linnermann, *vide* Chemical News, No. 428, p. 83 [*Amer. Repr.*, *April*, '68, *page* 191] and obtained aldehyde, and a white amorphous body, which was found to be a compound of aldehyde and mercurous bromide of the formula



$$\left.\begin{matrix}C_2H_3\\C_2HO_2\end{matrix}\right\}+2Hg_2Br.$$

—(*Zeitschr. Chem.*, N. F. iii., 675.)

Toluolsulphurous Acid.—R. Otto (second communication). The action of phosphoric perchloride upon toluolsulphurous acid gives rise to the formation of sulphotoluolic chloride, fusing at 68—69° C., and an oily body not obtained in sufficient quantity for examination. Nascent hydrogen forms metabenzylic sulphydrate:



$$\left.\begin{matrix}\Theta_7H_7S\Theta\\H\end{matrix}\right\}\Theta+4H=2H_2\Theta+\left.\begin{matrix}\Theta_7H_7\\H\end{matrix}\right\}S$$

identical with that of Märker (*Ann. Chem. Pharm.*, cxxxvi., 75). When heated with water to 120–130° toluolsulphuric acid and oxybenzoldisulphide, $\Theta_{14}H_{14}S_2\Theta_2$ is obtained. Potassic hydrate at 250–300° causes it to split into sulphite and toluol. Fuming nitric acid forms the compound $\Theta_{21}H_{22}N_2S_3\Theta_6$, which may be looked upon as diazotrisulphotoluolic hydride, and besides this, nitrosulphotoluolic acid.—(*Zeitschr. Chem.*, N.F. iii., 600).

Oxidation of Benzol.—L. Carius. Benzol is readily oxidised by a mixture of manganic dioxide and sulphuric acid (5 acid, 1 water). Amongst the products of the oxidation were found benzoic acid, and a new diabasic acid of the composition $\Theta_6H_4\Theta_3$—oxybenzenic acid, *i.e.*, benzenic acid + 1 atom of oxygen. Oxybenzenic acid is sparingly soluble in water, fuses at 175° C., and distils undecomposed at 250°. The author considers it extremely probable that this acid is identical with phtalic acid, their composition, as found by analysis, differing but little, and fusion-point (phtalic acid, according to C., fuses at 175°, not 120°) and solubility being the same with both. The formation of benzoic acid in this case is of interest as being the first in which, by simple oxidation of a hydrocarbon, an acid richer in carbon is obtained; this is due evidently to the presence of formic acid, which is formed during a certain stage of the reaction:

$$\Theta\Theta,H,\Theta H+\Theta_6H_5H=\Theta\Theta,\Theta_6H_5,\Theta H+\Theta H_2.$$

—(*Ibid.*, N.F. iii., 629.)

Derivatives of Mesitylene, containing Sulphur.—A. Holtmeyer. Mesitylensulphuric chloride, $\Theta_9H_{11}S\Theta_2.Cl$, is formed by the action of phosphoric perchloride on sodic mesitylensulphate. It dissolves in ether and alcohol, is insoluble in water, fuses at 57° C. Sodium amalgam converts it into mesetylin sulphurous acid, $\Theta_9H_{11}S\Theta_2.H$. soluble in alcohol and hot water, sparingly so in cold water, fuses at 98—99°. Mesitylensulphydrate $\Theta_9H_{11}SH$ is obtained by adding the chloride to a heated mixture of zinc and sulphuric acid; it is a liquid boiling at 228—229°, soluble in alcohol, ether, or benzol, insoluble in water. The sulphydrate may be converted into mesitylendisulphide by adding to its alcoholic solution aqueous sodic hydrate. The disulphide fuses at 125°, its solubility in alcohol, &c., is the same as that of the sulphydrate.—(*Ibid.*, N.F. iii., 686.)

Reactions of Anisic Aldehyde.—Saytzeff and Samosadsky. If anisic aldehyde in alcoholic solution is digested with sodium amalgam two crystalline bodies are obtained. The one fuses at 172° C., is little soluble in cold, readily in hot alcohol or ether. Its composition is $\Theta_{16}H_{18}\Theta_4$, being intermediate between anisic aldehyde and alcohol; it seems to be the doubled aldehyde combined with 2H—analogous to the compound obtained by Hermann from benzoic acid with sodium amalgam. The second compound fuses at 125°, but otherwise resembles the first closely, and is supposed to be the doubled aldehyde—analogous to benzoin.—*Ibid.*, N. F., iii., 678.)

NOTICES OF BOOKS.

Boiler Deposits: On the Chemical Nature of the various kinds of Boiler Incrustations, and on some of the methods which have been proposed to remedy them. By Dr. T. L. Phipson.

In the form of a small pamphlet, the author reproduces the text of a paper read at a meeting of the Inventors' Institute in December last. The chemical nature and circumstances of formation of boiler deposits are fairly discussed, several proposed means of prevention described, and a few practical remarks appended which are likely to prove useful to engineers. In the manufacturing districts of Lancashire and of the midland counties many disasters must have been prevented by the system of periodical inspection carried out during the last few years by the so-called Boiler Associations; but much yet remains to be done towards extending their operation in new localities, and particularly in order to guarantee the safe working of boilers attached to portable engines, which are now so commonly placed in the hands of agriculturists. The greatly extended use of small steam lifts and cranes, and of boilers constructed around the flues of forges and reheating furnaces, appears likewise to increase the risk of danger from explosion—and many sad calamities have been reported during the past year. Apart from the question of personal safety, there is a wide margin for the exercise of economy in the working of a steam boiler, and Dr. Phipson assures us that the loss of heat, or of combustible, in a thickly-incrusted marine boiler, may amount to as much as 40 per cent. ! In all cases where an adherent mineral scale is permitted to form, the consumption of fuel for all constructions of boilers must necessarily be somewhat augmented, and the operation of chipping tends certainly to loosen the rivets and weaken the plates.

The composition of boiler deposits is considered under three heads, viz.: Fresh water deposits consisting chiefly of carbonate of lime; salt water deposits invariably composed of a mixture of sulphate of lime and hydrate of magnesia; and abnormal deposits containing incrusting matters derived from mine waters, canals, or streams contaminated with metallic salts and other kinds of refuse from chemical works. Impure waters of this class are known to be especially liable to induce corrosion of the boiler-plates, and a red deposit analysed by Dr. Phipson contained more than nine per cent. of peroxide of iron, due to the rusting of the metallic surfaces of the boiler. Two years ago Dr. Voelcker* gave an account of marine-boiler incrustation, and called attention to the large amount of magnesia contained in it. Dr. Phipson now appends a further analysis which, from its interest, is here subjoined:

Marine Boiler Incrustation.

Sulphate of Lime	65·00
Magnesia	19·00
Water	13·50
Oxide of Iron and Alumina	0·85
Chloride of Sodium	0·70
Sand	0·45
	99·50

The author proceeds to remark—"This analysis shows that marine incrustation consists chemically of an atom of sulphate of lime united to two atoms of hydrate of magnesia; and that the sulphate of lime is combined here with *one* atom of water, instead of two as in ordinary gypsum." This statement is not, however, in harmony with his own results (quoted above), which demand, on the contrary, the following formula:—

$$2(MgO,HO)+HO,2CaOSO_3.$$

It will be perceived that *dihydrated* sulphate of lime—a

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* Report of the British Association, 1865; p. 39.

compound described by the late Professor Johnston under the name of "boiler sulphate,"—is really deposited, together with the hydrate of magnesia, at the increased temperature of water boiling under considerable pressure.

Dr. Phipson then enumerates several of the ingredients which have been proposed as means of prevention. Of boiler compositions which act only mechanically, the author mentions scrap-iron, clay, starch, and potato-parings. A varnish for coating the interior of boilers, is compound of equal parts of black-lead and suet, with small quantities of charcoal-dust and coal-tar oil. As an objection to this lacquer, it is said to require frequent renewal. Professor Chandelon, of Liège, recommends a mixture of—

Bullock's blood	5 parts.
Starch	2 "
Carbonate of soda	2 "

The use of sal-ammoniac is condemned on account of its rusting the plates. The most efficacious remedies are the carbonate and hydrate of soda, and the author proposes to feed the boiler with water from which the lime and magnesia salts have been previously precipitated by addition of alkali to the contents of the supply tank. Dr. Phipson calculates that 24 grs. of the carbonate of soda, or 14 grs. of the hydrate, should be allowed for every gallon of water, and now that a sufficiently pure quality of the hydrate can be procured for 3d. per lb. (misprinted 3s.) the use of soda is recommended as being both effectual and economical with all kinds of fresh water; but for marine boilers so large a quantity would be required, that the author prefers to combine this alkaline treatment with the system of frequent "blowing off," whereby much of the calcareous refuse can be ejected in a sandy or granular form.

CORRESPONDENCE.

Nessler's Test.

To the Editor of the Chemical News.

Sir,—I have used the following modification of Nessler's process for the estimation of small quantities of ammonia, by which considerable expedition is gained without, as far as I am aware, sacrificing accuracy. It is based on the fact that a solution of iodine, dissolved in iodide of potassium, imparts to water a colouration very similar to that produced by the Nessler test with ammonia. Where the quantity of ammonia was small, say ·4 mlgrm. in 200 c.c. of water, I could perceive no difference in looking *down* the two cylinders standing on a white surface; the real difference, however, becomes quite apparent if viewed laterally. I conduct the process as follows:—A permanganate burette, divided into ·1 of c.c., and fitted with a float, is filled with a solution of iodine dissolved in excess of iodide of potassium; the strength is best determined by experiment.

Into one of the cylinders is put an amount equal to ·4 mlgrm. of NH_3, together with 200 c.c. of water, and the Nessler test, taking care to mix thoroughly. As soon as sufficient time has elapsed to allow of the colouration attaining its darkest shade, a second cylinder, containing a volume of water equal to that in the first, and to which has been added a little solution of iodide of potassium (1 to 10 of water) is placed under the burette, from which is delivered the solution of iodine, until a point occurs when, on looking *down* the cylinders, the shades of colour in each are judged to be alike. From this it follows that, provided the "tinctorial" power, and consequently "litre" of the ammonia standard be correctly ascertained in terms of the iodine solution, the former may be dispensed with.

This modification, it will be observed, obviates the numerous assays it is necessary to make when the ammonia standard is retained, besides saving no inconsiderable time in the execution of an analysis.—I am, &c.,

Philip Holland.

Chorley.

Water Analysis at the Chemical Society.

To the Editor of the Chemical News.

Sir,—During the discussion at the last meeting of the Chemical Society, Mr. Abel, the chemist to the War Department, took occasion to remark, in reference to our method of water analysis, that our paper describing it contained many things that were incorrect, and on my asking him to be kind enough to name some of those things that were incorrect, I could get no answer from him.

* * * * * *

I have made an examination of our paper, and find absolutely nothing which I can say is incorrect; the most that I find is a single expression in a parenthesis, which appears to be doubtful. We have used the word "usually" instead of "sometimes" in this parenthesis. The paper is distinguished by extreme accuracy and fidelity to the facts, but is in parts unartistic, and gives an undue degree of prominence to certain results. Owing to this circumstance, notwithstanding its accuracy, it is calculated to lead those persons into error who glance at a paper without reading it.

There is much misconception current respecting our method—far greater misconception than could possibly have arisen from this cause. Indeed, it seems as if the current notions about it have been derived, not from our paper describing it, but second-hand from the writings of our detractors. A *bon-mot*, by a Fellow of the Chemical Society when our method was brought out, will serve to characterise much of the causes which have produced these misconceptions:—"The worst fault of your water-analysis," said this gentleman, "is that it proceeds from you."

One of the most curious of these misconceptions is that we direct people to determine the urea in a water by taking a litre of it, adding carbonate of soda and distilling off 300 c.c., and then measuring the ammonia in the 300 c.c. of distillate.

Our directions given last June were, "go on distilling until no more ammonia can be detected in the distillate." So far from, in any way, leading people to believe that the first 300 c.c. will contain all the ammonia proceeding from the urea, we warn people against expecting to find it all there; we talk about the necessity arising for the addition of ammonia-free water, so as to fill up the retort, and finally we give examples where the urea-distillate reaches 600 c.c.

I understand our detractors to say that our process for the estimation of urea does not give the whole of the ammonia derived from it, but only a fraction of it. Such a notion is intelligible enough in conjunction with the belief that our method consists in seeking for urea-ammonia only in the first 300 c.c. of distillate. I understand that some of them are anxious to construe the remark made in my paper on "the verification, &c.," that the estimation of urea is not so sharp as the estimation of albumen, into a round-about way of saying that the decomposition of urea into ammonia is incomplete. Let them be undeceived. There is no failure of the reaction. The want of sharpness in the estimation is due only to a manipulatory difficulty, which might be got over if necessary. It arises from the division of the ammonia into too many portions, and the consequent accumulation of experimental errors.

Those gentlemen to whom the *bon-mot* is applicable, are jubilant over the fate of the determination of albumen. Say they, you promised us a complete conversion of the nitrogen of albumen into ammonia, and you give us some kind of an incomplete transformation. We have given them everything we promised, and much more too. If our detractors will not merely glance at our paper, but read it carefully, they will find a warning that the first third of the ammonia derived from albumen is only to be got when the potash treatment is pushed to dryness, and after getting that first third in that manner, the remaining two-thirds are to be had on using the permanganate. It was in that manner that the total ammonia was got from albumen.

In our paper, however, we say in effect, if you have to make a water analysis do not distil to dryness. When we wrote our paper last June, it was for us an undecided point whether or not the neglect to evolve the first third of ammonia by means of potash would involve the ultimate loss of it—whether, in fact, there would be more ammonia on the employment of the permanganate to make up for absence of ammonia during an imperfect potash treatment.

This question, although interesting on scientific grounds, and the answer to which will go a long way towards the disclosure of the rational constitution of albumen, was in my opinion of comparatively little consequence in reference to water analysis, and we adjourned the consideration of it to a more convenient season. In our opinion it did not matter at all whether the modification we recommended on the score of practicability involved the representation of albumen by a fraction instead of the whole of its ammonia. The essentials for water analysis were that the method should really cope with the infinitesimal quantities of organic matter existing in water, and that its indications should be reasonably parallel with the degree of badness of the water, and that it should be practicable. The possession of these qualities is characteristic of our method, and will, we believe, secure its general acceptance. The absence of these qualities is one of the most prominent features of the method which at the present moment is the rival of our own. —I am, &c., J. Alfred Wanklyn.

London Institution, March 9, 1868.

On the Reduction of Carbonic Acid to Oxalic Acid.

To the Editor of the Chemical News.

Sir,—It is due to ourselves, as well as to the chemical public, to throw some light on our relations to a discovery which is claimed by a chemist who worked with us in the same laboratory. It is the synthesis of oxalic acid from carbonic acid and potassium, a fact which is decidedly of great interest, as it shows the direct transformation of carbonic into oxalic acid, and is a new proof of the truth of the views which reign at present on the constitution of the latter body.

Occupied during the course of last summer with similar synthetical experiments, we were, by a very simple reflection, led to the idea of preparing directly oxalic acid from carbonic acid, an idea the execution of which was clearly pointed out by the successful experiments of Mr. Wanklyn on the formation of propionic acid.

The reflection that the group CO,HO, common to all organic acids, and already prepared synthetically in combination with alcohol radicals, could exist independently as a molecule, only doubled, that is to say, as oxalate of potassium, leads, consequently, to the synthesis of the acid.

We began our experiments in the Leipsic Laboratory, in presence of a considerable number of chemists, who took a lively interest in the proceedings of our research. Amongst them we observed Dr. Drechsel.

The first we tried was the action of liquid carbonic acid on potassium. We were not disturbed by the observation made by Dr. Drechsel on this occasion, that an English chemist had tried already the same experiment (executed only in order to state the solubility of potassium in liquid carbonic acid), without mentioning the formation of oxalic acid, for which he evidently was not looking.

The experimental difficulties, namely, the execution of the reaction in a Faraday tube, as well as the close of the laboratory by the holidays, obliged us to interrupt our research, the continuation of which was clearly indicated at the occasion of the lecture experiments on liquid carbonic acid with Natterer's apparatus in the course of the following winter.

It is easily explained that our experiments in that direction escaped the memory of our highly esteemed teacher, Professor Kolbe, who at this time was fully occupied by the new building of his laboratory, and by other official occupations, so that he, half a year afterwards, proposed to Dr. Drechsel the same research which had led us, six months ago, quite independently, without any impulse, to the abovementioned experiments.

* * * * * *

We are, &c., J. Wischin and Th. Wilm.

Berlin, March 8th, 1868.

The Royal School of Mines.

To the Editor of the Chemical News.

Sir,—"A Student at the Royal School of Mines" states in the Chemical News of last week (*American Repr., May*, 1868, *page* 197) that I am in error when I look upon the Royal School of Mines as an appendage of the Geological Museum (when I say the Geological Museum, I include the Geological survey). Notwithstanding what he has said, I still think that I am right in so regarding it. To support me in this opinion I will for a moment refer to the "pamphlet." On page 3, paragraph 1, we are told that the Museum and the School have arisen from the wants of the Geological Survey; this is substantially repeated in paragraph 5; and in the following paragraph we are distinctly told that the School "has grown out of, or engrafted itself upon, the Geological Survey." So that the Geological Survey is the trunk, and the School is one of its branches, and therefore an appendage.

I perhaps made use of too strong an expression when I said that the prospectus was printed on the spare pages of a pamphlet belonging to the Geological Museum; what I meant was that the School does not occupy the position of primary importance in it which it should; the accounts of the Geological Survey, of the Museum, and of the Mining Record Office, all take precedence of it; and this may be seen from a glance at the title-page,—thus, "Geological Survey of the United Kingdom, Museum of Practical Geology, and Royal School of Mines," whereas those departments, being quite distinct from the School, should have a prospectus of their own, while the School should have its calendar printed in a separate form. The pamphlet is neither wholly a prospectus nor a calendar, it is nothing but an economical makeshift, attempting to perform the functions of both; advertisements of maps occupy one cover, whilst the "Arrangement of Lectures," as if of no importance, is ignominiously consigned to the inside of the other. I am rather surprised that a student of the Royal School of Mines should have raised a discussion about such a triviality as this is when compared with the general welfare and position of the School.

I cordially agree with "Delta" in his remarks respecting the Associates, although I must say that the term "Associate" is often used as an honorary one, and is generally regarded as such. I think that the excellent course of study now marked out might be somewhat improved by adding mathematics to the list of subjects; and it would be a great help to the students in the Geological department were the course to include botany; both these subjects are taught in the Dublin School. I may perhaps suggest that the certificates of proficiency would derive an additional value were they to be signed by the Director, Sir Roderick Murchison, as well as by the Professor.—I am, &c. A. L. E.

Electrical Resistances.

To the Editor of the Chemical News.

Sir,—The following table of diameters and resistances of pure copper wires may not, perhaps, be unacceptable to electrical students and others.

The capacity of pure copper is taken at 100, and the unit of resistance used is the ohmad adopted by the Electrical Committee appointed by the British Association, one unit being equal to 1760 yards, or 1609·31 metres of copper wire ·2302 inch diameter.

The value of the table consists mainly in its being a standard by which the student is enabled to ascertain the conducting power of any wire he may have under test, and also of its giving relative lengths of pound and kilogrammes, and proportionate diameters in decimals of inch and millimetres.

It is, perhaps, unnecessary to say that there is no better apparatus for obtaining tests than the electric balance manufactured by Mr. Becker, of Messrs. Elliott Brothers:—

Diameter of inch $\left(d = \sqrt{\frac{c}{l}}\right)$	Diameter of m.m. (= d × 25·4)	Number of yards per lb. $\left(l = \frac{C}{d^2}\right)$	Number of metres in 1 kilo. (= l × 2·016)	Resistance in ohms. of pure copper (unit of length 1760 yds. or 1609·31 mtrs.)
·2302	5·847	2·095	4.223	1·00
·226	5·740	2·175	4·384	1·038
·198	5·029	2·834	5·713	1·352
·183	4·648	3·317	6·680	1·583
·175	4·445	3·628	7·314	1·731
·160	4·064	4·350	8·75	2 068
·136	3·454	6·007	12·11	2·867
·128	3·251	6·781	13·671	3·237
·107	2·717	9·705	19·555	4·623
·10	2·54	11·11	22·398	5·300
·092	2·336	13·125	26·46	6·266
·08	2·032	17·36	35·00	8·288
·07	1·778	22·67	45.71	10.82
·065	1·651	26·29	53·00	12·25
·0625	1·587	28·472	57·40	13·59
·06	1·521	30·864	62·223	14·73
·058	1·473	33·03	66·588	15 76
·056	1·422	35·432	71·431	16·91
·054	1·371	38·104	76·818	18·18
·052	1·32	41·091	82·839	19·61
·05	1·274	44·444	89·60	21·21
·048	1·219	48·225	97·222	23·02
·046	1·168	52·51	105·86	25·06
·044	1·117	57·39	115·70	27·39
·042	1·066	62 98	126·96	30·06
·04	1·016	69·444	140 00	33·14
·038	·965	77·16	155·50	36·72
·036	·914	85·766	172·91	40·92
·034	·864	95·29	292·70	45·48
·032	·813	108·5	218·74	51·79
·03	·762	123·46	248·90	58·93
·028	·711	141·72	285·71	67·65
·026	·660	164·36	331·35	78·46
·024	·609	192·9	380·26	92·08
·022	·558	229·56	462·80	109 58
·02	·508	277·78	560·01	132·59
·018	·457	342·94	691·36	163·69
·016	·406	434·03	875·00	207·17
·014	·355	569·51	1148·10	270·58
·012	·305	771·60	1555·50	368·30
·01	·254	1111·11	2239·80	530·35
·0095	·241	1231·10	2481·90	587·64
·009	·228	1371·7	2765.30	654·75
·0085	·216	1537·8	3100·20	734·05
·008	·203	1736·1	3500·00	828·67
·0075	·190	1975·3	3982·20	942·84
·007	·177	2267·6	4571·00	1082·4
·0065	·165	2629·9	5300·00	1225·3
·006	·152	3086·4	6222·30	1473·1
·0055	·139	3673·1	7404·90	1753·2
·005	·127	4444·4	8960·00	2121 4
·0045	·114	5487·0	11062·00	2619·0
·004	·106	6944·4	14000·00	3314·7
·0035	·088	9070·3	18285·00	4329·4
·003	·076	12346·0	24890·00	5892 7
·0025	·063	17777·0	35838·00	8485·6

The foregoing table is sufficiently comprehensive, embracing every size of wire likely to be used; but should the student have an intermediate size, he can obtain the diameter for himself as follows:—

$$d \text{ (diameter)} = \sqrt{\frac{C}{l}},\ l \text{ being the length in yards of}$$

lb., or $d = \sqrt{\frac{\cdot 111111}{28\cdot 472}} = \cdot 0625$, corresponding to one of the diameters in table. The value of C is 1-9th, or ·111111.

An easy method of obtaining the conducting power (P) of copper wire, assuming the experimentalist to have ascertained the accurate diameter by the above formula, is to test for resistance (r) by means of the balance above referred to, and the conducting power, $P = \frac{R}{r} \times 100$.

As a familiar example, one mile of ·0625 copper wire has, say, a resistance, r, = 16·5 ohms; the value of R in table = 13·59; hence $P = \frac{R}{r} \times 100 = \frac{13\cdot 59}{16\cdot 5} = 82\cdot 36$ per cent. of pure copper.—I am, &c.,

Walter Hall.

Telegraph Works, Mansfield Street, Borough Road, S.E.

Arsenious Acid.

To the Editor of the Chemical News.

Sir,—Seeing a notice in the Chemical News of last week (*American Reprint, May*, 1868, *page* 237), on the occurrence of prismatic arsenious acid, I beg to communicate a few observations made by myself in September last, on some peculiar crystals which I found in a flue from a calciner in which copper ores containing arsenic and sulphur are calcined.

The crystals were spear-shaped, not unlike marcasite in form; the lustre was pearly. They were deposited on small crystals (octohedral) of arsenious acid, and on taking them into the air I found that they began instantly to deliquesce.

They were readily soluble in a little hot water, with the exception of a trace of coal dust, mechanically mixed. On analysis, they were found to consist of—

Arsenious acid	75·10
Sulphuric acid	23 00
Sesquioxide of iron	1·25
Coal dust	trace

I imagine the iron to exist as a basic sulphate of sesquioxide.

Perhaps some of your readers would inform me whether such a compound as I have described has ever come under their notice. It is certainly quite new to me.—I am, &c.,

Richard Pearce.

Morfa Works, Swansea, March 17, 1868.

On the Reaction of Nitrites with Iodide of Potassium.

To the Editor of the Chemical News.

Sir,—In the last number of the Chemical News (*American Reprint, May*, 1868, *page* 213), Mr. Holland proposes to make use of the well-known reaction between nitrites, iodide of potassium, and sulphuric acid, to determine the amount of nitrous acid present in a water. The reaction referred to is certainly at first sight a most inviting one, and some time ago I made a few experiments to ascertain if a quantitative process could be procured from it; the results I then obtained convinced me that no accurate determination of nitrous acid could be founded upon it. The difficulty is that the reaction is a repeating one: that a minute amount of nitrite is capable (theoretically) of decomposing any quantity of hydriodic acid. I was led to this conclusion by the following experiments:—

To a weak solution of nitrite of sodium was added ten times as much iodide of potassium as there was nitrite present, and sulphuric acid exactly sufficient to decompose the iodide of potassium. After the addition of some starch paste, the iodine liberated was determined by a standard solution of hyposulphite of sodium. This determination of the iodine evolved, it was conceived, would afford the necessary datum for the estimation of the nitrous acid present. After decolourising the solution with the hyposulphite, the blue colour reappeared almost as strong as ever after a minute's standing. The solution was again decolourised, but the colour reappeared as before. I observed that the blue colour always commenced to show itself on the *surface* of the liquid, and thinking that the oxygen of the air was taking part in the reaction, I filled a test-tube to the brim with some of the decolourised liquid, and inserted a tightly-fitting cork, thus excluding air from a portion of the solution. Thus circumstanced, the blue colour did not reappear, while in the remainder of the liquid in the open beaker it was soon as dark as ever.

What is then the nature of the reaction of nitrites with hydriodic acid? Fresenius tells us* that nitrites, acted on by an acid, split up into nitrates and nitric oxide.

$$3(NaNO_2) + H_2SO_4 = NaNO_3 + Na_2SO_4 + H_2O + 2(NO).$$

Nitric oxide is without effect on hydriodic acid, but on contact with air it is at once converted into hyponitric acid (NO_2), and this decomposes hydriodic acid, *nitric oxide being reproduced.*

$$NO_2 + 2(HI) = NO + H_2O + I_2.$$

If, therefore, the nitric oxide were not slowly lost by diffusion into the atmosphere, a minute quantity of it would be sufficient to liberate an infinite amount of iodine. Here, I believe, is the reason of the extreme delicacy of the reaction: the minutest trace of nitrous acid in a water is sufficient, *in time*, to liberate a very distinct amount of iodine.

Mr. Holland says the solution is to be "allowed to stand until the colour is fully developed." Supposing the operator to wait till the greatest depth of colour is arrived at, the tint will depend (if an excess of hydriodic acid has been present) on the rate of diffusion of the nitric oxide into the atmosphere; this will be regulated by the temperature of the solution, the surface it exposes to the air, and by its state of concentration, as the gas will escape more rapidly from a strong solution than from a weak.

It appears then, I think, that the reaction in question does not afford the means for a satisfactory determination of nitrous acid.—I am, &c.,

R. Warington.

Contraction on Solidification.

To the Editor of the Chemical News.

Sir,—During the discussion on "devitrification," at the close of Mr. Chance's interesting lecture at the Chemical Society, Mr. Forbes mentioned that Mr. Chance, at his suggestion, compared the measurements of the wooden frames used for making the moulds for casting certain blocks of basalt, with the blocks so prepared when cold, and it was ascertained that the frames and blocks corresponded in size, from which it was inferred that molten basalt suffers neither contraction nor expansion on solidification.

A short time since, I made a few observations on the cooling, after fusion, of some six or seven crystalline substances, and I noticed that, with the exception of bismuth, they *contracted* on solidification. The contraction, however, did not manifest itself, generally, by making the solidified masses smaller than the interior of the vessels in which the substances cooled; but by the formation of cavities, sometimes of considerable size.

The experiments referred to, were made with comparatively very small quantities of the various substances, the fusing points of all being far lower than that of basalt; still, as there was a considerable difference in their fusing points as well as in the amounts employed, I think it extremely probable, were the blocks in question carefully examined, cavities would also be found in them, or on their surfaces, which, in conjunction with the facts already elicited, would, I believe, settle the interesting question whether or not contraction takes place during the solidification and devitrification of basalt.—I am, &c.,

Alfred Tribe.

149, Gt. Portland Street, March 23, 1868.

Estimation of Phosphates.

To the Editor of the Chemical News.

Sir,—As this is the season when so many persons are interested in the manufacture and composition of artificial manures, perhaps the following popular explanation of Mr. Burnard's process will serve as an answer to several of your correspondents and readers, and will be of use to any intelligent chemist and druggist, as an extremely simple mode of estimating their phosphatic value.

I take for granted that phosphate of lime consists of 3 equivalents of lime (3CaO) and 1 equivalent of phosphoric acid (PO_5), and is converted into biphosphate by the addition of 2 equivalents of sulphuric acid ($2SO_3$). The following diagram will exhibit this interesting reaction:—

Neutral phosphate of lime.	1 eq. phosph. acid	$PO_5=72$	Biphosphate.
	3 eq. lime	$CaO=28$	
		$2CaO=56$	Sulphate of lime.
	2 eq. sulph. acid	$2SO_3=80$	

Here we see 2 eq. of sulphuric acid combine with 2 eq. of lime to form sulphate of lime, while the remaining equivalent of lime combines with the phosphoric acid to form biphosphate of lime.

Mr. Burnard, in his ingenious process, recommends 100 gr. of the manure to be tested to be added to 2 pints of water; and this large quantity of water is employed that sufficient of the sulphate of lime may be taken up by the water, to afford lime by its decomposition with soda for combination with the biphosphate, which is also in solution. The following is the exact method I have adopted in working out Mr. Burnard's process:—I have a measure holding 1000 gr. of water; it is, in fact, a piece of glass tube, about half an inch in diameter, stopped at one end, and divided into 100 equal parts; consequently each division represents 10 gr. of water. I then dissolve 10 gr. of neutral carbonate of soda in rain-water, and make it up to 1000 gr. This constitutes my test solution of soda; every ten divisions of the measure represents exactly 1 gr. of soda. I then take another glass tube, about 1 ft. long and $\frac{1}{4}$th of an inch diameter, stopped at one end with a plug of gutta percha, which is perforated with a pin; the object of this tube is to add the test solution of soda, drop by drop, at the termination of the operation, when great nicety is requisite; this may be effected by applying the finger at the other end of the tube.

I now proceed to test, agreeably to the recommendation of Mr. Burnard, one half of the solution, or one pint, with my small tube, graduated into four divisions, so that each division represents $\frac{1}{10}$th of a grain of soda. It may require soda solution equal to about ten divisions of the small drop tube (or 1 gr. of soda) to neutralise the free sulphuric acid that may be present in the solution operated upon; but this is readily ascertained by adding the test solution gradually, and so long as the liquor remains quite clear when well agitated by stirring; directly the liquor assumes a slight milky appearance, the free sulphuric acid is neutralised, and

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* "Qualitative Analysis," sixth edition, p. 82.

the conversion of the biphosphate in solution into neutral phosphate is beginning, the milkiness being occasioned by the precipitation of phosphate of lime through the decomposition by the soda of the sulphate of lime in solution.

At this time a piece of blue litmus paper, fastened to a piece of cork, must be put into the liquor, when it instantly becomes red. Having noted the exact quantity of soda solution employed to neutralise the free acid, more of it may now be added gradually, until the liquor begins to show an alkaline reaction, constantly stirring the whole time; this will be readily seen by the litmus paper assuming a bluish colour. We will now suppose that 1 gr. of soda has been employed in neutralising any free acid that may be present, and 12 gr. have been employed in decomposing the sulphate of lime for the precipitation of all the biphosphate as neutral phosphate; it now remains to estimate the quantity of biphosphate in the 100 gr. of manure.

Now, 1 equivalent of carbonate of soda (53) combines with 1 equivalent of anhydrous sulphuric acid (40) to form sulphate of soda. Consequently, we have this proportion —as 53 is to 40, so is the 12 gr. of soda to the quantity of anhydrous sulphuric acid employed in the conversion of the neutral phosphate of lime into biphosphate, which is 9·05; but as we have only tested half the liquor, the whole will show 18·1 gr. as the quantity of anhydrous sulphuric acid employed in the 100 gr. of manure.

Again, our diagram shows that 80 parts of anhydrous sulphuric acid are necessary to form 100 parts of biphosphate; consequently, we have this further proportion to show the percentage of biphosphate contained in the 100 gr.

As 80 : 100 : : 18·1 : to the percentage of biphosphate formed by the 18·1 gr. of anhydrous sulphuric acid, which shows the manure to contain about 22½ per cent of soluble or biphosphate of lime.

As this is not intended for your professional readers, I hope it may not be considered too tedious for the pages of your valuable publication.—I am, &c.

W. Little.

Heckington Hall, Lincolnshire.

LECTURE EXPERIMENTS.

MELTING METAL IN A HANDKERCHIEF.

We are all familiar with the experiment of wrapping a handkerchief tightly round the bowl of a spoon, and holding the part of the handkerchief thus stretched over a spirit lamp, as an illustration of the conducting power of the metal of the spoon for heat. A more sensational form of the same experiment is to be found in such books as "The Young Man's Book of Amusement," "Endless Amusement," &c., in which a bullet is to be melted in a handkerchief by wrapping it round the bullet, and then holding the enclosed bullet over a candle until melted. On trying this experiment I have failed owing to the difficulty of preventing creases. The following modification of the experiment, however, is easily managed, and is very telling:—Two or three pounds of fusible alloy are melted, and run into an evaporating dish; when cold, a handkerchief is stretched over the smooth convex form thus obtained, and the mass may then be melted over a Bunsen's burner in the course of a few minutes; on piercing the handkerchief the melted metal runs out, and may be received in a mould.

C. J. Woodward, B.Sc.

Midland Institute, Birmingham, March 11th, 1868.

MISCELLANEOUS.

Test for the Presence of a Free Acid.—Dissolve chloride of silver in just sufficient ammonia to make a clear solution. If a little of the test be added to ordinary spring water, the carbo
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their own respective establishments, and an account of Professor Liellegg's discoveries has been published in the *Austrian Gazette for Mining and Metallurgy.* The spectrum pointed out by Professor Liellegg, belongs to the flame of carbonic oxide. It can be seen in the flame escaping from the mouth of the converter during the preliminary operation of heating this vessel with coke only; and in that case the lines referred to are very faint, and it requires some practice or knowledge of the precise spots in the spectrum where these bright lines should be looked for, to discover them. During the first period of the Bessemer process the spectrum is very faint. The yellow portion is almost invisible, and even the sodium line is missing; the blue and purple portions are extremely faint. The absence of the sodium line can be accounted for only by the consideration that there is no real flame formed by incandescent gases escaping from the converter at that early stage, but only a mass of sparks carried by the nitrogen from the blast, the oxygen of which remains in the converter, combining with silicium. As the flame gradually appears in the centre of the volley of sparks, the spectrum widens and shows yellow light, until suddenly, the sodium line in the yellow field becomes visible, first only for moments as a flashing bright streak, and after less than one minute as a constant and clearly defined line. The appearance of the sodium line marks the commencement of the decarburisation, although this line does not belong to the charge of iron at all, but rather to the accidental presence of sodium compounds in very minute quantities. It is therefore only indirectly connected with the combustion of carbon; *i. e.*, the appearance of the sodium line is a signal of the completion of the continuous spectrum, and this continuous spectrum belongs to the combustion of carbon. As soon as the sodium line has taken a steady and permanent appearance, the characteristic lines of the carbonic oxide may be looked for in the greenish-yellow, in the green, and in the purple field. In each of these three fields one bright line becomes clearly visible at that time. As the flame increases in size and brilliancy, the spectrum comes out more and more clearly. Bright lines increase in number in each of the first-named three fields, and ultimately, at the height of the process, some bright lines show themselves in the red and, occasionally, also in the blue field. The green field in the spectrum, however, is the real point of observation in practice, as in this the lines are most clearly visible, and in it they appear first and disappear last. The spectrum, as a whole, is by no means steady or constant, but its fluctuations do not displace any of the bright lines; they only alter the background or the continuous spectrum upon which they appear. After the "boil," the maximum intensity is reached; and at that stage, and only with very hot charges, a bundle of bright lines appear in the bluish-purple portion of the spectrum. About four or five minutes before the end of the charge of three tons, the lines begin to disappear in rapid succession, and in the invented order of their appearance—first, the bluish-purple, then the blue lines, after these the red, &c. When the last green line disappears, the vessel is turned, and the charge completed by the addition of spiegeleisen. The yellow sodium line does not disappear to the end of the operation. Sometimes the vessel is turned when all lines in the green field with the exception of two have disappeared. This depends upon the special experience of the case, and it is clear that it is of less importance whether the one or the other mark be taken, if it is only regularly adhered to, and the charge of spiegeleisen regulated accordingly. The practical results are highly satisfactory, since they make the regularity of the "temper" of Bessemer steel practically independent of the skill and experience of the charge-manager, the changes of the spectrum being made more marked and unmistakable than those of the appearance of the flame itself. Hitherto, no experience with British hæmatite irons has been gained, and the use of the spectroscope in this country must be preceded by some careful trials and observations in order to fix the character of the changes. It is highly probable that they will prove very similar, if not absolutely the same as those observed with Styrian charcoal iron, but mere probabilities are not sufficient in the case like this. If the Bessemer steel makers should gain no more by the use of the spectroscope than the possibility to show to the noisy disbelievers in the uniformity of Bessemer steel that a child may conduct the charge without the least chance of error, just the same as a boy can now work the whole mechanical apparatus of the converters, the gain would be very great. But there is a greater gain immediately to be realised by the use of the spectroscope. The steel-masters will become less dependent upon the skill and attention of their charge-managers or foremen, and the percentage of waste or unsuitable material produced by carelessness or mistakes will be lessened in the general run of practice.—*Engineering.*

An Alleged Preservative against the Cattle Plague.—Chloride of copper is now extensively used in Germany against the cattle plague, or rather as a preservative. The *modus operandi* is as follows:—Take green crystallised chloride of copper, 8 grm.; spirits of wine, 2 kilog., and dissolve. With this solution impregnate a pad of cotton, lay it on a plate, and set fire to it in the centre of the stable, turning the animals' heads towards the flame, so as to make them breathe the fumes. This operation is performed morning and evening, burning one pad for every three heads of cattle. At night, a spirit lamp, filled with the solution, is lighted in the stable. To prevent accidents, the flame is surrounded with wire gauze. The liquid is also administered internally, with the addition of 15 grm. of chloroform for the above quantity. A teaspoonful of this is put into the animal's drink three times a day. As a further precaution the litters are watered with the same solution.

Harmless "Pharaoh's Serpents."—A new method of making the curious chemical toys called "Pharaoh's Serpents" has been suggested by Vorbringer. The black liquor which results as a useless product when coal oil is purified with sulphuric acid, is to be treated with fuming nitric acid. The dark coloured resinous matter which swims on the surface is then collected, washed and dried, when it forms a yellowish-brown mass having about the consistency of sulphur which has been melted and poured into water. When this mass is ignited it undergoes such a wonderful increase in bulk that a cylinder one inch long will give a snake about four feet in length. The briefness of the popularity enjoyed by the "original" serpents was due to the unhealthy vapours given off in the process of burning.—*Scientific American.*

Absorption of Arsenic, Tungstic, and Arsenious Acids from Solution by Charcoal.—In a former number of the Chemical News, I stated that charcoal absorbed nitric acid from sulphuric acid; I have since found that it also absorbs the substances here specified, under the following circumstances. If a few drops of a solution of a salt of arsenic, or arsenious acid, is put into a few ounces of dilute sulphuric acid and the mixed solution agitated at intervals with recently ignited charcoal for an hour or two, the clear liquid obtained by filtration does not manifest any reaction of arsenic when tested by Marsh's process. Lignite has not the same effect as charcoal, though absorbent of weak acids and bases generally, as I have before shown. Tungstic acid also from acid solutions is removed by charcoal applied in like manner, and is given up to a solution of caustic alkali.—W. Skey.

Use of the Lime-Light in Barracks.—On Monday evening a series of experiments with the lime-light were conducted in the Queen's Barracks, Perth, with the view of testing the practicability of its introduction instead of gas, the Horse Guards having resolved to discontinue the use of gas in the Perth barracks, owing to its recent rise to 6*s.* 8*d.* per 1,000 feet. The experiments were made in the open air, in one of the lobbies extending the whole length of one of the wings of the barracks, and in one of the ordinary barrack-rooms. An apparatus, about 20 feet high, was erected in the square, having at the top an appliance for showing off the light, and a reflector above. When the flame was applied and the light regulated, the entire square was lighted up almost as clear as noonday, it being quite easy to read the smallest print at a distance of 100 yards. In the lobby a light of small size was used, covered with a glass globe, and the flame burned so brightly that a pin might have been seen on the floor at the extreme end of the lobby, a distance of fully 30 yards. The company then adjourned to one of the ordinary barrack-rooms, where a very small apparatus was fitted up; and, on the light being applied, the room was lighted up much more brilliantly than it would have been by gas. In fact, if there was any fault, it was that the light was too brilliant for the size of the room. The experiments were witnessed by a scientific gentleman from London sent by the War Office, and by the colonel commanding the Royal Engineers in Scotland, both of whom expressed themselves highly satisfied. We understand that contracts have been entered into for using it in the camp at Aldershot, and Government intend shortly to introduce it into all the barracks in the country.—*Edinburgh Courant.—Journal of Gas-Lighting.*

Select Committee on Scientific Education.—In the House of Commons on Friday last, on the motion of Mr. Samuelson, it was agreed that the Select Committee on Scientific Instruction do consist of 18 members:—Mr. Acland, Mr. Akroyd, Mr. Bagnall, Mr. Bazley, Mr. Henry Austin Bruce, Mr. Berecroft, Lord Frederick Cavendish, Mr. Dixon, Mr. Graves, Mr. Gregory, Mr. Thomas Hughes, Sir Charles Lanyon, Mr. M'Lagan, Lord Robert Montagu, Mr. Edmund Potter, Mr. Powell, Mr. Read, and Mr. Samuelson.—*Times.*

Modern Physical Science.—The following forms the conclusion to a course of lectures on "Heat," recently delivered at Eton College by Mr. G. F. Rodwell. "I have said much in the foregoing lectures concerning the motions possible to particles of matter, because I have wished to impress upon you the fact that physical science is daily becoming more and more a science of kinetics,—a science which resolves the direct acting causes of phenomena into motions of particles, variety of form being induced by variety of motion. We have done with imponderable fluids, their media, subtle essences, and with all the meaningless terms which have from time to time been proposed to designate the so-called Physical Forces. We now regard them as attributes of matter, inseparable from matter; as actions possible to small particles, whether they be extremely extended as in the ether which pervades space, or comparatively close together as in the metals. Forces differ from each other either because the velocity of the motion which constitutes them varies, or because the motions themselves differ in form. All matter possesses motion. We have seen that if we increase one kind of motion to a certain extent the substance possessing it becomes what we call hot, while if that motion be further increased light appears; hence the inference that the motion called light is an intensified form of the motion called heat, that is that the difference is one of velocity, not of form or character. Again we have seen that the motion of heat interferes with the propagation of the motion called electricity; hence the inference that the one motion differs in form and character from the other, and not simply (as in the case of heat and light) in velocity unaccompanied by change of form. We can readily comprehend this if we bear in mind that the particles of a mass of matter moving with a certain kind of motion, cannot so readily assimilate a motion of a different kind as if they were at rest at the time of its inception, or as if the new motion were similar in form, and diffe
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great satisfaction. As a distinct private experiment, a small torpedo, charged with 20 lbs. of a highly explosive agent (equal to about 100 lbs. of gunpowder), the invention of Mr. Horsley, scientific chemist, of Cheltenham, but formerly of Portsmouth, was brought in contact with a vessel, which was instantly and totally destroyed with a grand effect, the report of the explosion being heard for miles round Pembroke. There is no doubt about its being one of the most destructive engines of war yet invented, and will make short work of an enemy.—*Hants Telegraph and Sussex Chronicle.*

Applying Charcoal to Sewer Ventilators.—In a recent number we gave an abstract of the report to the Metropolitan Board of Works "On the Ventilation of Sewers," by Dr. Miller. Our attention has since been called to the fact that so early as 1862, the proposal of Dr. Miller had already been successfully carried out in the City of London by Dr. Letheby and Mr. Haywood. The district experimented upon was in the Eastern portion of the City of London, comprising a space of about fifty-nine acres, with 1,700 houses, and about 14,000 inhabitants. Wood charcoal was employed, broken into pieces of the size of a filbert. It was packed closely, but without compression, upon the various trays; and each tray held about 1$\frac{1}{12}$ lbs. of charcoal, making altogether 6½ lbs., distributed over the six trays of each air filter. The experiment was commenced on the 14th of July, 1860, and continued for a period of rather more than eighteen months. The general conclusions from these experiments, and from the consideration of collateral evidence, are:—that dry charcoal in the presence of atmospheric air is a powerful means of destroying the mephitic gases and vapours of sewers and house drains; that the charcoal filters may be used with efficacy in the course of the air channels from the drains and closets of houses, as well as in the ventilation of the public sewers; that in applying the charcoal, those contrivances should be used which offer the least resistance to the free passage of the air; that the situation of the filters is best, when the charcoal is protected from wet and from dirt, and is easily accessible; that from the ascertained efficacy of charcoal in destroying the dangerous emanations from sewers, the system may be generally applied with great advantage; and that from the experience derived from this extensive practical inquiry, they are satisfied that the expense of the system might be considerably reduced below that indicated by the cost of the experiment.

Professor Gamgee's Method of Preserving Meat.—The necessity of some plan for preserving meat has long been felt. Hence it is that every plan, as soon as announced, is seized by the anxious public. If we may believe late reports from London, this desire is at last soon to be gratified, and in a manner which will leave nothing desirable unaccomplished. It seems that Professor Gamgee, President of the Albert Veterinary College of London, author of several works on the cattle plague, and a recognised authority in such matters, discovered a new process for preserving meats, which he has patented in Europe and America. The process is simple and quite inexpensive. The animal, when practicable, is caused to inhale carbonic oxide gas. Before it is quite insensible it is bled in the usual way. When dressed the carcass is suspended in an air-tight receiver, the air exhausted, and the receiver filled with carbonic oxide gas; a small quantity of sulphurous acid gas is also added. After remaining here from 24 to 48 hours, meat may be removed, and hung in a dry atmosphere; it will keep for one, two, or three months, or longer, with no perceptible change in taste or appearance. The tests of the method thus far applied have been attended with success. Beef killed in London in March last was sent to New York in June, and as late as the middle of July was shown to a prominent butcher in Fulton market, who did not discover that it was other than ordinary beef, and expressed the opinion that it had probably been killed about two days. Mutton killed in London last July, and sent to this city soon after, is now perfectly fresh, and one piece of beef kept for ten days in a can surrounded by water at a temperature of 90 to 100 degrees, came out perfectly fresh. The process, in the opinion of eminent chemists, does not injure the meat in the least, which is an advantage very difficult of attainment, even in the case of transportation of live stock, which is liable to the bad effects of confinement and the length of the journey. Among the beneficial results of the adoption of this scheme would be a better supply in our markets of wholesome meat, and at a desirably cheaper rate. It is expected that Professor Gamgee will soon visit this country for the purpose of inaugurating his project.—*American Journal of Mining.*

Production of a Fragrant Substance from Resin (probably an essential oil).—Common resin, lac, or kauri gum, in a state of powder, is generally heated with somewhat dilute nitric acid for a few hours; the mixture or the solution, as the case may be, is then evaporated to dryness, or nearly so, and treated with an excess of a strong solution of common soda, caustic potash, or lime in water; the resulting liquid is then transferred to a retort, and distilled; at first the distillate has an odour of garlic, but this gradually gives way to an odour decidedly fragrant. On redistilling the portion last drawn over from concentrated sulphuric acid, a strong aqueous solution of this odourous substance is obtained, the solution itself has a warm aromatic flavour, and the odour assimilates to that of peppermint mixed with lavender. Bichromate of potash with sulphuric acid, also, may be used for the oxidation of the resin employed.—W. SKEY.

Champagne from Petroleum.—It is no longer a secret of the chemist's laboratory that clear golden syrups can be made from starch and sulphuric acid; that delicious wines and brandies can be made from beet-root; that a barrel of peanuts can be transformed into excellent coffee; that lard can absorb an enormous quantity of water in certain conditions; that in fact there seems no limit to the adulterations that an intelligent and dishonest chemist can practise upon his fellow-men. All these marvels of chemical science have in these latter days become degraded into mere tricks of trade, and their chief beauty is in their capacity to enable unscrupulous dealers to lighten the pockets and destroy the stomachs of the confiding and consuming public. Concerning the article of champagne, a writer in the Portland, Me., *Star* tells us that it is made from a thousand different substances—even from refined petroleum. Yes, from the fiery benzoles a sparkling, bubbling, foaming champagne can be produced which will delight the eye, tickle the palate, gladden the heart momentarily—but quicken our paces toward the graveyard. This is a new use for petroleum, which those who have been experimenting with it as an agency for generating steam have little dreamed of. Who can say that the Pennsylvania oil territory, now considered mostly worthless, may not some day be regenerated into the great champagne-producing country of the world.—*Cincinnati Journal of Commerce.*

PATENTS.

3537. A. V. Newton, Chancery Lane, "An improved method of manufacturing cast steel and malleable iron."—A communication from E. L. Seymour, New York, U.S.A.—Petition recorded December 12, 1867.

3695. J. Jowett, Parkhead, Lanark, N. B., "Improvements in furnaces."—December 28, 1867.

3707. M. A. F. Mennons, Southampton Buildings, Chancery Lane, "The manufacture of a vegetable substitute for animal hair from a product not hitherto used for that purpose."—A communication from W. Staufen, Rue Auber, Paris.

3714. H. Bessemer, Cannon Street, London, "Improvements in the treatment of crude or cast iron, and in the manufacture of malleable iron and steel."—December 31, 1867.

28. J. T. Emmerson and J. Murgatroyd, Heaton Norris, Lancashire, "Improvements in the manufacture of iron, and in the application thereof to certain useful purposes."

33. W. H. Atkinson, Aldersgate Street, London, "Improvements in the preparation and use of compositions for cleansing and sweetening casks and other vessels, such composition being also applicable to other purposes."—January 4, 1868.

impurities from iron, steel, and other metals."—A communication from J. T. Bennett, Pittsburgh, Penn., U.S.A.

59. G. Davies, Serle Street, Lincoln's Inn, Middlesex, "Improvements in the mode of combining wrought and cast iron or other metals for various useful purposes."—A communication from W. M. Arnold, New York, U.S.A.

64. P. Spence, Newton Heath, Manchester, "Improvements applicable to roasting or calcining copper and other ores containing sulphur and also regulus, and in apparatus connected therewith."—January 7, 1868.

78. W. E. Kenworthy, Crown Point Road, Leeds, "An improved method of purifying drains and sewers."—January 8, 1868.

85. C. J. B. King, Great Portland Street, Middlesex, "An improved process to be employed in the tanning of skins or hides."

86. C. H. Newman, Brentford End, Middlesex, "Improvements in the manufacture of unfermented and unintoxicating malt liquors."—Januare 9, 1868.

95. J. Fawcett, Kirton in Lindsay, Lincolnshire, "An improved manufacture of cattle food."

96. J. M. Rowan, Glasgow, N.B., "Improvements in the manufacture of artificial fuel."—A communication from J. R. V. S. Traunfels, Vienna.—January 10, 1868.

105. J. Somervell, Kendal, Westmoreland, "An improved method of obtaining and preserving alimentary substances in a highly-concentrated form."

112. T. Whitwell, Stockton-on-Tees, Durham, "Improvements in furnaces."—January 11, 1868.

131. G. Nimmo, Jersey, New Jersey, U.S.A., "An improved composition for furnace linings, fire bricks, pots, crucibles, and other articles."

138. J. Kidd, St. Paul's Wharf, London, "Improvements in obtaining artificial light, and in apparatus employed therein."

139. J. Head, Newport Mills, near Middlesborough, Yorkshire, "Improvements in furnaces for puddling, boiling, melting, or heating iron or steel."—January 15, 1868.

149. J. A. Jones, Middlesborough, Yorkshire, "Improvements in the manufacture of steel."—January 16, 1868.

164. H. Aitken, Falkirk, Stirling, N.B., "Improvements in treating iron ores or iron stones for the purpose of effecting economy in the obtainment of iron and other products therefrom."—January 17, 1868.

NOTES AND QUERIES.

It has been represented to us that our column of Notes and Queries has occasionally been made the vehicle for the surreptitious disposal of trade secrets by subordinates in chemical works, unknown to their principals. This column has proved to be sufficiently useful to a large class of our readers for us to be reluctant to discontinue it for the sake of a few who abuse its privileges. Probably a more rigid supervision will enable us to obviate the difficulty. There will be no objection to a correspondent asking for information on trade subjects; but the answer must likewise be made public, and in such cases no name or address can be given, no private communications forwarded through us, and no offer of payment for information can be published.

Sulphur in Pyrites.—In answer to the query of your correspondent F. W. W., I beg to offer the following remarks. The method of analysis of sulphur ores usually adopted for commercial purposes is in substance this: a known weight of the ore reduced to fine powder is oxidised (best in a small flask with a funnel placed in the mouth to avoid loss by spirting, and heated on a sand-bath) either by strong nitric acid or nitro-hydrochloric acid (aqua regia), perfectly free from sulphuric acid; after the oxidation is complete, the liquid is evaporated down as far as possible to expel the majority of the remaining nitric or hydrochloric acid; the residue is boiled with a little water, and almost but not quite neutralized by ammonia; a solution of barium chloride of known strength is then added until no further precipitate is produced, the exact point being found by filtering off a little of the liquid after each addition of barium chloride and adding to it a few more drops of the standard solution, care being always taken, in case of a further precipitate being thus produced, to add this filtrate to the original solution, and mix well before filtering a second time. In case of overstepping the mark, it is convenient to have at hand a solution of sodium sulphate of strength precisely equal to that of the barium chloride; this solution may then be cautiously added with repeated filtration and examination of the filtrate with the sulphate solution, until the point is just reached, when addition of sulphate solution produces no further precipitate: by subtracting the volume of sulphate solution thus used from the total volume of barium solution added, the exact quantity of this latter consumed is known. If 1 gramme of sulphur ore be taken, and 32·5 grammes of pure anhydrous barium chloride be dissolved to a litre of fluid, each cubic centimetre of barium solution used will represent ½ per cent. of sulphur in the ore examined; 22·19 grammes of anhydrous sodium sulphate being dissolved to a litre of the second solution. In case of lead being contained in the ore, an error is introduced by the formation of lead sulphate; to avoid this, it is best to precipitate the lead by ammonium carbonate, &c., … of nitrate, and to weigh the barium sulphate produced. Instead of oxidising by acids, the powdered ore may be suspended in caustic potash (free from sulphate), and oxidised by passing washed chlorine into the liquid; lead being converted into dioxide, is thus rendered non-injurious; the alkaline liquid obtained is acidified and precipitated by barium chloride as before. The finely powdered ore may be mixed with 7 or 8 times its weight of a mixture of equal parts of sodium carbonate and potassium nitrate, and heated cautiously in a capacious crucible; the mass being finally boiled with water, and the sulphate estimated in the filtrate; in practice, however, it is somewhat difficult thus to avoid loss by a deflagration, especially with rich sulphur ores. In the volumetric determination usually pursued, a curious circumstance is occasionally observable when much free acid exists in the solution, viz., that a point may be reached when the filtered liquid is clear, and remains so even on standing for a short time, but yields a cloud, or even a precipitate, on the addition either of barium solution or sulphate solution; this source of error is mostly avoidable by nearly neutralising the free acid with ammonia. Another convenient process consists in fusing the weighed ore with a weighed quantity of anhydrous sodium carbonate, twice as much potassium chlorate as ore, and 12—20 times as much sodium chloride (added to moderate the action); CO_2 is expelled, KCl formed, and all the sulphur converted into Na_2SO_4; by dissolving the residue in water and estimating alkalimetrically the unaltered sodium carbonate by a standard acid solution, the portion converted into sulphate, and hence the sulphur in the ore is known. Besides the difficulty of preventing loss by deflagration, this method is open to the small errors caused by the reckoning all arsenic present to be sulphur—usually, however, of no moment for commercial purposes; any calcium carbonate in the ore may, if required, be previously dissolved out by dilute hydrochloric acid.—C. R. A. WRIGHT, B.Sc.

Carbonic Acid.—Can either of your correspondents suggest any crude, native, earthy, or metallic carbonate which parts with its carbonic acid, pure and undecomposed, at a moderate red heat?—G.

Prussian Blue Paste.—Would any of your kind correspondents inform me how the bronze is brought out on this article?—J. R. J.

Microscopic Detection of Flour.—Could you oblige by informing me if there is any, and what, method for detecting pea or bean flour when mixed with common wheat flour, except from the appearance of the starch granules under the microscope?—Q.

Estimation of Phosphoric Acid.—Will you oblige a subscriber to your journal, by informing him where he will find the best and simplest means for ascertaining the quantity of phosphate in artificial manure? Would it be too much to ask you to give the best method in your journal? I observe in your number for February 28th (*Amer. Repr. April*, 1868, *page* 168), a valuable contribution from Mr. Burnard, but it is not sufficiently explicit. Our manure dealers tell us that we cannot judge of the quality of their manures by the soluble phosphate alone, as they contain also a considerable quantity of insoluble, or neutral phosphate. I do not care to know what other matters their manures contain, such as ammonia, soda, potash, &c., &c.; I simply want to know their phosphatic value.—AGRICOLA.

Estimation of Phosphoric Acid.—Reading in your last week's paper an article "On the Volumetric Estimation of Phosphoric Acid," by C. F. Burnard, F.C.S. (*Amer. Repr. April*, 1868, *page* 168), I venture a few remarks upon this subject. The author uses the process of precipitating the phosphoric acid as phosphate of uranium by a solution of nitrate of uranium, and applies it principally to the analysis of phosphatic manures. Several attempts have been made to introduce the above-mentioned process, but they have failed until now, and principally because of the presence of iron in very nearly all manures. To determine the phosphoric acid in manures, a quantity of acetate of soda has to be added, since a precipitate of phosphate of uranium will only appear when the solution has been made acid by acetic acid. The iron present is precipitated as phosphate of iron. Had this precipitate the same composition in every case, the phosphoric acid in it could easily be determined; but this not being so, a more complicated method has to be adopted. I have tried to substitute citric or tartaric acid, to prevent the precipitation of phosphate of iron, but they unfortunately hinder also the precipitation of phosphate of uranium. Perhaps Mr. Burnard has found some more simple way to overcome this difficulty, and if so, I am sure that all chemists would be very glad to hear an account of it.—V. C.

Estimation of Phosphoric Acid.—Having seen some remarks in your last issue on the above subject by Mr. Charles F. Burnard (*Amer. Repr. April*, 1868, *page* 168), I should feel greatly obliged if he will inform me what is the reaction of the soluble phosphates with caustic soda, also the strength of the standard solution. On reading his article, it occurred to me that it might answer for the determination of $3CaO.PO_5$ in bone-ash and other commercial phosphates. I therefore prepared a normal solution of caustic soda. I then dissolved 10 grammes of bone-ash, which I knew to contain 75 per cent. of $3CaO.PO_5$, in HCl, neutralised the free HCl, made up to 400 c.c., and proceeded with the standard solution of NaO, of which 50 c.c. were required to make the solution alkaline. Now 50 c.c. = 50 × ·031 = 1·55 grammes of NaO required for 3·75 grammes $3CaO.PO_5$, or 2 equiv. NaO are equal to 1 equiv. of $3CaO.PO_5$, and not 3 equivalents NaO to correspond with the same number of equivalents of CaO in $3CaO.PO_5$.—J. J. K.

Treating Mineral Oils.—Can any of your correspondents give me a process for treating heavy mineral oils so as to free them from smell, and make them of a bright colour on a manufacturing scale.—A. C. K.

Sugar Refining.—Will you kindly inform me in your "Answers to Correspondents" what becomes of the acetic acid which is set free by the sulphurous acid in "Scoffern's Process" for the refining of sugar? As nearly all the acetates are soluble, and free acid in contact with the dissolved sugar would convert some of the cane sugar into grape sugar. —J. Davies, Bath.

Sulphite and Hyposulphite of Soda.—In reply to "G. W. R., Liverpool" (*Amer. Repr. April*, 1868, *page* 203), I beg to say that there is a large demand for hyposulphite of soda by paper makers as so-called *antichlore*; about 200 tons per annum are yearly consumed in photographic operations alone, while a far larger amount is used by paper makers; hyposulphite of soda is also used by bleachers of calico fabrics. Sulphite of soda is of a more limited use, and somewhat superseded by the hyposulphite.—Dr. A. A.

Signs of Rain.—Of the many prophets that appear in the present day, certainly the weather-eyed ones predominate. Of their prophecies, I may dub myself sceptical, as nearly 99·999 per cent. of the human world do; for the simple reason that their statements are always the reverse of facts. But there is a sign of rain that is known as true to every observing observer, without the study of the stars, or anything else. I allude to a red sunrise, which invariably denotes rain within a few hours. I wish to ask if any reader of the Chemical News can explain why the sun should rise apparently so red when the atmosphere is in, I suppose, a dense state previous to rain. I remember reading a very interesting paper or lecture in the Chemical News, giving the reason for red sunsets. I read the papers at the time with great relish; but I have been subsequently occupied with other matters, incompatible with the retention of theoretic knowledge, and I have not my volumes of the News at hand to refresh my memory. However, I think my memory is correct enough to be able to assert that the facts, as represented in the papers referred to, do not give any reasons for this atmospheric phenomenon, or any basis for establishing a theory of its cause.—An Observer of Nature.

Safflower.—Would any of your correspondents be good enough to enlighten me how to extract the colouring matter from safflower for dyeing pink?—G. Johnson.

Distillation of Grease.—Is there any Treatise published on the distillation of greases and animal and vegetable oils?—David Shaw & Co.

Oxychloride of Magnesia.—If Mr. J. E. Hamilton is still looking for a supply of this article, he may hear of a cheap source by applying to the Publisher.

Production of Carbonic Acid.—In reply to "G," in the Chemical News of the 13th inst. (*Amer. Repr. May*, 1868, *page* 251), I beg to inform him that he may obtain carbonic acid very readily from limestone, marble, or chalk, at a low red heat, by passing a current of steam over them at that temperature.—W. H. Potter.

Preservation of Meat.—Several sheep, killed in England, and preserved by Professor Gamgee's process, have been exhibited in New York last week in an excellent state of preservation. Would you please to give, through your Chemical News, or otherwise, a description of Prof. Gamgee's process, and the mode in which it is carried into effect? —A. G. Hunter, Fair Haven, Connecticut, U. S.

Laboratory Stove.—Preserving Insects.—Can any of your correspondents tell me—1st, What do they consider the best laboratory stove? My present fireplace, besides being useless for heating a crucible, raises such a daily dust as seriously to interfere with analysis (chiefly agricultural analyses). 2d, I have some eastern insects preserved in spirits in small glass bottles, but the spirit has softened the sealing-wax, and evaporated, leaving the insects dry and very brittle. What am I to do?—C. D. H.

Explanation to Mr. S. A. S.—In the Chemical News, of March 6 (*Amer. Repr. Apr.*, 1868, *page* 204), you very severely take me to task, and criticise, 1st, the short hint I gave in "Notes and Queries" (Chemical News, No. 429; *Amer. Repr. April*, 1868, *page* 203); and 2d, the equally very brief note of mine "On the Approximative Determination of Sulphur in Pyrites" (Chemical News, No. 428; *Amer. Repr. April*, 1868, *page* 203). Permit me to observe that your presumption of my ignorance is, in both cases, not proved by facts. I consider "Notes and Queries" only to be intended for short and brief hints; moreover, one might offend querists by supposing them not to be sufficiently well up in analysis to enable them to think and judge for themselves. Certainly, I think it would be quite out of place to expect "Notes and Queries" to contain full directions in every respect. As regards analysis of superphosphates, I am as well aware as the most prominent analyst in such matters, that, without full and thoroughly complete analysis, what I wrote about the estimation of free sulphuric acid would be leading to gross errors. As regards your objection to the approximative valuation of sulphur in pyrites, permit me to say that I thought it unnecessary to add, that I always and invariably, after having for some time ignited the previously weighed pyrites, allow it to cool, then moisten it with pure nitric acid, expose again to heat with due precautions, and repeat this process at least from three to four times, when nothing but peroxide of iron will remain. My only reason for not mentioning this is, that I considered any one might think of it.—Dr. A. Adriani.

ANSWERS TO CORRESPONDENTS.

NOTICE.—The American Publishers of The Chemical News *give notice that in accordance with a suggestion of* Mr. Crookes, *the Editor and Proprietor of the English publication, they will be pleased to receive and forward to him in London any scientific publications issued in America, for review—and also any Notes and Queries, Articles, Correspondence, etc., for publication or reply. Their facilities of communication with* Mr. Crookes *render this very desirable to all persons in the United States who wish to confer with him. Address,*

W. A. TOWNSEND & ADAMS,
434 *Broome Street, New York.*

R. H. R.—"Liebig's Agricultural Chemistry;" "Johnstone's Chemistry of Soils."

P. Holland.—Received with thanks; too late for insertion this week.

R. Broadhurst.—Use hydrofluoric acid gas to obscure the glass globes. Those parts which are required bright must be coated with wax.

W. Schofield.—The letter has been forwarded.

G.—Why will not chalk answer? It will satisfy your conditions.

A. G. S.—The *Bulletin de la Société Chimique de Paris* is published monthly. It can be procured by order through any foreign bookseller.

Solutus.—The normal sulphate of cerium is much more soluble in cold than in hot water, and is precipitated when a cold saturated solution is boiled.

G. Kellogg.—No report has yet been made on the alleged discovery. It is not generally credited. Our neighbours do not hurry themselves.

Librum.—Dr. Marcet's is the most suitable of the books you name; then Dr. Hassall's.

H. Fisher.—Our publisher will write to you on the subject.

Amateur.—The method of cleaning and drying U-tubes is too simple to need description in our columns.

Drs. Wischin and Wilm's telegram from Berlin, asking us not to publish their letter "On the Reduction of Carbonic Acid to Oxalic Acid," arrived on Monday morning—three days too late.

Enquirer.—Liebig's process for silvering glass is well known, and has been more than once described in these pages. Some improvements have been lately introduced, which we will give in a few weeks.

North.—We are not aware that such a work exists. Consult the articles in "Watts's Dictionary," and in the text-books.

Gaskell, Deacon and Co.—We will communicate with our Paris correspondent, and endeavour to ascertain further particulars.

Drysalter.—See answer to "*North.*"

A. G. Hunter, U. S.—Permanganate of soda will do, but we think sulphate of magnesia should also be employed.

Communications have been received from Dr. Wood; Miss Chapman; J. B. Giles; J. Davies; R. Eaton (with enclosure); J. T. Brown, F.C.S.; Bollman Condy & Co.; W. Field; Dr. W. Bird Herapath, F.R.S.; C. F. Burnard, F.C.S.; F. C. Calvert & Co.; Jones & Co.; P. Dorn; Capt. W. A. Ross, R.A.; H. B. Ingram; Rev. Richard Kirwan; J. Muspratt & Sons; A. Payne; B. Wheeler & Co.; W. Kellner; T. L. Patterson; Dr. E. W. Davy (with enclosure); Dunn & Co.; C. M. King; C. Mitchell & Co.; Baron von Seckendorff (with enclosure); Dr. Forbes Watson; John Knight & Co.; Kingsbury & Co.; Fletcher & Co.; John Davies, M.D.; E. Brooke & Co. (with enclosure); L. Demuth (with enclosure); J. R. Irvine; Reginald Petre; R. Broadhurst; C. Steer; Philip Holland (with enclosure); F. J. M. Page; Dr. H. Muller, F.R.S.; W. Schofield (with enclosure); H. Merivale; F. Scott; W. Skey, New Zealand (with enclosure); W. Cortis, jun.; T. A. Readwin; J. Spiller; D. Forbes, F.R.S. (with enclosure); Dr. Odling, F.R.S.; Professor Heaton; H. Bailliere; Dr. F. B. Courteney; G. H. Mann, Troy, United States; F. Sutton; W. Bywater; Dr. Adriani (with enclosure); Victor Cruse; J. Landauer (with enclosure); R. Beesley; Rev. Edwin Smith (with enclosure); H. Neale; W. Lea; Ernest Hart (with enclosure); Dr. Odling, F.R.S.; G. Kellogg; G. Wischin and Th. Wilm (with enclosure); W. Little (with enclosure); H. Sugg; C. Collins; A. Liversidge; H. McLeod (with enclosure); Dr. H. E. Roscoe, F.R.S. (with enclosure); H. Fisher; F. O. Ward; J. Samuelson; P. Spence; Messrs. Townsend & Adams, New York; Stephen Dowell; Capt. W. A. Ross (with enclosure); J. Blackburn (with enclosure); J. Apjohn; Foot & Co.; A. Payne; Prof. Wanklyn; Alex. S. Macrae; T. R. Fraser, M.D., Nova Scotia; Dr. Watts; S. Mellor; Niel Mathieson; T. Brown; Dr. W. Bird Herapath, F.R.S.; C. J. Woodward; J. Dobson (with enclosure); Gaskell, Deacon & Co.; W. H. Walenn; Dr. Adriani (with enclosures); M. Scott; A. G. Hunter; F. C. Correy (with enclosure); Norman MacLeod (with enclosures); F. Sutton; G. Johnson; W. H. Potter; C Hunter (with enclosure); Drs. Wischin and Wilm (with enclosure); W. M. Watts (with enclosure); T. R. Fraser, M.D.; W. Chapman (with enclosure); Mr. Hargreaves (with enclosure); Dr. Sheridan Muspratt; A. Sarl; T. Reader); W. Smyth (with enclosure); J. Carter Bell (with enclosure); G. Foord, Victoria; J. Burton; J. Mayer (with enclosure); Dr. Letheby (with enclosure); F. H. Hobler; R. Lyle; David Shaw & Co.; W. Little (with enclosure); Baxter, Rose, Norton & Co.; Jose Piodela; A. Tribe (with enclosure); A. Coppins; J. Browning; T. A. Readwin (with enclosures); E. Stanford; T. Dorn (with enclosure); J. Van Voorst; Nicholson & Maule; J. C. Wilson; C. M. King; A. S. Herschel; Dr. W. B. Herapath, F.R.S. (with enclosure); Dr. Apjohn; C. M. King; W. B. Giles; Captain W. A. Ross, R.A.

Books received.—"The Illustrated Photographer;" "Dictionary of Chemistry," by Henry Watts, B.A., F.R.S., F.C.S. Part XLIV. Types, Water. London: Longmans, Green & Co.; "Hardwicke's Science Gossip;" "Pharmaceutical Journal;" "American Journal of Pharmacy," November, January; "The Scientific Review;" "Le Maraviglie Della Scienza;" "Zeitschrift für Chemie;" "Faraday as a Discoverer," by John Tyndall. London: Longmans & Co.; "List of Chemicals and Drysalteries," from Messrs. E. Brooke and Co., Manchester, Glasgow, Bradford, and Huddersfield; "On Vanadium," by Henry E. Roscoe, B.A., F.R.S.: "On Faraday, as a Discoverer," by Professor Tyndall, LL.D., F.R.S.; "Report of Medical Officer of Health for the Limehouse District; "The Acadian Reporter;" "The Mining Gazette," Halifax, N. S.; "Le Moniteur Scientifique;" "A Sketch of a Philosophy, Part II.—Matter and Molecular Morphology," London, Williams and Norgate; "Zeitschrift fur Chemie;" "Le Maraviglie Della Scienza;" "Recollections of the Paris Exhibition of 1867," by Eugene Rimmel, London: Chapman & Hall.

[English Edition, Vol. XVII., No. 433, page 146; No. 434, page 157; No. 431, page 122; No. 432, page 134; No. 433, page 146; No. 434, page 157; No. 431, page 122; No. 432, page 134; No. 434, page 157.]

ON THE SPECTRUM OF THE BESSEMER FLAME.

In our number for March 20th (*Am. Repr.*, May, '68, page 247), we quoted an article from *Engineering*, giving the results of Professor Lielegg's observations on the spectrum of the Bessemer-flame. As these results were published in *Engineering* as entirely new, and no mention was made of any prior observations, it is only right that attention should be called to the fact that as long ago as 1862 the same results had been obtained by Professor Roscoe, and were published in the form of a short preliminary notice in the *Proceedings* of the Manchester Literary and Philosophical Society, for February 24th, 1863. As the note is extremely short, we transcribe it in full.

"Professor Roscoe stated that he had been for some little time, and is still, engaged in an interesting examination of the spectrum produced by the flame evolved in the manufacture of cast steel by the Bessemer process, on the works of Messrs. John Brown and Co., of Sheffield. The spectrum of this highly luminous and peculiar flame exhibits during a certain phase of its existence a complicated but most characteristic series of bright lines and dark absorption-bands. Amongst the former the sodium, lithium, and potassium lines are most conspicuous; but these are accompanied by a number of other, and as yet undetermined, bright lines; whilst among the absorption-bands, those formed by sodium vapour and carbonic oxide can be readily distinguished. Professor Roscoe expressed his belief that this first practical application of the spectrum analysis will prove of the highest importance in the manufacture of cast steel by the Bessemer process, and he hoped on a future occasion to be in a position to bring the subject before the Society in a more extended form than he was at present able to do."

In a lecture delivered before the Royal Institution (May 6, 1864), a year later than the communication quoted above, Dr. Roscoe described the Bessemer-spectrum more fully, and pointed out the existence of lines produced by carbon, iron, sodium, lithium, potassium, hydrogen, and nitrogen.

An important practical result of the observations on which these communications were based, was the discovery that the exact point of decarbonisation could be determined by means of the spectroscope with much greater exactitude than from the appearance of the flame itself, the change in which indicating the completion of the process is minute, and requires a lengthened experience to detect with certainty. This method of determining the point at which it is necessary to stop the blast was, indeed, at that time (1863) in constant use at Messrs. Brown's works, at Sheffield, and has since been introduced with equal success by Mr. Ramsbottom (at the suggestion of Dr. Roscoe) at the London and North Western Railway Company's steel works, at Crewe.

Dr. W. M. Watts now draws our attention to a paper "On the Spectrum of the Bessemer-flame," which he published in the *Philosophical Magazine* for December last. In it he states that he was at that time acting as assistant to Prof. Roscoe, and in that capacity conducted a lengthened examination of the Bessemer-spectrum at the works at Crewe. The results of that investigation

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The lines of lithium, sodium, and potassium are always seen, and are unmistakeable.

The three fine bright lines 73·7, 76·8, and 82 are due to *iron*. The red band of hydrogen is seen as a black band, more prominent in wet weather.

After the charge of iron has been blown, it is run into the ladle, and a certain quantity of the highly-carbonized *spiegeleisen* is run into it. The effect of the addition of the spiegeleisen is the production of a flame which is larger and stronger when the blow has been carried rather far. This flame occasionally gives the same spectrum as the ordinary Bessemer-flame; but more commonly a quite different spectrum is seen, which reminds one at first of the ordinary carbon-spectrum, but differs from it very remarkably.

In the carbon-spectrum, each group of lines has its strongest member on the left (*i.e.* less refrangible), and fades gradually away towards the right hand; in the spectrum of the spiegel-flame the reverse is the case; each group has its brightest line most refrangible, and fades away into darkness on the least-refracted side. A comparison of the drawing of the spectrum of the spiegel-flame with that of the Bessemer-flame will show that they really contain the same lines; but the general appearance of the spectrum is completely changed by alteration of the relative brightness of the lines. This was shown by direct comparison of the actual spectra.

Dr. Watts concludes his paper in the *Philosophical Magazine* by saying—"There can be no doubt that the principal lines of the Bessemer-spectrum are due to carbon in some form or other. My own belief is that they are due to incandescent carbon-vapour. The experiments in which I am at present engaged have already shown the existence of *two* totally different spectra, each capable of considerable modification (consisting in the addition of new lines) corresponding to alterations in the temperature or mode of producing the spectrum, and each due to incandescent carbon. It is possible that the Bessemer-spectrum may prove to be a third spectrum of carbon, produced under different circumstances from those under which the ordinary carbon-spectrum is obtained; and the intensity of the dark bands is more probably due to contrast with the extreme brilliancy of the bright lines, than to their actual formation by absorption."

COAGULATION AND PRECIPITATION OF CLAY BY NEUTRAL SALTS GENERALLY.

BY WILLIAM SKEY,

ANALYST TO THE GEOLOGICAL SURVEY OF NEW ZEALAND.

A GREAT many substances are recommended as substitutes for filtration in the clarifying of water turbid from the presence of clayey matter, but so far as I can learn, they all depend for their individual effects upon some chemical interchange between themselves and a portion of the clayey matter in suspension, or upon the formation of a new compound out of the elements of the agent employed; in either case the clay, or the residue of the clay, being carried down mechanically, entangled in the newly formed substance. The object of this communication therefore is to bring under notice that several neutral salts, having their component parts so strongly combined among themselves as to render their decomposition by clay apparently impossible, are individually capable of producing the same effect upon clay in suspension; thus a strong solution of chloride of sodium, chloride of ammonium, chloride of calcium, chloride of magnesium, or chloride of barium, or sulphate of soda, applied to a small quantity of clayey water, causes an immediate aggregation of the particles, and their complete precipitation shortly afterwards.

When the solutions are applied to a rather large proportion of clay water, the precipitation is not complete for several hours. The volume of clayey water clarified by one grain of certain of the above-named salts in 24 hours is approximately as follows—1 grain of common salt clarifies 5 ounces, 1 grain of chloride of barium or calcium, clarifies 10 ounces; 1 grain of sulphate of soda clarifies 5 ounces of clayey water; in addition to these it was found, 1 grain of lime clarifies 15 ounces, and 1 grain of sulphuric acid clarifies 50 ounces of clay water in the same time. Magnesia also when intermittently agitated with clay water has the same effects. Upon washing these clay precipitates repeatedly with pure water, the clay re-acquires its tendency of permanent diffusion. The quantity of clayey matters present appears of secondary importance, complete precipitation having nearer relation to the degree of dilution allowed to the salt employed. From these considerations, taken along with the fact of the existence of powerful affinities between the component parts of most of the substances here specified, it seems certain that these results of coagulation and precipitation are not due to their decomposition; the alternative is therefore, that they must thus act solely from their affinities for water; and if this is so, then from inference it would appear that the well-known quality of clay to remain in permanent suspension in rain or spring water in spite of its relative superior gravity is entirely due to the effect of a true chemical affinity existing between them; possessing in common with soluble bodies an insatiable quantitative affinity for water. Clay differs therefrom in the low intensity of this affinity (if I may use the term), a circumstance of course predicable from its insolubility; clay thus deports itself like certain other substances, which, though soluble in pure water, are precipitated therefrom by others possessed of a superior affinity for the solvent,—for instance, ferrocyanide of iron by salts generally, silica in ammonia by chloride of ammonium, nitrate of baryta by nitric acid.

In conclusion I would desire to suggest that the transparency of the sea, into which are continually pouring such enormous quantities of turbid water, may be entirely due to the presence of so much saline matter.

ON THE PRECIPITATION OF COPPER BY HYPOPHOSPHOROUS ACID.

BY WOLCOTT GIBBS, M.D.,

RUMFORD PROFESSOR IN HARVARD UNIVERSITY.

IN a memoir on the hypophosphites, A. Wurtz* has shown that when solutions of copper are heated to 70° C. with hypophosphorous acid, a hydruret of copper is precipitated, which on boiling is reduced to metallic copper with evolution of hydrogen. On repeating this experiment, I found that the precipitation of copper is complete, and as the alkaline hypophosphites are now to be had in commerce, it appeared probable that the process might be applied to quantitative estimation.

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* Ann. de Chimie et de Physique, 3rd series, vol. vi., p. 199.

liquid containing a little free acid. The precipitation from the nitrate is always incomplete. When chlorhydric acid or chlorides are present the method fails entirely, the copper being reduced to subchloride and remaining in solution. The solution must not be too dilute; the precipitation is complete if the saturated solution of sulphate be diluted with not more than ten times its bulk of water, before the addition of the hypophosphites. In order to avoid the sudden evolution of hydrogen gas, and also to obtain the precipitate in a spongy coherent form, it is best not to allow the liquid to boil. The solution of hypophosphite having been added in the cold, and in excess, the temperature is to be gradually raised until, after standing for some minutes between 80° C. and 90° C., the hydruret of copper has entirely separated in coherent masses. It is easy to determine when the precipitation is complete, by taking out a drop of the clear liquid with a rod, and testing upon a porcelain plate with a drop of sulphydric acid solution. No filter need be used if the precipitation be effected in an assay flask; the copper is easily washed by decantation, and may then be transferred to a porcelain crucible by the well-known method of inversion, dried and gently ignited in a current of hydrogen. The following analyses will serve to illustrate the accuracy of this method. In all of them hypophosphite of magnesium was employed as the precipitant.

	Gr. pure sulphate of copper.		Gr. of copper.		Per cent.	
1	1·1650	gave	0·2965	=	25·45	(Chauvenet.)
2	1·5590	"	0·3970	=	25·45	"
3	1·4255	"	0·3625	=	25·43	"
4	1·3050	"	0·3327	=	25·42	(R. B. Carman)
5	0·8208	"	0·2087	=	25·42	(E. F. Gale)

In (4) and (5) a large excess of sulphate of nickel was present.

The formula $ƟuSƟ_4$ + 5aq. gives 25·42 per cent. of copper. In the third analysis, sulphates of iron, manganese, nickel, and zinc, in very large excess, were added to the solution of copper.

I. In a very pure subsulphide of copper from Arizona, Mr. Chauvenet found, in four analyses, 74·24, 74·37, 74·36, and 74·41 per cent. copper.

II. In an alloy of copper and nickel

	Gr.		Gr. of copper.		Per cent.	
6	0·4245	gave	0·3605	=	84·92	(Chauvenet)
7	0·3615	"	0·3070	=	84·92	"
8	0·1380	"	0·1170	=	84·85	"
9	0·1980	"	0·1680	=	84·84	"

III. In brass wire

	Gr.		Gr. of copper.		Per cent.	
10	1·6300	gave	1·0705	=	65·67	(Chauvenet)
11	1·8655	"	1·2240	=	65·61	"
12	1·6770	"	1·1010	=	65·65	"

In the last seven analyses the alloy was dissolved in sulphuric acid, nitric acid being added from time to time to assist in solution. The solution was then evaporated until the last traces of nitric acid were expelled. The presence of iron in the form of sulphate does not in any way interfere with the complete precipitation of copper by hypophosphite of magnesium. When sesquichloride of iron is present, however, the copper is always reduced to subchloride, and is not precipitated as metal or hydruret. A solution of a hypophosphite chlorhydric acid. I have endeavoured to base upon this reduction a method for determining iron volumetrically, but all the experiments failed, in consequence of the difficulty of determining the exact point at which the reduction of the iron is complete. Sulphocyanide of potassium, proposed for this purpose by Winkler,[*] in his process, with subchloride of copper as a reducing agent, was not found to give sharp indications. When copper and iron are present together, as chlorides, the addition of hypophosphite of magnesium simply reduces the copper to subchloride, as above stated. If in this case we add an alkaline chloride to keep the subchloride of copper dissolved, the copper may be easily precipitated as subsulphide by sulphydric acid gas. When arsenic or antimony are present with copper, these must first be separated before precipitating the copper as hydruret, as careful experiments by Mr. C. Lilly have shown that both arsenic and antimony are precipitated with the copper. Mr. Lilly obtained the following analytical results when arsenious acid was present.

Gr. of sulphate of copper.		Gr. metallic copper.		Per cent. copper.
1·2690	gave	0·3279	=	25·83
1·5127	"	0·3905	=	25·77
0·9638	"	0·2509	=	26·03

The formula gives 25·42 of metallic copper. In presence of antimonious acid—0·7100 gr. sulphate of copper gave 0·2454 gr. of copper = 34·56 per cent. After addition of $Sb_2Ɵ_3$ and Rochelle salt, 0·9875 gr. sulphate of copper gave 0·2426 gr. copper = 24·56 per cent. Repeated analyses by Mr. Lilly also showed that copper could not be determined accurately in Schweinfurt green by hypophosphite of magnesium, and that th
presence of Rochelle salt did not completely preve
the precipitation of arsenic with the copper wh
arsenious or arsenic acid were mixed with sulphat
copper.

In assaying copper ores, it is usually desirab
bring the metal at once into the form of sulphate.
merous experiments made in this laboratory ful
tify me in recommending the following metho
finely pulverised ore (sulphides of copper and
to be mixed in a porcelain crucible with thr
times its weight of a mixture of 1 molecul
phate and 1 of nitrate of potassium. The mix
to be slowly heated to low redness, whic
complished in a muffle. The metallic
completely oxidised without the least f
heated mixture. Enough strong sulphu
vert all the sulphate of potassium into b
added, and the crucible is to be again
until the contents run to a clear fuse
ing the mass usually separates read
ble, which is not attacked, and on s
copper are found completely conv
This process has been tried suc
variety of ores. The whole op
an hour. In the case of ores
phide of iron, it is best to hea
as long as sulphur is given
the oxidising mixture and he
of lead, zinc, and antimony
the same process.—*Amer.*

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[*] Zeitschrift für Analyt

COAL GAS AS A POSSIBLE SOURCE OF CONTAMINATING SUBSTANCES TO BE TESTED FOR AMMONIA.

In a paper on analysis of water, published at the latter end of last year in the *Scheikundige Bijdragen uit het Laboratorium van het Athenæum Illustre*, at Amsterdam, Dr. Gunning calls attention to the fact, that coal-gas however well purified is by no means free from ammonia; he felt induced to institute some experiments on this subject, the result of which is, that last summer the gas used in the Laboratory at Amsterdam contained 0·00075 gramme of ammonia, or ammoniacal substances in 1 litre of illuminating gas, amounts in bulk to a little over one cubic foot thereof in one thousand cubic feet of gas. Attention is called to the fact, that where wet gas meters are in use, the water of which is never replenished unless by some accident, this water must become pretty fairly saturated with ammonia; on emptying the water from one of the meters at the laboratory at Amsterdam its bulk was found to amount to 219 litres, *i.e.*, 48·66 gallons. 10 cubic centimeters of this fluid yielded 192 mlgrm. of ammonia, or bases of a similar nature. The whole quantity of water contained, therefore, no less than 4·2 kilogs. of these bases; the meter had been in use for only two years. Since coal-gas, moreover, always contains sulphur compounds, there is formed sulphate of ammonia, which, on becoming converted by the intense heat into bisulphate of ammonia, attacks the glass cylinders, or chimneys, placed on the Argand gas burners. Dr. Gunning has found from expressly instituted experiments that no combustion of the ammonia takes place, not even in Bunsen burners, and mentions that a platinum basin filled with perfectly pure water, and placed for even less than an hour over a Bunsen burner, had got contaminated with a perceptible quantity of ammonia in the form of sulphate. There are two gas companies supplying Amsterdam, and there is a strong competition, but also a good surveillance to secure the gas to be as pure as possible. The experiment above alluded to was made with gas taken directly from the street main.

BLOW-PIPE COAL ASSAY.*

BY BENJAMIN SMITH LYMAN.

Many young assayers are perhaps hardly aware how well adapted the blow-pipe apparatus is to the assaying of coal. Not only does the portableness of the apparatus make it very convenient for use away from home, wherever the scales can be set up; but its use at home is quite as satisfactory on the score of exactness as the assay with the muffle or retort, or large platinum crucible and large scales.

Besides the ordinary pieces of the blow-pipe apparatus, as made at Freiberg, all that needs to be made expressly for the coal assay is a small covered platinum crucible of the same size and shape as the clay crucibles of that apparatus; and there must be a little ring for the crucible to stand on, of German silver, about three-eighths of an inch across and half that in height. Such a crucible cover and ring weigh about two grammes and a half more than the ordinary metallic cup that rests on the pan of the scales; the crucible and ring without the cover weigh less than two grammes more than the cup. If it be desired to determine the amount of hygroscopic moisture in the coal, a small drying bath must be made too; but W. R. Johnson's coal assays have shown that the hygroscopic water in ordinarily well dried coals (not brown coals) is of little importance.

The size of the crucible allows the coking of 200 to 600 or more milligrammes of coal, according to the dryness of the coal and the extent of its swelling up when heated; and as the blow-pipe scales (of Lingke's make) weigh within a tenth of a milligramme, it is easy to weigh within much less than a tenth of one per cent. of the amount of coal assayed, much nearer, in fact, than the exactness of the coke assay in other respects. In this point, indeed, the blow-pipe assay is quite as good as the assay with the larger scales, especially the muffle assay, where the coal must be brushed into a clay receptacle after weighing, and the coke or ashes brushed off from it before weighing; while here the crucible is weighed each time without removal of its contents, and without danger, therefore, of losing anything or adding any dust. It may be objected that the smallness of the amount of coal that can be assayed with the blow-pipe makes it a less trustworthy indicator of the general composition of the coal than a larger assay; but the size of the lumps or powder assayed may be made finer accordingly, so that when mixed up, an equally just sample of the whole mass would be got for the small assay as for the large.

Any one who has a little experience, both in the use of the blow-pipe and in the ordinary muffle assay of coal, would scarcely need any further teaching for the coal assay with the blow-pipe. For others, it is worth while to say that the coal may be assayed either in a fine powder or in little lumps, and either with a slowly increasing or with a quickly increasing heat. A quick heat will give less coke by several per cent., but will often make a dry coal cake together that would not cake with a slow heat. The cover of the crucible should be left open a little crack, for the easy escape of the gas, but covered enough to prevent any flying off of solid material. The heat should increase to redness, and as soon as the escaping gas stops burning the heat should be stopped. As some coals part with their gas more quickly than others, of course no definite time can be fixed for heating all coals; but the burning of the gas is a good enough sign. Care should be taken not to let the coke take up moisture from the air before weighing, as it will quickly do if it has a chance. Of course, owing to the different effect of quick or slow heat, a certain uniformity of result, even with perfectly uniform samples of coal, can only be got, without error, by practice and by mechanical skill, by reproducing with nicety the same conditions in successive assays.

After the coke has been weighed, it can be heated again with very free access of air, say, with the crucible tilted to one side, with the cover off, until everything is thoroughly burnt to ashes; and these should be re-heated until no change for the less is made in the weight. With free burning soft (semi-bituminous) coals this burning to ashes is very slow, so that it is very fatiguing or even impossible to carry it out with the blow-pipe; but in that case the crucible may be heated over a Bunsen gas-burner or an alcohol lamp, and left to glow for hour after hour. For the matter of that the coking is far more conveniently done in the same way than by blowing with the mouth.

Here is a pair of blow-pipe assays, made five years ago, of some West Virginia asphaltum, that seemed it-

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* American Journal of Mining.

[English Edition, Vol. XVII., No. 435, pages 161, 162.]

No. 1	47·29 per cent.	52·71 per cent.	1·65 per cent.
No. 2	46·93 "	53·07 "	1·81 "
Mean	47·11 "	52·89 "	1·73 "

SWEET PRINCIPLE OF FROZEN POTATOES.

BY DR. A. OTT.

At the meeting of the Polytechnic Branch of the American Institute, January 16th, Dr Adolph Ott, well known as the author of a useful and interesting work on soap and candles, detailed the results of his recent investigations and experiments with potatoes, his object being to determine the sweet principle of frozen potatoes.

With a view to determine this question, he exposed 1 lb. of this vegetable to a very low temperature, and when thoroughly frozen, he first reduced a ½ of a lb. of it to a pulp, by rasping, and expressed the sap, to 100 centimetres of which (33·8 fluid ounces) he added 10 cubic centimetres of basic acetate of lead; and this, filtered, and transferred to the glass tube of Mitscherlich's polarimeter, and placed between Nichols' prisms, gave no rotation of the plane of polarisation, thus establishing the fact that no sugar is present in raw, frozen potatoes. To decide whether sugar is formed in the process of cooking, he steamed 250 grm. (nearly 9 oz.) of the vegetable for one hour, and mashed them with 200 cubic centimetres of tepid water. The solution thus obtained was divided into two portions, from one of which was precipitated the gummy and protein matters with sub-acetate of lead, also discolouring the reddish brown liquid with a few drops of sulphuric acid. This, placed as before in the tube of the polarimeter, gave a change of colour, indicating the presence of uncrystallisable sugar. No calculation, however, was made from this, as the rotation of the grape sugar to the left is partly compensated by the dextrine and other substances; which are right-handed in respect to polarised light, and which are generally the product of heat upon albuminous starchy matter. He therefore had recourse to Fehling's test, by using the infusion of the boiled potato which had been set aside, by which the following elements were determined:—

1st. The percentage of sugar in the infusion.
2nd. The amount of water in the boiled potato.

Thus the percentage of sugar in the latter was calculated, and found to be 1·45 per cent. What, then, is the cause of the sweet principle? He would answer by saying that in freezing and thawing, the sap of the potato bursts the cells, and thus destroys vitality; at the same time, decomposition is setting in, which, though retarded by the cold, is not entirely arrested; the more so as, at the season most likely to freeze, and especially during a snow-storm, there abounds that powerful oxidising agent, ozone.

No doubt the outer portions of the starch grains are first attacked by it, and may thus be transformed into *diastase*, a body which, as we know, possesses the same power as dilute acids of converting a comparatively large quantity of starch, first into dextrine, and then into sugar, at a temperature of 140° to 170°, as in the process of cooking. Wheat contains enough diastase, as does every seed when sprouting, to convert all its

The outer layers of the starch grains contain the albuminous bodies.

ON NAPHTHA AND ILLUMINATING OIL FROM HEAVY CALIFORNIA TAR, AND ON THE PROBABLE ORIGIN OF PETROLEUM.

BY PROF. B. SILLIMAN.

Having lately had an opportunity to examine a specimen of "surface oil," so called, from Santa Barbara county, in California, I present the following experimental results in the hope that they may not be without interest, as an addition to our knowledge of one extreme of that class of hydrocarbons, which occur in nature in the fluid form, and of every density, from those which are but little lighter than water, down to the lightest naphtha found in a natural state.*

It is proper to state that the chemical examination of this sample had chiefly a technical object, to prove whether or not illuminatiug oil of good quality could be obtained from the distillation of so dense a body. The experiments were conducted on quantities of from five to ten gallons each. The crude oil was very dark, almost black, transmitting yellow brown light in thin films. At ordinary temperatures (60° F.) it is a thick viscid liquid resembling coal tar, but with only a very slight odour.

Its density at 60° F. is 0·980, or 13½° Baumé. It retains, mechanically entangled, a considerable quantity of water, which is neutral in its reaction. The odour of sulphydric acid, which is very decided in this product, as I have noted in its locality, had entirely disappeared in the specimen under consideration.

The tar froths at the commencement of distillation, from the escape of watery vapour. It yields by a primary distillation no product having a less density than 0·844 or 37° B. at 52° F.

Distillation to dryness produced in two trials an average result as follows:—

Oil having a density of 0·890 to 0·900	69·82
Coke, water, and loss	30·18
	100·00

In one of these trials the product was divided ɾ follows:—

Oil of density 29° B. at 52° (885 sp. gr.)	
Oil of density 24·75 B. at 58° (908 sp. gr.)	
Coke, water, loss, &c	

The coke is very large in quantity, stron good fuel, resembling gas-house coke. Th ammonia is given off towards the close of tion. It is well known to distillers of pe by the process called "cracking," heavy illumination are broken up into bodies from light naphtha to the heavier illu bricating oils. This process is simp

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* I am indebted to A. J. Corning, formerly for conducting this research under my direct and Merrill, of the kerosene works in Sout obligations for the permission to employ conducting this research. For the crude debted to the California Petroleum Con derived.

[English Edition, Vol. XVII., No. 435, pages 162, 163; No. 436, page 1

of a carefully regulated heat producing a slow distillation. By this treatment the molecules apparently rearrange themselves into groups of different density, which by a subsequent distillation are divided into fractions (or "heaps," as Mr. Warren calls them) of tolerably constant boiling points.

The first distillate, having a density of about ·890 at 6° F., treated in this manner, yielded a product having a density of about ·885 at 60°, or only 1° Baumé lower than before distillation. After treatment with sulphuric acid and soda and redistilling from soda, it had a density of ·880 at 60° F. Upon distilling, 100 measures of this last distillate yielded:—

Light oil having a density of about ·835 at 60° F....	21·58
Heavy oil having a density of about ·880 at 66° F...	37·41
Heavy oil having a density of about ·916 at 64° F...	34·53
Coke, &c..................................	6·48
	100·00

In another experiment undertaken with a view to "cracking," &c., treating and re-distilling with soda, the products were as follows, stated in percentages of the whole quantity operated on, the several steps being as before:—

Naphtha,* sp. gr. about ·760 at 60° F	11·33
Oil,† sp. gr. about ·836 at 60° F....................	66·22
Oil: sp. gr. about ·893 at 60° F.....................	12·67
Oil, sp. gr. about ·921 at 60° F.....................	3·56
Loss......................................	6·22
	100·00

The illuminating oil from both these experiments, after treatment with sulphuric acid and soda in the usual manner, acquired an agreeable odour, a light straw-yellow colour, and burned as well in a lamp as good commercial oil.

With a view to test the effect of heat aided by pressure in breaking up the heavy hydrocarbons—a method of treating heavy hydrocarbon oils patented in 1866 by James Young, of Glasgow—a portion of the first distillate from the crude oil was subjected during distillation to a pressure of 10 to 15 pounds to the square inch, in an apparatus adapted to the purpose, the distillate thus obtained being about the same density as in the first-named experiment, ·890 at 60° F.

From this distillate were obtained, after the ordinary treatment with sulphuric acid and soda, the following products:—

Light oil, sp. gr. ·825 at 60° F.......	19·2	per cent.
Heavy oil, sp. gr. ·885 at 60° F.......	25·86	"
Heavy oil, sp. gr. ·918 at 60° F.......	38·14	"
Coke, loss, &c.......................	16·80	"
	100·00	

The illuminating oil from the last experiment flashed at 80° F., and lighted on the surface at 85° F., showing the presence of naphtha or some very light body, the quantity of which cannot be very considerable. The light oil could, with care, be taken off in practice without materially diminishing the yield of illuminating oil. It would be rash to conclude that there may not be an important economical advantage in employing in the large way, Mr. Young's method of treatment, under pressure, over that of "cracking" by a regulated heat alone. It is highly probable that there would be found an important saving of time, as under a regulated pressure, and a corresponding increase of temperature, the transformation of the heavy oils into a mixture of less density will occur more speedily. The experiments herein mentioned gave nearly the same result, whether pressure was used or not; a certain loss, all falling upon the lighter portions, was found to result from leakage of the apparatus under pressure, which in the larger way of operating commercially could be avoided.

No paraffine could be detected by refrigerating the heavy oils obtained in these distillations in a mixture of salt and ice. It is, no doubt, the absence of this body from the series of products obtained from the California oils generally, that accounts for the illuminating oil burning well at a density considerably below the commercial standard for oil obtained from Pennsylvania petroleum—a difference enhanced also by the absence of any considerable quantity of light naphtha. The lubricating oils of this series, likewise free from paraffine, retain on this account their fluidity at low temperatures.

The light oils obtained in this series of experiments correspond respectively to 12·96, 14·56, and 18·96 per cent. of the crude oil. The total commercial products are about 60 per cent. of the crude body, which likewise yields sufficient coke to supply the fuel required in the distillations.

In the large way of returning the lightest oils to the heavier portions in the successive distillations, and employing Mr. Young's method by pressure, it is probable the product of light or illuminating oils may be raised in these very heavy natural products to 30 per cent., and for those of less density the proportion will be correspondingly greater.

It is evident from these experiments that heavy hydrocarbon oils containing no naphtha are convertible into oils of the naphtha series under the action of heat by molecular transformations, the excess of carbon being left behind as coke; each successive distillation eliminating a new, but always diminished, portion of carbon. It may, therefore, be confidently affirmed, that even the heaviest of the California hydrocarbons belong to, and are derivatives from, the petroleum series. The transformation of light oils into denser products ending with tar, like that which is the subject of this research, results not, as has been assumed by some, from the addition of oxygen producing an oxidised body, but on the contrary, by the removal of successive atoms of hydrogen in the form of water, thus leaving the carbon in excess, that excess being left behind in the form of coke when the crude product is distilled.—*From the San Francisco Bulletin.*

ON THE PRECIPITATION OF COPPER AND NICKEL BY ALKALINE CARBONATES.

BY WOLCOTT GIBBS, M.D.,

RUMFORD PROFESSOR IN HARVARD UNIVERSITY.

The precipitation of copper by zinc, or by the electrolytic method, requires that the metal should be present in the form of sulphate or chloride, and does not succeed with the nitrate. As stated before, the employment of the hypophosphites is limited to the case in which the metal exists as sulphate. The old mode of precipitating copper as oxide by caustic potash has disadvantages which are familiar to all chemists, but on the other hand, is independent of the nature of the solu-

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[*] This naphtha caught fire from a match at an atmospheric temperature of 56° F.

[†] This oil flashed at 113° F., and ignited at 124° F.

tion of copper employed, so long at least as no organic matter is present. According to Rose,* the alkaline carbonates precipitate copper less completely than caustic alkalies. This statement, however, is not accurate for *all* the conditions under which the experiment may be performed; and I have found that copper may be completely precipitated from the sulphate, nitrate, or chloride when the solutions are boiled together for a sufficient time and are sufficiently dilute. Mr. E. R. Taylor, who has made a careful study of this method of determining copper, has arrived at the following as the best method of conducting the process. The solution of copper is to be diluted with water until the liquid contains not more than about 1 grm. of the metal in one litre. A solution of carbonate of potash or soda is then to be added in small excess, and the whole boiled for about half an hour. The boiling proceeds quietly, and without succussions; the blue green carbonate soon becomes dark brown, and has a fine granular character which renders it extremely easy to wash. After washing it is to be ignited in an atmosphere of hydrogen, and the copper weighed as metal; it will be found to be free from alkali. In this manner Mr. Taylor obtained, in five analyses, the following results:—

1·8384 gr. pure sulphate of copper gave 0·4688 gr. metallic copper = 25·44 per cent.

1·7144 gr. pure metallic copper dissolved in aqua regia gave 1·7161 gr. copper = 100·09 per cent.

1·3860 gr. pure metallic copper dissolved in aqua regia gave 1·3853 gr. copper = 99·93 per cent.

1·4657 gr. pure metallic copper dissolved in nitric acid gave 1·4670 gr. copper = 100·09 per cent.

1·4685 gr. pure metallic copper dissolved in nitric acid gave 1·4634 gr. copper = 99·65 per cent.

The filtrate is perfectly free from copper if the process has been well conducted.

The ignited oxide is in a state of great subdivision, and the ignition must therefore be conducted with much care to avoid loss. A small portion of the oxide or basic carbonate usually adheres to the sides of the vessel in which the boiling takes place. This is to be re-dissolved, and again precipitated, but great care must be taken not to add a large excess of the alkaline carbonate, which gives a solution from which the copper is not precipitated by boiling.

Nickel may be completely precipitated from its solutions by precisely the same process. The green basic carbonate may be washed much more readily than the oxide precipitated by caustic alkali; it is to be ignited and weighed as oxide. In two analyses Mr. Taylor obtained the following results:—

1·9808 gr. anhydrous sulphate of nickel gave 0·9551 gr. $\text{Ni}\Theta$ = 37·79 per cent.

1·4601 gr. anhydrous sulphate of nickel gave 0·7008 gr. $\text{Ni}\Theta$ = 37·64 per cent.

The formula $\text{NiS}\Theta_4$ requires 37·69 ($\text{Ni} = 58$). Dr. F. A. Genth informs me that he has also used the alkaline carbonates in precipitating nickel, and with most satisfactory results.

The precipitation of cobalt by an alkaline carbonate can only, with much difficulty and by long boiling, be made complete. As a means of determining cobalt it is not to be recommended. On the other hand, Mr. F. W. Clarke has found that cobalt is completely and easily precipitated by the process of oxidation, first given by Popp,† which consists in neutralising the solution with carbonate of sodium, adding acetate of

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* Handbuch der Analytischen Chemie, ii. 175. Sechste Auflage.
† Zeitschrift für Analytische Chemie.

* Sechste Auflag
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It is frequently associated with the salts of metals, and beautiful crystals, of galena, or sulphide of lead, chalybite, or brown spathic carbonate of iron, resembling spar in lustre; copper pyrites with silver, iron pyrites, &c., are found therein, arranged in masses segregated from the white, transparent, ice-like cryolite.

It remained for the Pennsylvania Salt Company to introduce to our country this valuable material. This energetic Company, whose works are in Western Pennsylvania, has secured the privilege of using a large part of all that is mined, and has, within two years past, imported into Philadelphia thirteen cargoes, or 9,000 tons, which have been sent to their works for manufacture.[*] The greater portion of this has been used for their patent saponifier. They are now devoting their attention to the preparation of caustic soda, carbonates, and other salts of soda, sulphate of alumina, &c.

Soda is obtained from cryolite by simply mixing with lime, and subjecting to heat. The fluorine combines with the calcium, forming fluoride of calcium; while the remaining metals absorb oxygen from the air, and become alumina and soda. Carbonic acid is then passed through the solution, forming, with the sodium, a carbonate of soda, which remains suspended, while the alumina, being insoluble, is deposited at the bottom of the vessel. The carbonate of soda is deprived of its acid by means of lime in the usual manner, and thus rendered caustic, and fitted for the use of the soap-maker.

One hundred pounds of cryolite yield—

44 lbs. dry caustic soda,
or 75 " " carb. "
or 203 " crystal carb. "
or 119½ " bicarb. "
and 24 " alumina.

The sulphate of alumina contains 2·82 of sulphuric acid to 1 equivalent of alumina, therefore this is more than a neutral salt (3· being neutral), which is very desirable for manufacturers of paper, calico printers, &c. It is also entirely free from iron, another very important characteristic.

There is another very important use to which cryolite can be applied. By a fusion of 1 part of cryolite with from 2 to 4 of pure silex, a beautiful glass is formed, susceptible of mould and polish, and capable of being manufactured into an endless variety of useful and ornamental articles, and probably many utensils for chemical and pharmaceutical use will be made of it. A company has been operating in Philadelphia for some time past, on an experimental scale, entitled the "Hot Cast Porcelain Company." The results have been so satisfactory that they have now taken a large establishment, and will be prepared to carry on the manufacture quite extensively. The cost is, at present, from 10 to 20 per cent. higher than ordinary flint glass. The ware seems to be stronger than glass.—*Proc. Am. Pharm. Association.*

A NEW PRECIPITANT FOR POTASH, &c.

RESEARCHES ON THE COMBINATIONS OF MOLYBDIC ACID WITH PHOSPHORIC ACID.[†]

BY M. H. DEBRAY.

At the beginning of this century, after Berzelius had determined by numerous and delicate analyses the composition of most of the then known mineral substances, chemists were struck with the simplicity with which their composition could be expressed by means of the proportional numbers given by his researches. This character of remarkable simplicity to which we have now been long habituated, appeared to distinguish Mineral from Organic Chemistry, in which complicated formulæ consequent upon the infinite variety of bodies formed from a small number of elements are the general rule.

The distinction is disputed by most of the eminent chemists of the present day. There is indeed no essential difference between the reactions of organic and those of inorganic chemistry, neither have the compounds in the latter that degree of simplicity which some like to attribute to them.

The discovery of the silico-tungstic acids and their salts, by M. Marignac, has recently furnished a very remarkable illustration of a series of bodies of very complex composition, and yet possessing a sharpness of reactions and a beauty of crystalline form at least as great as the simple products of our laboratories. The study of the combinations of phosphoric and molybdic acids has led me to the discovery of bodies of the same order, of a still more complicated composition, but as well defined and as perfectly crystallised as the compounds of silico-tungstic acid.

I. It is known that the solution of molybdate of ammonia in nitric acid possesses the property of precipitating ordinary phosphoric acid, giving a yellow body almost insoluble in all acids. This precipitate contains about 89 per cent. of molybdic acid, a little over 4 per cent. of phosphoric acid, the rest being ammonia and water. Upon boiling this in an excess of *aqua regia*, the ammonia is destroyed, and a yellow liquid obtained which yields by spontaneous evaporation beautiful doubly oblique prisms of a yellow colour, which consist of a combination of one equivalent of anhydrous phosphoric acid with twenty equivalents of anhydrous molybdic acid, and a certain quantity of water, corresponding to 13·3 per cent. of the weight of the hydrate.[*]

These crystals, very soluble in water, can form two other hydrates; the one containing 23·4 per cent. of water, that is to say, double that contained in the first for the same quantity of anhydrous acid; the other only 19·6 per cent. The hydrate with 23·4 per cent. is obtained by the spontaneous evaporation of aqueous solutions of phospho-molybdic acid in large regular octohedra; the hydrate with 19·6 per cent. is deposited from concentrated solutions strongly charged with nitric acid; these crystals, less beautiful and stable than the preceding, belong to the rhombohedral system.

The small quantity of phosphoric acid which unites in these compounds with molybdic acid (3·7 to 4·1 per cent.) suffices to modify profoundly its properties. True molybdic acid may give a soluble hydrate which was first isolated by Mr. T. Graham in the dialysis of acid solutions of molybdates, and more recently by M. Ullik from molybdate of baryta and sulphuric acid,[†] but this hydrate gives colourless solutions and is uncrystallisable, while the hydrates of phosphomolybdic acid are yellow and easily crystallisable. Moreover, the re-

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[*] They imported last year (1867) eight thousand tons.

[†] This memoir, recently read before the French Academy, appears so important that we have decided to give it uncondensed, notwithstanding its length and the numerous demands upon our space at this season. —Ed. C. N.

[*] I have already pointed out this reaction in a former paper on Molybdenum (*Comptes Rendus*, xlvi., 1098), but I did not continue the study of these bodies, not having then a convenient method of analysis.

[†] Ann. d. Chem. u. Pharm., cxliv., 204.

actions of this acid differ essentially from those of molybdic and phosphoric acid. Thus whilst all molybdates are soluble in acids, we find that phosphomolybdic acid precipitates from their strongly acid solutions, potash, the oxides of rubidium, cœsium and thallium, ammonia and the nitrogenous organic alkaloids. Soda and lithia, which give no precipitate under these conditions, separate themselves by this reaction, as by many others, from potash and its congeners, whilst thallium approaches it in a decided manner.

The metallic oxides are not precipitated by phosphomolybdic acid in a sufficiently acid solution.* Oxide of bismuth is not an exception, although it forms with phosphoric acid a compound almost insoluble in nitric acid, as M. Chancel has shown; moreover, the mixture evaporated deposits crystals of phosphomolybdic acid in the acid solution of bismuth.

If it is demonstrated that a well-defined body can result from the combination of one equivalent of one substance with twenty of another, there is no reason why there may not some day be discovered still more complex combinations. It will, therefore, be important to examine if the substances which we constantly find, in minute quantity it is true, in a great number of minerals, do not form an integral part of those minerals, the same as more abundant substances, and do not communicate to them special characters. This fact, demonstrated as far as the association of fluorine and chlorine with phosphates, may extend to many other combinations. I may be permitted to remark that if there exist definite combinations of iron and carbon, it will not be necessary to imagine a relation very different to that governing the combination of molybdic acid with phosphoric acid, to obtain bodies having nearly the composition of iron and steel. Thus the compound CFe_{30} would contain only 0·72 per cent. of carbon ($Fe = 28$, $C = 6$)

II. The composition of the yellow phosphomolybdates of potash, thallium, and ammonium, obtained by precipitating these bases in acid solutions, may be represented by the general formula,

$$3RO,\ PO_5,\ 20MO_3;$$

the salts of potash and ammonia also contain 3 equivalents of water of hydration.

These are well-defined compounds, and not mixtures, for it is easy to obtain them crystallised. It is sufficient to fuse, at a dull red heat, the salts of potassium and thallium, to obtain an oily liquid, solidifying on cooling to a mass of crystals; in the thallium salt these crystals are sufficiently distinct and brilliant to admit of the hexagonal pyramid which terminates them being distinguished with the naked eye. A few grammes of material are sufficient for these experiments.

The ammoniacal salt is obtained in small yellow brilliant crystals by mixing two solutions of pyrophosphate of soda and acid molybdate of ammonia; the precipitate is produced slowly in consequence of the transformation of the pyrophosphoric acid into ordinary phosphoric acid under the influence of the acid liquid.

A solution of phosphomolybdic acid precipitates neutral nitrate of silver. The precipitate gradually changes to microscopic crystals, the composition of which may be represented by the formula:—

$$7AgO,\ PO_5,\ 20MO_3 + 24HO.$$

This salt dissolves in dilute nitric acid, and the liquid on evaporation furnishes small yellow brilliant crystals of a salt containing two equivalents of base,

$$2AgO,\ PO_5,\ 20MO_3 + 7HO.$$

III. Phosphomolybdic acid and its salts are only stable in the presence of acids; alkalies generally change them into ordinary molybdates and into phosphomolybdates, in which the two acids are united in the more simple proportion of 1 to 5,

$$(PO_5,\ 20MO_3) + Aq = PO_5,\ 5MO_3,\ Aq + 15MO_3.$$

These phosphomolybdates are colourless or nearly so, and have a pearly appearance; they are soluble in water and easily crystallisable; an excess of acid reconverts them to the state of yellow phosphomolybdates, setting phosphoric acid free,

$$4(PO_5,\ 5MO_3) + Aq = 3(PO_5,\ 3HO) + PO_5,\ 20MO_3 + Aq.$$

I give below the formulæ of some of the beautiful salts of this new class of phosphomolybdates:—

Ammonia salt	$6(NH_4O)$,	$2PO_5,\ 10MO_3 + 14HO$,
Potash	"	$6KO$,	$2PO_5,\ 10MO_3 + 14HO$,
Soda	"	$6NaO$,	$2PO_5,\ 10MO_3 + 28HO$,
Silver	"	$6AgO$,	$2PO_5,\ 10MO_3 + 14HO$.

It would appear that these formulæ could be simplified by dividing all the terms by two, but as the action of acids furnishes a fresh type of salts equally well crystallised, represented by the general formula,

$$5RO,\ 2PO_5,\ 10MO_3 + Aq,$$

it is preferable to retain for the first salts the formulæ assigned to them.

Some of these salts may also form double salts with nitrates: I give one of these compounds:—

$$[6(KO,\ NO_5) + 6KO,\ 2PO_5,\ 10MO_3] + 18HO.$$

The facility with which the phosphomolybdic acid giving white salts changes into yellow phosphomolybdic acid and phosphoric acid, has prevented me hitherto isolating it.

IV. The analysis of the preceding compounds presents very great difficulties when recourse is had to the hitherto known methods of separating the bodies composing them. I have used to effect this object two new processes which deserve notice, as they are susceptible of generalisation.

Separate the phosphoric acid from the molybdic acid by passing over a mixture of phosphomolybdic acid and lime, heated to incipient redness in a porcelain boat, first a current of sulphuretted hydrogen, then of hydrochloric acid. There are formed chloride of calcium, sulphide of molybdenum crystallised like the native sulphide, and chlorophosphate of lime, or apatite, also crystallised. The chloride of calcium is removed by water, and the apatite by hydrochloric acid, which does not attack the sulphide of molybdenum; the latter being easy to wash and collect, is carefully weighed. The phosphoric acid is easily estimated in the hydrochloric solution.

When alkaline phosphomolybdates are under examination, a portion of the alkali transformed into chloride volatilises at the high temperature of the operation in the gaseous current; to estimate the alkali recourse must be had to the following process:—Dissolve the phosphomolybdate in excess of ammonia, and add to the solution ammoniacal nitrate of silver; on ebullition crystallised tribasic phosphate of silver is first obtained,

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* I have already shown this property of phosphomolybdic acid to the Chemical Society of Paris (*Bull. de la Soc. Chim.*, 2, v., 405) but without studying the compounds so formed. Long before my researches M. Sonnenschein had pointed out for the precipitation of organic alkaloids, a reagent obtained by treating with an excess of soda the precipitate of phosphomolybdate of ammonia, to drive off the ammonia, and dissolving the residue in nitric acid; it is now clear that this reagent is nothing more than a salt of phosphomolybdic acid, which behaves like the acid itself.

and then colourless molybdate of silver equally crystallised; the alkali remains in the liquid, where it is easy to determine it.

ON THE

FORMATION OF DOUDLE SULPHOCYANIDES OF CERTAIN OF THE ALKALOIDS, &c.

(Continuation.)

BY WILLIAM SKEY,

ANALYST TO THE GEOLOGICAL SURVEY OF NEW ZEALAND.

Since my last communication I have found that platinum, gold, iron, tungsten, and chromium should be added to the list of metals capable of forming insoluble double sulphocyanides with the alkaloids generally and sulphocyanogen, and others no doubt remain to be added especially from the platinum group.

Annexed are short descriptions of some of the more characteristic of these salts—

Sulphocyanide of Platinum and Morphia precipitates as a red oily looking transparent substance, when a solution of ammonio-bichloride of platinum is brought in contact with a mixed solution of a salt of morphia, and a soluble sulphocyanide; it does not give any reaction of sulphocyanogen with the perchloride of iron, except it is first decomposed by an alkali. If quina is substituted for morphia a yellow crystalline precipitate falls, which melts at a temperature under 200° F. to a yellow oil. The nicotina salt with platinum is a dark red crystalline substance, which as in the case of the morphia salt has to be decomposed by an alkali before the sulphocyanogen reveals its presence by the iron test.

Sulphocyanide of Gold and Atropia, obtained by adding an atropia salt to a solution of sulphocyanide of gold in sulphocyanide of potassium, forms semi-spherical red oily looking drops, adherent to the vessel's sides; at a temperature under 212° they agglomerate to a viscid substance.

The Quina Salt with Gold appears as granular crystals, adherent to enclosing vessel. A morphia salt gives no precipitate with this solution of gold.

Sulphocyanide of Iron and Nicotina is an oil at common temperatures, blood red by transmitted, and green by reflected light; its lustre is metallic.

The morphia and quina compounds with iron are plastic and adhesive at common temperature, while veratria yields pale red crystals, readily soluble in water, the other salts of iron mentioned being but slightly soluble.

Sulphocyanide of Tungsten and Quina falls as a gelatinous yellowish precipitate when an alkaline tungstate with excess of a soluble sulphocyanide is acidified and brought in contact with a solution of this alkaloid; it fuses at a low temperature, gradually acquiring a green colour.

The other alkaloids generally furnish gelatinous precipitates with this solution of tungsten.

A solution of the red sulphocyanide of chromium also furnishes flaky or gelatinous precipitates with the alkaloids generally; with nicotina, however, a semi-solid translucent substance forms, adherent and continuous, of a purplish colour, insoluble in alcohol or ether; heated with solution of potash it fuses, turns to a pink colour and evolves nicotina; it also gives the reaction of chromium and sulphocyanogen.

Certain of the alkaline bases also appear to form double sulphocyanide salts with certain metals and sulphocyanogen, for when mixed with a soluble sulphocyanide and shaken up with ether, the ether is but slightly coloured, but on the addition of a salt of zinc or tin the whole of the colouring matter can be removed to the ether by further agitation; the etherial solution from the tin when removed on to water and boiled, furnished oily looking blue globules which sank through the water; when long heated it becomes solid, it gave abundant indications of tin and sulphocyanogen. The presence of water or ether has not been tested for.

As before stated, these double sulphocyanides are generally characterised by their comparative insolubility in water; the degree of this, however, has only been determined in the case of the strychnia compounds with zinc, which requires about 30,000 parts of water to every part of strychnia present; it is therefore as insoluble in water as tannate of strychnia.

The behaviour of veratria with iron leads to the supposition that in those cases where solutions of metal fail to afford precipitates when mixed with the alkaloids in presence of hydrosulphocyanic acid, double sulphocyanides may still be formed.

As being connected with the subject in hand the following particulars relative to the formation of other double salts are noted here.

A double sulphocyanide of mercury and zinc falls as a crystalline precipitate when a salt of zinc is mixed with a solution of sulphocyanide of mercury; it is almost insoluble in water; no precipitate is formed with cadmium or tin in place of the zinc. Possibly a good process for the separation of zinc, cadmium, and tin from each other could be based upon their respective deportments with mercury or the alkaloids in presence of sulphocyanogen.

Double iodides of mercury or platinum with the alkaloids also form when the proper solutions for the same are mixed together; the sulphocyanides of mercury with the alkaloids are light coloured, and insoluble in excess of iodide of potassium. The double sulphocyanide of platinum and strychnia is a dark bluish red crystalline substance, feebly soluble in water, more soluble in sulphocyanide of potassium. The other sulphocyanides are generally still more insoluble in water or sulphocyanide of potassium; their colour is also bluish red.

January 17, 1868.

ON THE CHEMICAL REACTIONS IN THE ROASTING OF PYRITES.*

BY J. H. TIEMANN, JUN.

Ever since pyrites has been used in the manufacture of sulphuric acid, it has been frequently noticed that instead of the roasting chamber being filled with an invisible mixture of atmospheric air and sulphurous acid, visible white fumes arise from the glowing pyrites. This has been noticed more particularly in muffle furnaces (where the pyrites is roasted in a chamber heated from the outside), and especially in Spencer's muffles, which are fifteen metres long. It is hardly possible that this occurs only in muffle furnaces; its occurrence here is more readily perceived than in other furnaces—"kilns," for instance. These white vapours, as is known, are anhydrous sulphuric acid. The condensation of liquid sulphuric acid in the flues connecting with the leaden chambers, is undoubtedly owing to this cause. The fact that these fumes are frequently, if not

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* Dingler's Polytechnic Journal, vol. 187, part 2.

always formed, induced Mr. Fortman, at the laboratory of the Collegium Carolinum, in Brunswick, to institute a number of experiments on the subject.

The material used was a piece of nearly pure iron pyrites, with well-defined crystals, interspersed with quartz; analysed in the ordinary way, it yielded 50·21 per cent. of sulphur. The pure pyrites FeS_2 requires 53·3 per cent. sulphur. For all the experiments, the pyrites was reduced to a very fine soft powder.

The powder was placed in a hard glass tube ¾ inch wide, which was placed in a Liebig's combustion furnace (the same as for an organic analysis), and brought to a red heat, while air was drawn through the tube by an aspirator. With careful and gradual heating, the point at which the pyrites ignites may easily be seen at the same moment with the first evolution of sulphurous acid; the white fumes make their appearance, and continue without interruption during the whole time of roasting. In the first experiment, the tube was connected with a flask containing a solution of caustic soda, and a tube with dry caustic soda between it and the aspirator. It was at once apparent that the white fumes passed through both absorbents into the aspirator; the absorption was, therefore, very incomplete, although all the sulphurous acid was absorbed. It is known that anhydrous sulphuric acid mixed with air or other gas, is very difficult to condense. It is, therefore, not surprising that in this case the white fumes were imperfectly retained; the quantity of the uncondensed portion (principally anhydrous acid) was remarkably large, as shown by analysis. 1·5 grammes of powdered pyrites were roasted; the dry caustic soda was dissolved in the solution of soda in the flask; this was oxidised with chlorine, and precipitated with chloride of barium, which gave 3·657 grammes sulphate baryta—corresponding to 33·49 per cent. sulphur. According to which, 16·72 per cent. of the sulphur in the pyrites was lost—as anhydrous sulphuric acid [50·21—33·49=16·72.] The roasted pyrites retained a small portion of sulphur at those points where it was in direct contact with the combustion tube. This evil was corrected by frequently turning the tube during the roasting, and the mass was heated gradually from the front of the tube towards the rear; in this way a residue was obtained entirely free from sulphur, and of a bright red colour.

In the above experiment, it was found that the dry caustic soda absorbed more rapidly than the solution; the soda tube was therefore changed for one three times larger, and the experiment again made, giving the following results:—Roasted pyrites, 1·366 grammes. The gases, as before, first passed through soda in solution, and then through the tube with dry soda. At the end of the operation, the soda in both was neutralised; the solution divided into two portions; one portion was immediately precipitated with chloride of barium; the other half was first oxidised with chlorine and then precipitated. The first gave 2·485 grammes, the latter 1·986 grammes, of sulphate of baryta, so that in 100 parts pyrites—

Sulphur, absorbed as sulphurous and anhydrous sulphuric acid	48·39 pts.
Sulphur, absorbed as anhydrous sulphuric acid alone	38·31 "
Sulphur, absorbed as sulphurous acid	10·08 "

So that from 50·21 per cent. sulphur in the pyrites, 1·82 parts were lost by incomplete absorption (50·21—48·39=1·82). According to which, the amount of sulphur eliminated as anhydrous sulphuric acid, was almost four times as great as that which passed off as sulphurous acid. An apparently unlikely result, especially when we consider that the insolubility of the sulphate of baryta in a concentrated solution, or an imperfect roasting of the precipitate, may have easily caused an error.

A more perfect result was expected by determining the sulphurous acid with iodine solution by the burette. In so far as the roasted pyrites retains more of the sulphur, and the sulphurous acid is entirely absorbed, this method must give a perfect way of determining the relation between the two acids. The anhydrous acid can easily be determined by the difference between the sulphur in the pyrites and in the sulphurous acid formed.

1·549 grammes pyrites was roasted as before; a. the caustic soda was united in one solution, and the solution was diluted to exactly 1·000 cubic centimeters. 10 c. c. treated by the well-known method, required 1·75 c. c. iodine solution (1 c. c. iodine solution=0·00032 sulphurous acid); so that again, according to this experiment, the amount of anhydrous sulphuric acid is much greater than the amount of sulphurous acid.

These preliminary experiments, which will be continued, show that the amount of anhydrous sulphuric acid, formed in roasting pyrites, is very important, and much greater than has been supposed. They show further, that the amount varies, depending upon the temperature and other circumstances which are still to be investigated.

ON THE DETECTION OF METHYLATED SPIRITS BY CHEMICAL REACTIONS.*

BY DR. J. W. GUNNING,

PROFESSOR OF CHEMISTRY AT THE ATHENÆUM ILLUSTRE AT AMSTERDAM, AND SCIENTIFIC ADVISEUR TO THE NETHERLANDS MINISTRY OF FINANCE.

In consequence of the passing of an Act by the Netherlands States General in July, 1865, changing and greatly enhancing the excise duty on spirits, it became necessary to provide the industry of Holland with an alcohol denaturised in such manner as to be unfit for drinking purposes, and for the manufacture of liqueurs, such as Curaçoa, &c. The excellent success obtained with methylated spirits in Great Britain induced the Netherlands administration to allow in a similar manner the use of methylated spirits in the Netherlands. The wood spirit, or methyl alcohol of commerce, is a mixture of various substances which may be met with therein in larger or smaller quantity according to differences in the manufacture, and divers methods of purification. English made wood spirit may, and often does, contain ammonia in consequence of the method which obtains here of saturating the rough pyroligneous liquor with chalk previous to distillation. The methyl-alcohol which is supposed to be chiefly present in wood spirit is very often only met with in really subordinate quantity, beside this, wood spirit contains aceton, acetate of methyl, and probably also formiate of methyl. The presence of compound ethers in wood spirit is easily proved by treating it with caustic potassa, or, better yet, by heating it in a sealed glass-tube with ammonia, and subsequent distillation, when there will be left behind in the retort salts of the acids of the previously existing compound ethers. Aceton can be proved to exist, since if wood spirit is treated with fuming nitrous-nitric acid and a

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* Translated from the Dutch by A. Adriani, M.D. Ph.D. &c.

salt of silver, fulminate of silver is formed. The peculiar odour which wood spirit emits, and which is rather pungent, is not owing to the substances just alluded to, but to peculiar volatile empyreumatic oily substances present in very small quantity, and not separately known in consequence of the difficulty of separating them. It is clear from what has been just stated in reference to wood spirit, that it would be utterly futile and impossible to find for such a compound as a whole a ready and perfect chemical test. One must not lose sight of the fact, moreover, that the compounds met with in wood spirit are neither of them such as are prominent by some or other conclusive and strictly characteristic test, or yield products of decomposition which are highly characteristic; while in methylated spirits, moreover, wood spirit is mixed with ethyl-alcohol, which latter both in chemical and in physical properties bears a great likeness to the former. We are therefore obliged to have recourse to empirical reagents for detecting wood spirit in alcoholic liquids. Although it is true that methyl-alcohol yields on oxidation formic acid and ethyl-alcohol, on the contrary acetic acid, and both these acids, admit of being readily distinguished from each other, the plan to apply this property as a test and reagent is a too cumbrous mode of working to be adopted with ease and economy of time.

The application of the reagents known as Fuch's, to wit, a solution of iodide of potassium, and iodide of mercury in caustic potassa, and that of Reynolds', viz., chloride of mercury in caustic potassa, for the detection of wood spirit, owe their efficacy to the property possessed by the compounds met with in wood spirit to dissolve and keep in solution certain compounds of mercury which are insoluble in pure alcohol and in water. I have laboured in vain to obtain the precise composition of the precipitate which Fuch's reagent yields with pure alcohol, since I have not been able to obtain that precipitate in a sufficiently pure state. As regards the composition of Reynolds' precipitate, it is undoubtedly the yellow oxide of mercury; the following simple experiment proves this: add to a very weak solution of chloride of mercury (corrosive sublimate), a few drops of a solution of caustic potassa, and add afterwards wood spirit, when, on being shaken, it will be seen that the precipitate formed at first is entirely dissolved. While studying these reagents I have in the first place tried to determine whether this solvent action is due to all or only to some of the compounds met with in the wood spirit of commerce; it was of course also necessary to determine what influence is exerted by such substances as essential oils, fusel oil, and compound ethers, which in larger or smaller quantity may and often do occur in such alcoholic fluids wherein it is desired to detect the presence of wood spirit, and to determine whether or not they also exercised a solvent action. As regards the first point I have clearly made out that the reaction just alluded to is not caused by the pure methyl-alcohol, but is owing to other substances met with in commercial wood spirit, and especially to aceton. I have purposely prepared pure methyl-alcohol, both by decomposing pure crystallised oxalate of methyl, and by the well-known chloride of calcium plan, and on experimenting with this pure substance I have found it to behave towards the reagents above alluded to as pure ethyl-alcohol; but the precipitate which obtains is immediately dissolved on addition even of the slightest quantity of aceton. As regards the other point, I have found that many essential oils, fusel oil, and compound ethers exert a disturbing influence on the reaction of the above-mentioned tests, so that as many of these substances cannot be, either at all, or even only partly eliminated from fluids in which it is desirable to test for the presence or absence of wood spirit, the use of the test and reagents becomes either doubtful, or in many cases even impossible. I have therefore found it preferable to modify the composition and application of the reagents in some points, keeping, however, the main principle the same. Fuch's reagent is, as will be easily perceived, Nessler's ammonia test, but in this instance its use is not exactly to detect ammonia; if it so happens, however, that a fluid wherein one desires to test for wood spirit with Fuch's reagent does at the same time contain ammonia, phenomena will be observed indicating the presence thereof, but it will be readily perceived also that then the reactions are more clearly defined and more strongly marked just for the very category of substances wherewith in this case one may have to deal; as a consequence hereof in the majority of instances there need not be the least uncertainty concerning the presence or absence of wood spirit. I give herewith the prescription for making and describe the use of the modified reagent as fit for the purpose for which it is here desired. Dissolve 16·66 grammes of iodide of potassium and 23·08 iodide of mercury in the smallest possible quantity of pure distilled water, this solution is then diluted with 500 cubic centimetres of alcohol, containing at least 92 per cent. of pure alcohol. Also prepare a solution of ammonia, and one of caustic potassa in alcohol of the same strength; it is hardly necessary to say that the solution of caustic potassa in alcohol should be made extempore, *i.e.*, just when wanted. The alcohol to be applied for this purpose ought to be of the very purest quality; the purity may be recognised by applying the reagent just described as a test in the following manner: after the solution of the iodide of potassium and iodide of mercury has been diluted with alcohol, a few drops of the alcoholic ammonia solution should be added to a small portion of the alcoholic solution of iodides, and after that a few drops of the alcholic solution of caustic potassa should be added; there should then ensue a beautifully brown coloured precipitate, similar in pigment to the well-known kermes mineral, without even the slightest yellow tinge. It is very seldom the case that alcohols met with in commerce are so pure, in fact I have found that and the presence of compound ethers and of fusel oil to some extent so alters the reaction of the test just described, that the precipitate obtained is always more or less divergent from the true colour it should exhibit; in order to obtain alcohol fit to be used as above directed, the alcohol of commerce should be treated with animal charcoal, and repeatedly distilled after addition of some caustic potassa. My reason for applying alcohol as solvent [menstruum rather] is that, by its use, one is not disturbed in testing scents, *e.g.*, Eau de Cologne for wood spirit, by the essential oils and resinous substances met with therein, which, if water was applied as solvent for the reagents, would become precipitated. If the reagent just before alluded to be applied to methylated spirit, no precipitate is formed at all, and the fluid remains perfectly clear. It is also the aceton present in wood spirit which prevents the formation of a precipitate with the modified test; but it is clear that the solvent power of this substance has its limits, and that thus by adding excess of the reagent to the fluid to be tested, one might be brought to erroneous conclusions. It is best to take about 10 cubic centimetres of the fluid to be examined, to add thereto, first, one or two drops of the mercurial solution, next, as much of the ammoniacal solution; and, finally, from six to ten drops of the solution of caustic potassa; if no precipitate ensues, then a fresh portion of 10 c. c. should be taken, and the experiment repeated with a somewhat larger quantity of the mercurial solution. If so it happens that the quantity of wood spirit be very small, *i.e.*, if, for instance, it is less than 1 per cent., the fluid under examination will then, after the addition of the caustic potassa, not remain clear, but exhibit either an opalescence, and with some kinds of wood spirit, even become somewhat yellow, and at the same time turbid. I have good reasons to ascribe this phenomenon to a difference in the quantity of the aceton, or of the acetate of methyl usually met with in commercial wood spirit, since, as I will explain more fully presently, compound ethers have the tendency to render the precipitate yellow. I have found it quite possible and easy to estimate even quantitatively within pretty fair range the quantity of wood spirit present in a given sample of an alcoholic fluid. It is therefore, however, best to modify the order of adding the required reagents in this manner that one first adds the alcoholic ammonia solution, next the alcoholic potassa solution, and then drop by drop, and cautiously, and at intervals of time, the alcoholic mercurial solution, until a permanent turbidity sets in; experiments made by me with mixtures of pure alcohol and 1, 2, 4, 5, and 10 volumes per cent. of wood spirit, have proved to me that the number of drops of the mercurial solution applied is pretty fairly proportional to the quantity of wood spirit present in the mixture under examination.

Inasmuch as there might be present in an alcoholic fluid which one should desire to test in the manner described, non-volatile organic substances which would interfere with the proper action of the reagents, it is clear that the non-volatile should be removed by previous careful distillation, while, as regards the volatile organic substances, their disturbing influence may be judged from the following experiments, made by adding to 5 c. c. of pure alcohol a few drops of the under-mentioned substances, and after having well mixed these with the alcohol, the reagents have been added in the usual manner, viz., mercurial solution, ammonia, and caustic potassa.

Amyl alcohol.—With 3 drops, no sensible difference, *i.e.*, the reddish-brown precipitate ensues as with pure alcohol; with 10 drops the precipitate gets a decidedly yellow tinge.

Acetic Ether.—With 3 drops [let it be understood, 3 drops of the ether added to 5 c.c. of pure alcohol], but with addition of half the bulk of the pure alcohol of ether, a reaction ensues, as if a very small amount of wood spirit were present, viz., a faint yellowish opalescence.

Valerianate of Amyl.—(A dilute solution in pure alcohol was applied.) With 10 drops thereof added to 5 c.c. of pure alcohol a reaction ensued, as if a very small amount of wood spirit was present.

Pine Apple Essence.—Reaction as the last foregoing, but less strongly marked.

Essential Oils.—Eau de Cologne, *i.e.*, the same made with non-methylated spirits, yields the same reaction as pure alcohol, but the precipitate is more yellow-coloured. If to 5 c.c. of eau de Cologne, 2 drops of wood spirit [not methylated spirit, of course] are added, there ensues no precipitate at all, and at the utmost, a faint opalescence. Larger quantities of essential oils than are met with in scented waters disturb the action of the reagents in a far higher degree. Alcohol mixed with from 4 to 6 per cent. of the oils of lavender, rosemary, entirely prevent the formation of any precipitate, and act therefore as wood spirit does. Oil of turpentine exerts the same action but in a somewhat less degree.

Sulphuric Ether.—The same reaction as if a small quantity of wood spirit were present.

Aldehyde, *i.e.*, in this case the crude distillate obtained on treating alcohol with bichromate of potassa and sulphuric acid, acts as wood spirit, *i.e.*, no precipitate ensues.

Spiritus Nitri dulcis.—No precipitate.

Muriatic Ether.—The same reaction as wood spirit.

Chloroform exercises no influence; *i.e.*, the reddish-brown precipitate is formed.

Benzol exercises a slight influence; the precipitate is somewhat yellow.

Amylen exercises a very marked influence; the reaction is the same as if a small quantity of wood spirit was present.

These experiments prove that there are some substances which interfere with and more or less disturb the reaction for wood spirit; some of these substances act indeed as if wood spirit itself were present, while others again hinder the reaction, and modify the colour of the precipitate due to pure alcohol only.

As regards the first batch of these disturbing substances, there is no real difficulty to detect them, neither is there the least difficulty to eliminate them from a fluid which it is desirable to test; for distillation with caustic potassa, followed by treating the distillate, previously diluted with water, with animal charcoal, will have the desired effect, while if aldehyde is present, as for instance in the case of spiritus nitri dulcis, a separate distillation with ammonia is required to fix the aldehyde.

As regards the second batch, viz., the essential oils, if present in rather larger quantity in an alcoholic fluid which one should desire to test for wood spirit, they may be reduced to small traces by the application of the following expedient manipulation. Mix the alcoholic fluid in question with as much magnesia alba, the ordinary magnesia of the chemists' shops, as will yield a thick magma, next add twice the bulk of the alcohol of a thoroughly saturated aqueous solution of common salt, and then bring this whole mixture on a filter previously filled with magnesia; the perfectly clear filtrate is next submitted to distillation, the first small portion of the distillate, which yet will show turbidity on becoming mixed with water, is set aside, and the remainder of the distillate can then be applied to be tested for wood spirit. I ought to observe here, that if an alcoholic fluid which contains wood spirit is submitted to fractional distillation, all portions of the distillate will yield pretty fairly an equal amount of wood spirit.

The experience I have acquired by the opportunity offered to me, especially of testing on a large and ample scale divers alcoholic fluids, enables me to state that chemists will find the test described by me quite reliable to speak of, or discern with certainty the presence or absence of wood spirit in an alcoholic liquid submitted to examination. But I must expressly say that in the strictest sense the reagent only applies to aceton, and it is, therefore, only then permitted to draw the conclusion that wood spirit is really present when also the smell and other concomitant phenomena justify this conclusion. The administration of the excise in the kingdom of the Netherlands, *vulgo* Holland, does not allow wood spirit to be used for making methylated spirit, *i.e.*, mixing with alcohol, unless the wood spirit has been previously submitted to the following test: 1 part by volume of wood spirit mixed with 99 parts by volume of pure and absolute, *i.e.*, anhydrous alcohol, must be very plainly and readily recognised by the reagents above described. If there might be a doubt, or also in cases where it might be of great importance, I think the experiment by oxidation ought not to be omitted; for this reason I will briefly allude to it yet. I proceed in the following manner: 97·5 grammes of bichromate of potassa are mixed with 146·25 grammes of sulphuric acid previously diluted with 775·35 grammes of water; 35 c. c. of this fluid is mixed with 4 c. c. of the alcoholic fluid to be submitted to experiment, and placed in a small retort; the mixture while in the retort is left to itself for 24 hours, and then about 4-5ths of the contents of the retorts (several experiments of this kind are conducted at the same time, and also with mixtures of known purity) is distilled off, care being taken to keep all under the same conditions. The distillate is mixed with magnesia and evaporated. The reason why I prefer magnesia to carbonate of soda is, that the latter acts upon the aldehyde of the distillate; in consequence whereof the residue of the evaporation is rather strongly colored, and consequently apt to reduce more of the alkaline solution of permanganate of potassa than can be accounted for by the quantity of formiate of soda which is formed. When magnesia is applied the residue of the evaporation remains colourless even when the process of heating upon a water bath is very greatly prolonged, which is always required when essential oils are to be entirely eliminated. The residue of the evaporation is next taken up with distilled water, and then mixed with excess of an alkaline solution of permanganate of potassa of precisely known strength, *i.e.*, oxidising power, and left for at least two days quietly standing; the solution of permanganate is, after that time, tested by the well known method. I found it necessary to have two days' rest, as by experiments purposely instituted with a small quantity of pure formiate of soda, I found out that even up to 48 hours after the beginning of the experiment the titre of the permanganate had only become constant.

The following are results obtained with the described mode of proceeding:—

First Series.

	Quantity of oxygen required to oxidise the formic acid formed.	
Pure alcohol	1·2	milligrammes
“	1·6	“
“ + 1 per cent wood spirit	2·6	“
“ + 3 “	5·2	“
“ + 5 “	8·3	“

Second Series.

Eau de Cologne	2·7	“
“	2·4	“
“ + 2 per cent wood spirit	6·9	“
“ another variety	6·2	“
“	6·4	“
“ + 1 per cent wood spirit	9·2	“
“ “	10·1	“

Third Series.

Pure alcohol	0·7	milligrammes
“	1·1	“
Methylated spirit	17·0	“
“	12·0	“
Eau de Cologne	4·4	“
“	4·0	“
“ made with methylated spirit	26·0	“

The above-mentioned results of experiments by oxidation may be left to tell their own tale, but it will be seen that as regards the disturbing influence of essential oils especially, the checking of the experiments by simultaneously making the experiment with alcohol of known purity, is desirable.

ON GUN-COTTON TRANSPORT.

The accidents which occurred at Newcastle and elsewhere, in consequence of the disregard of precautions in the transport and handling of nitroglycerine, have created a feeling of distrust in the minds of the traffic managers of railway companies in connection with all explosive substances other than gunpowder. According to the *Pall Mall Gazette*, this distrust has now increased to such a degree that permission is frequently withheld for the transmission by railway even of the compressed gun-cotton charges used for blasting purposes, although the regulations which apply to the transport of gunpowder more than suffice to guard against the possibility of serious accident with gun-cotton.

With the object of investigating the risks incurred in conveyance of compressed gun-cotton charges by railway, Mr. Wilson, of the goods manager's office, North Eastern Railway, in conjunction with Mr. Prentice, the managing director of the Gun-cotton Company, has tried a series of experiments, of which the following is an abstract:—

A small box of cotton containing 125 charges, said to be equal in effects as a blasting agent to a quarter cask of gunpowder, was taken into the cricket field. A fuse was inserted and lighted. When the flame reached the gun-cotton there was a great blaze like the burning of a heap of loose straw, but no explosion; in less than half a minute there was no flame except from the burning of the brown paper in which the gun-cotton had been packed inside the box. The box was of wood about a half-inch thick, and was nailed, but not bound with iron at the corners; it was one of the ordinary packages used for sending the cotton out.

Several charges were then laid on the rails near the coal depôts, and coal waggons were run over them: some of them were ignited, others were not. Some of them were placed so that an engine should pass over them; they were all ignited. Mr. Prentice took an axe and chopped one charge into several pieces; there was no explosion or ignition. Small pieces of gun-cotton placed on the iron rim of a wheel and sharply struck with a hammer exploded, or rather detonated.

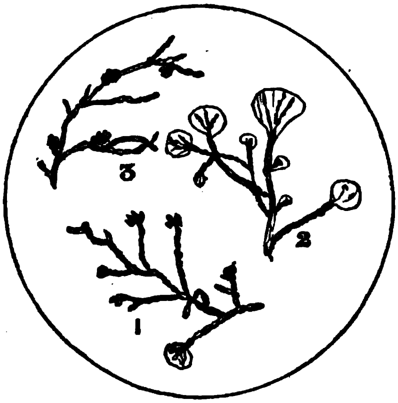
In all the cases where ignition was produced by concussion, whether of a hammer on iron, or of the wheels of an engine or waggon on the rails, it was very evident that only so much as was actually struck exploded or detonated, the part not struck firing from the explosion, and burning like so much straw or flax.

To make sure that they were dealing with the article which produces such an effect when exploded in close confinement, a hole was bored into a large block of hard tough wood, in which Mr. Prentice placed a charge of gun-cotton with a fuse attached to it; he then filled up the hole with broken slate tightly rammed, and fired the fuse. When the gun-cotton exploded the block of wood was shivered to pieces, each piece being blown several yards away.

Mr. Wilson says that the results of these experiments convince him that they may safely carry gun-cotton along with other goods in ordinary waggons, adopting the same rules as now apply to the conveyance of cartridges.

ORGANIC APPEARANCES IN COLLOID SILICA OBTAINED BY DIALYSIS.

In the report of the meeting of the Chemical Society held on April 2nd, given in our number for April 10th (*Am. Reprint*, June, '68, page 274), we gave a brief account of some observations on "Organic appearances in Colloid Silica obtained by Dialysis." The author of the paper, Mr. W. Chandler Roberts, has kindly forwarded the accompanying drawing of these remarkable appearances, together with some further details.



The dendritic forms in the air-dried gelatinous silica, vary in size from 0·2 to 0·5 millimetre.

When magnified 90 times they appear as radiating fibres; and when magnified 700 times each fibre resolves itself into one or other of the following forms, as shown in the figure:

1. The most common form.
2. The end of each fibre surrounded by an apparently vacuous space indicating its growth in the partially solidified jelly by abstracting water from the mass.
3. Apparent fructification of the organic forms.

TABLE

FOR ASCERTAINING THE WEIGHT OF A CUBIC FOOT OF ANY MINERAL ORE, METAL, EARTH, OR ANY OTHER SUBSTANCE, EITHER NATIVE OR ARTIFICIAL, FROM ITS SPECIFIC GRAVITY.*

BY DR. LEWIS FEUCHTWANGER.

1728 inches comprise one cubic foot, and one cubic foot of water weighs at a temperature of 60° Fahrenheit, 62½ pounds avoirdupois. By ascertaining the specific gravity, and multiplying with 62½ pounds, the exact weight of one cubic foot is obtained.

	Sp. Gr.	Pounds Avoirdupois. Cubic foot weighs.
Anthracite coal	1·5	94
Antimonial copper, tetrahedrite, or grey copper	5·0	300
Antimonial silver	9·5	600
Antimony ore, grey sulphuret	4·5	279
Antimony metal	6·5	400
Apatite, or phosphate of lime	3·0	186
Arsenical iron pyrites, mispickel	6·0	370
Asbestos	3·0	186
Asphaltum, mineral pitch	1·0	62
Baryta sulphate	4·5	310
Baryta carbonate, Witherhite	4·0	248
Bismuth	9·7	600
Bituminous coal	1·5	90
Black lead, graphite	2·0	125
Black jack blende, sulphuret of zinc	4·0	250
Bog iron ore	4·0	250
Brown hæmatite	4·0	250
Building stones, comprising granite, gneiss, syenite, &c.	3·0	186
Calamine	3·3	190
Chromic iron	4·5	260
Copper pyrites	4·0	260
Derbyshire spar, fluor spar	3·0	186
Feldspar	3 0	190
Flint	2·5	110
Loose sand	—	95
Franklinite	5·0	310
Galena	7·5	465
Gold (20 carats)	15·7	1000
" (pure)	19·2	to 1200
Gypsum	2·3	130
Iron—cast iron	—	450
" magnetic ore	5·0	310
" spathic ore	3·0	200
" pyrites	5·0	310
" pyrrhotine, or magnetic pyrites	4·5	280
" specular iron ore	4·5	290
" wrought	—	487
Limestone, hydraulic	2·7	150
" magnesian	2·5	130
Manganese, binoxide of	4·8	294
Malachite	4·0	248
Mica	2·8	160
Novaculite, or whetstone	3·0	186
Ochre	3·5	217
Platinum, metal and ores	16 to 19	1116
Porcelain clay	2·0	140
Pyrites, iron	4·5	280
Quartz, pure, compact	2 6	155
" loose, angular, and round sand	—	100
Trap	3·0	186
Vitreous copper, copper glance	5·5	341
Wood tin, stream tin	7·0	434
Zinc, sulphide or blende	4·0	248
Zincite, red zinc ore	5·5	331
Zinc carbonate	4·4	268
Zinc silicate	3·4	200

ON THE ESTIMATION OF MANGANESE AS PYROPHOSPHATE.

BY WOLCOTT GIBBS, M.D.

RUMFORD PROFESSOR IN HARVARD UNIVERSITY.

The existence of an orthophosphate of manganese and ammonium, corresponding to the well-known salt of

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* Condensed from the American Journal of Mining.

magnesium, was long since ascertained by Otto.[*] The subject has more recently been studied by Debray,[†] who has described a series of analogous phosphates, all of which are remarkable for their insolubility. Otto's salt, $P_2\Theta_8\cancel{Mn}_2(NH_4)_2 + 2H_2\Theta$, from its highly crystalline structure, the facility with which it is formed, and its insolubility, appeared well adapted to the quantitative estimation of manganese, and the following analyses show that this metal, like magnesium, may be advantageously precipitated as ammonio-phosphate and weighed as pyrophosphate.

To the solution of manganese, which may contain salts of ammonium or of the alkaline metals, disodic orthophosphate is to be added in large excess above the quantity required to precipitate the manganese as orthophosphate. The white precipitate is then to be redissolved in excess of sulphuric or chlorhydric acid, heated to the boiling point, and ammonia added in excess. A white or semi-gelatinous precipitate is produced, which on boiling or standing for some time, even in the cold, gradually becomes crystalline, and finally is completely converted into beautiful talcose scales which have a pearly lustre and a pale rose colour. It is best to precipitate each time in a platinum vessel, in which the ammonio-phosphate may be boiled for ten or fifteen minutes, and to allow the salt to remain at a temperature near the boiling-point of the liquid for an hour after it has become crystalline. The ammonio-phosphate may then be filtered off and washed with hot water. The washing takes place with extraordinary facility on account of the crystalline character of the salt. The orthophosphate, after drying and ignition, yields pyrophosphate of manganese as a nearly white powder. In this manner—

	Grm.			Grm.				
1.	0·9555	$\cancel{Mn}S\Theta_4$	gave	0·8985	$P_2\Theta_7\cancel{Mn}_2$	=46·68	p. c.	$\cancel{Mn}\Theta$
2.	1·1400	"	"	1·0717	"	=46·67	"	"
3.	0·8145	"	"	0·7646	"	=46·63	"	"
4.	0·9464	"	"	0·8886	"	=46·66	"	"
5.	1·3181	"	"	1·2390	"	=46·68	"	"
6.	1·0565	"	"	0·9950	"	=46·76	"	"

The formula requires 46·67 per cent. ($\cancel{Mn}=54$). The sulphate employed was pure and perfectly anhydrous. In two analyses of crystallised chloride of manganese, not quite free from mechanically mixed water, Mr. F. W. Clarke obtained 27·08 and 27·07 per cent. of manganese. In the same salt the percentage of chlorine was found to be 35·68, which corresponds to 27·14 per cent. of manganese.

The advantage of this method over that commonly employed for the estimation of manganese, is that the process permits us to weigh the metal in the form of a perfectly definite compound, and not as an oxide which cannot be safely assumed to consist wholly of $\cancel{Mn}_3\Theta_4$. When manganese is associated with the alkaline earths, it is of course first to be separated as sulphide, or by Schiel's method, as a hydrate of the sesquioxide. The ammonio-phosphate is almost absolutely insoluble in boiling water, in ammonia, and in solutions of salts of ammonium. The salt is nearly white, but sometimes becomes a little more red upon the filter. If it assumes a rather deep dull red colour, the whole of the phosphate of manganese has not been converted into ammonio-phosphate. The precipitate is then to be redissolved into dilute chlorhydric acid, more phosphate of sodium added, and then ammonia in excess, after which the boiling is to be repeated. This repetition is very rarely necessary, a little practice enabling the analyst to judge when the conversion from the flocky-gelatinous to the crystalline condition is complete. The filtrate from the crystalline salt is perfectly free from manganese. Phosphoric acid cannot be determined by precipitation, as ammonio-phosphate of manganese, because the crystalline character of the salt upon which the success of the process depends is only produced by digestion with an excess of phosphate. Bette[*] has described an ammonio-phosphate of zinc which, like the corresponding manganese salt, is almost absolutely insoluble in water. Debray[†] has analysed similar salts of nickel and cobalt; and Otto[‡] has also described the analogous ammonio-phosphate of iron. I have myself prepared an ammonio-phosphate of cadmium which, like the other salts of this group, is extremely insoluble in water. All of these salts, however, are more or less readily soluble in ammonia and in salts of ammonium, and after repeated trials I have not succeeded in rendering any of them available for analytical purposes.—*Am. Journ. Science.*

FOREIGN SCIENCE.

PARIS, MARCH 31, 1868.

Method of estimating the proximate constituents of meteoric iron.

REFERENCE has already been made to the researches of M. Meunier, on meteoric iron, in one of my former letters; the same investigator has, more recently, proposed a general method for the proximate analysis of meteoric irons. According to his experiments, iron of meteoric origin is composed of mixtures of iron and nickel, carbide of iron, sulphide of iron, phosphide of iron, and graphite. Certain species of minerals are never found intermingled, wherefore it is often unnecessary to consider the separation of some of the usual constituents. Frequently the carbide of iron disappears, or does not exist in estimable amount; such, for example, is the case in the meteoric iron discovered in 1784 at Xiquipilco, in the Valley of Toluca, Mexico. M. Meunier examined the various components given above separately. Nickeliferous iron obtained from various specimens of meteoric iron, differed considerably in composition, but the properties were sufficiently analogous to allow of its being considered as a single substance in the processes of separation. The substance is soluble in most acids, yielding a nickel salt and an iron salt; sometimes, although rarely, the solution is accompanied by the deposit of a little carbon: cold fuming nitric acid is without solvent action on it. Solutions of potash and soda, even at ebullition, are without action. Fused caustic alkalies do not dissolve, to any sensible extent, the nickel-iron mixtures. Sometimes nickeliferous iron does not precipitate salts of copper, such as nitrate and sulphate, in the cold, but upon heating to ebullition, the precipitation is always effected. Chlorine attacks nickeliferous iron pretty rapidly, especially in the presence of water; bromine and iodine exert a similar but less powerful action. Carbide of iron partakes, for the most part, of the properties of nickeliferous iron. The sulphide of iron, termed *troilite*, dissolves in hydrochloric acid with disengagement of sulphuretted hydrogen; fuming nitric acid has no action upon it; concentrated solution of sulphate of copper is not decomposed, even upon boiling, by troilite; alkaline solutions are almost without action in the cold, but they exert action, though sluggishly, upon boiling; the fused alkalies dissolve it instantaneously. The

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[*] Bull. de la Société Chimique. Nouvelle Série ii., p. 11.

[†] Ann. der Chemie und Pharmacie, viii., 173.

[*] Ann. der Chemie und Pharmacie, xv., 129.

[†] Loc. cit.

[‡] Ann der Chemie und Pharm., xvi., 199.

[English Edition, Vol. XVII., No. 438, pages 195, 196; No. 435, pages 163, 164.]

name of phosphide of iron M. Meunier gives provisionally to the compound better known as *schreibersite*, and which exists in most metallic meteorites. The composition of the mineral has not yet been defined; at the same time, the existence of phosphorus, iron, and nickel, is recognised. Magnesium possibly enters into the composition of schreibersite also. The mineral is not acted upon by boiling hydrochloric acid; alkaline solutions only attack it when aided by heat; fused potash and soda dissolve it instantly; *graphite* resists the majority of reagents.

By means of the various reactions indicated above, M. Meunier was enabled to separate the iron of Toluca into its four proximate constituents. The analytical process is conducted as follows:—(1.) Estimation of the nickeliferous iron—1 gramme of iron-filings, obtained by means of a very hard file, is projected into some grammes of pure potash in tranquil fusion, contained in a silver crucible. Care must be taken to throw the metal quite into the middle of the fused mass, and not on to the sides of the crucible, where it would undergo oxidation. Schreibersite and troilite are decomposed, and nickeliferous iron and graphite alone remain in the solid state. After cooling the fused mass is placed in strong alcohol, and allowed to remain there for about forty-eight hours. At the end of this time the whole of the potash is dissolved, and the lixivium, at the bottom of which the mixture of graphite and iron is found, is filled with brown flocks of oxide of iron. Decantation easily removes the light oxide; the residue is well washed with alcohol, and then dried. This residue is then treated with slightly warm fuming nitric acid: all the graphite disappears. It is only necessary to wash the nickeliferous iron, and dry. M. Meunier obtained about 96 per cent. of nickeliferous iron.

(2.) Estimation of the graphite.—3 grammes of iron-filings are projected, as before, into fused potash, and a mixture of graphite and nickeliferous iron obtained. This mixture is treated with hydrochloric acid, which dissolves the iron and leaves the graphite. The graphite might contain a little carbon resulting from the decomposition of carbide of iron, but carbon due to this source can always be estimated. Another method of separating the iron and graphite would be lixiviation. It is, however, indispensable that the physical separation be controlled, to some extent, by a chemical process. The Toluca iron gave 1·176 per cent. graphite.

(3.) Estimation of the troilite.—To estimate this, 3 grammes of filings are boiled for about a quarter of an hour with a solution of oxide of copper. All the nickeliferous iron is dissolved, and by decanting and washing, a residue is obtained, composed of troilite, schreibersite, graphite, and metallic copper. This mixture is treated with fuming nitric acid; the copper and graphite are removed, and troilite and schreibersite are thus obtained in a state of purity. No reaction has been discovered permitting the isolation of the troilite; it is therefore necessary to have recourse to lixiviation, and to separate the schreibersite and troilite by their different specific gravities. The specific gravity of troilite is never more than 4·7, while that of schreibersite is 7·01 to 7·22. Toluca iron gave by the process described 1·482 per cent. of troilite.

Estimation of the schreibersite.—The preceding operation evidently gives a first determination of the schreibersite, in the lixiviation. The number thus found can be controlled by a chemical process. Having obtained the mixture of schreibersite and troilite, treatment with hydrochloric acid will dissolve all the sulphide, leaving consequently the phosphide in a state of purity. Toluca iron gave 1·232 per cent of schreibersite. The numbers found for each of the proximate constituents added together will be found to give 100·191.

Paris, April 6, 1868.

Volumetric method of estimating carbonic acid in natural waters.—Aniline marking ink.—Glaze for crystallising pans.—Preservation of saccharine juice.

M. Barthélemy, Professor of Physics at the Lycée de Pau, has published a volumetric method of estimating carbonic acid in natural waters. His process depends upon the reaction of the protonitrate of mercury upon the alkaline and earthy carbonates; by means of the same reagent he is enabled to estimate small quantities of acid—for example, the nitric acid present in rain water after a storm. The crystals of neutral protonitrate of mercury are soluble in water, which at the same time decomposes the compound into insoluble sub-nitrate, and acid nitrate which remains in solution. The supernatant liquid, in the presence of mercury, may be kept a long time without undergoing any decomposition. The reagent is prepared by treating mercury with cold dilute nitric acid. Upon adding to a dilute solution of an alkaline or earthy bicarbonate, protonitrate of mercury, a precipitate forms, which is at first white, afterwards orange, and often greenish: this precipitate is soluble in excess of the reagent; also in sulphuric and nitric acids, in urine, and other organic matters.

In a solution of neutral carbonate the same reagent produces a brown precipitate which, when the alkaline carbonate is mixed with bicarbonate, takes a more or less deep green tint; this brown precipitate is insoluble in excess. By passing carbonic acid into the solution (it is sufficient to breathe through a tube), the reaction indicated for the bicarbonate is produced. The amount of acid nitrate which it is necessary to add for complete precipitation and resolution, is proportional (1) to the quantity of carbonate, (2) to the degree of concentration of the reagent, (3) to the quantity of carbonic acid engaged in the solution. M. Barthélemy prepares his normal solution by dissolving ·5 grm. of bicarbonate of potash (equal to ·241 of carbonic acid), previously heated in a current of dry carbonic acid, in a litre of distilled water. It is necessary before pouring out the solution of nitrate of mercury from the burette to agitate well, since the mercurial solution is very dense. When waters contain chlorine, the determination of the carbonic acid cannot be made exactly. Approximate results may, however, be obtained by acidulating 100 c.c. of the water with nitric acid, decomposing thus the carbonates, and then noticing the number of divisions required to precipitate the chlorides and produce a definite grey tint; afterwards the same given volume of water is treated with the solution of nitrate of mercury, until the yellowish orange first produced has disappeared, and the tint of the chloride alone remains. The first experiment serves as a standard of colour, and the addition of fluid from the burette is arrested, when the tints appear identical.

The process also admits of the separate determination of earthy and alkaline carbonates. By boiling 100 c.c. of water, maintaining the volume by adding distilled water, leaving to deposit, filtering, and passing a current of carbonic acid, matters are arranged for the volumetric determination of the carbonic acid combined with the alkalies; knowing already the amount combined with alkalies and alkaline earths conjointly, there is no difficulty in finding that due to carbonic acid combined with alkaline earths only. Here is another method, and one to which M. Barthélemy gives the preference:—A solution of potash (containing ·5 grm. of potash in a litre of water) is added in definite volume to 100° c.c. of the water; the simple carbonates are deposited on the sides of the vessel. At the end of a few days the solution is decanted and saturated with carbonic acid. The carbonic acid in solution is then determined, and that in the same volume of potash solution saturated with carbonic acid also; the difference between the amounts of solution poured from the burette, in the first experiment and the second, is the amount required by the alkaline carbonates in 100 c.c. of the water.

An indelible marking ink is prepared from aniline by mixing the two following solutions: *a*, cupreous solution—8·52 grm. of crystallised chloride of copper, 10·65 grm. chlorate of soda, and 5·35 grm. of chloride of ammonium are dissolved in 60 grm. of distilled water; *b*, aniline solution—20 grm. of hydrochlorate of aniline are dissolved in

30 grm. of distilled water, and 20 grm. of a solution of gum arabic (1 of gum to 2 of water) with 10 grm. of glycerine are added. By mixing in the cold four parts of the aniline solution with one part of the cupreous solution, a green liquid is obtained which can be used immediately for tracing characters upon linen; the marks, however, alter after the lapse of a few days. It is necessary to keep the solutions separate until required for use. If the fluid does not flow easily from the pen, it may be diluted without fear of diminishing the intensity of the tint, which at first green, gradually darkens and becomes black. Heat causes the change to take place instantaneously; a steam heat is sufficient, and is better for the fabric than a hot iron. Afterwards the linen is washed in warm soap and water. This ink resists acids and alkalies, and is remarkably permanent.

Some remarks upon the glaze of vessels used for crystallising in chemical works, have been published by M. Stinde. By his experience, the majority of glazes proposed for iron vessels do not fulfil their purpose well; either they become detached, or traversed by rust when the vessel remains empty for a few days. The mixture of oxide of zinc and soluble glass adheres to the iron well enough, but it does not prevent rust. Of all materials proposed, that of a mixture of oil and minium of iron (peroxide of iron mixed with alumina) is unquestionably entitled to preference.

After having thoroughly pulverised the minium, it is mixed with linseed oil, rendered pasty with manganese. This mixture is applied to the iron surfaces carefully cleaned, and deprived of rust by means of pumice-stone.

MM. Périer, Possoz, Cail & Co. have patented a process for the preservation of saccharine juices. Lime, it would appear, has been long known as a preservative substance; in applying it directly to the saccharine juices of plants, saccharate of lime is formed, and this can be preserved unaltered for a great length of time. Upon decomposing the compound, however, for the recovery of the sugar, it is found that the foreign matters existing in the juice have undergone such changes as to impede the extraction and crystallisation of the sugar. The patentees of the present process propose therefore to apply the lime no longer to the juice of the plant, but to the juice after removal of the foreign matters. They consider also that the lime ought to be employed in larger proportion than it has been heretofore. While M. Kuhlmann, in 1833, indicated as sufficient for the raw juice, ·3 to ·5 per cent. they consider 1 per cent. necessary for the purified juice, and when required to be preserved during some months, 2 per cent. the density of the juice being ·1040.

PARIS, APRIL 13, 1868.

Examination of fatty matters.—Manufacture of porous carbon.—New method of preparing magnesium.—Industrial preparation of oxygen.

AN ingenious method of testing fatty matters, founded upon the solubility of rosaniline in certain fatty acids, has been devised by M. Jacobsen; it is applicable, among other things, to the examination of cod-liver oil. A little piece of dry rosaniline placed in a sample of perfectly neutral oil, agitated and heated upon the water bath, remains undissolved, but if placed in a rancid oil, a red tint is rapidly developed. Oleic acid and the other fatty acids dissolve rosaniline in large quantity, and become opaque from the depth of the tint, because oleate of rosaniline is soluble, in all proportions, in oils and other fatty substances. This property enables the presence of fatty acids in oils to be detected. For instance, in commerce we have had for some years pretended white cod-liver oils which are only fatty fluids from very young animals, or veritable cod-liver oil, but which has been agitated with potash, allowed to repose and filtered. Since the therapeutic effects of cod-liver oil depend essentially upon the amount of free fatty acids which it contains, neither of these white oils can be valuable. Genuine cod-liver oil agitated with a little rosaniline is promptly coloured red, in the cold, and if heated upon the sand bath, the colour is very deep, while the bad specimens already referred to remain perfectly uncoloured.

When an oil which is only slightly rancid, contains but a small amount of fatty acids, the colouration often does not become sensible at first. In this case it is better to prepare a solution of rosaniline in absolute alcohol, and add a few drops of this to the oil to be examined, and heat on the water bath until all the alcohol has been evaporated. If no fatty acid exist, the rosaniline soon separates and rises to the surface, or when the oil is too thick, rests in suspension as a brown powder. Samples of ordinary oil occurring in commerce, have given the following results:—Olive oil and that of sweet almonds remained uncoloured by rosaniline; poppy oil became slightly red; linseed oil became strongly coloured, its natural colour rendering the tint brownish; palm oil gave a colouration still more intense. One more experiment, it has sufficed to mix the olive oil with 5 per cent. of oleic acid, to obtain with rosaniline a tint equal to that of raspberry juice. It is not proposed to heat to more than 100° C., otherwise errors might occur.

The fabrication of porous carbon, in various shapes, engages the attention of no inconsiderable number of persons in the present day; this kind of carbon is made most advantageously, your correspondent is informed, by the following process:—A mixture of wood charcoal and animal charcoal is ground to a coarse powder, mixed with sawdust, and dried at a steam heat; as soon as the material is dried, and while still warm, 20 per cent. of tar is added. When cold a certain amount of asphaltum is added, and the mass pressed into moulds. The proportions in which the ingredients are used vary according to circumstances. The moulded objects are placed in boxes of sheet iron, and covered all over with a mixture of sand and charcoal; afterwards they are heated on the sole of a furnace. Gases which are disengaged during the operation are burnt in the furnace. The entire operation lasts about twenty-four hours. Careful attention is required during the calcination; the properties of the carbon depend, in a great measure, upon the management of this part of the process.

M. Reichert has devised a new method of preparing magnesium. He takes 1,000 grammes of the anhydrous double chloride of magnesium and potassium, pulverises it, and mixes it with 100 grammes of finely powdered fluor spar; this mixture is fused with 100 grammes of sodium. The compound proposed for use occurs in the mineral kingdom in tolerable abundance as carnallite. White pieces of this mineral are available and require no previous treatment; coloured fragments must be dissolved in water, the impurities allowed to settle, and the lixivium evaporated.

M. Gondolo has made some improvements in M. Boussingault's process of extracting oxygen from the air by means of baryta. M. Boussingault, in 1852, found that in passing a current of air over baryta, heated to dull redness, oxygen was subtracted from the air, and binoxide of barium formed, and that upon then raising the heat to bright redness the oxygen was set at liberty so easily that the oxygen might be first absorbed and then evolved *ad infinitum*. M. Gondolo has made, in carrying out the details of the process, certain changes which admit of oxygen being prepared upon a manufacturing scale. For the porcelain tubes he substitutes iron ones, which may be made either of wrought or cast iron. Internally a coating of magnesia is applied, and externally asbestos, so as to diminish the porosity of the tube and the consumption of fuel. These tubes are arranged in a brick furnace having dampers, by means of which the temperature may be changed at will, and dull redness and bright redness easily obtained. To the baryta a mixture of lime, magnesia, and a small quantity of manganate of potash is added; this prevents fritting of the material. M. Gondolo says that he has made 122 alternate operations, and that the atmospheric oxygen and nitrogen are easily separated upon an industrial scale; the apparatus has been at work during six months, and fulfilled its purpose thoroughly. The process is patented.

[English Edition, Vol. XVII, No. 436, pages 178, 179; No. 437, pages 190, 191.]

PARIS, APRIL 21, 1868.

Ferments present in commercial bicarbonate of soda.—Action of saline solutions on minerals.—Detection of arsenic in cases of poisoning.

M. LE RICQUE DE MONCHY has published a note on organised ferments which occur in commercial bicarbonate of soda. He has observed in all unfiltered, concentrated solution of this substance, that he has yet examined with the microscope, very small moving corpuscles, commonly designated molecular granulations; these vegetable cells or their germs can only come from the atmosphere, where they were in suspension, since it is not conceivable that organised matter should withstand the high temperature to which soda in manufacture is submitted. The corpuscles only appear after the manufacture, and their presence explains the production of vegetable matter in media, where one is surprised to meet with it; they are ferments, the action of which varies with the surrounding matter; in certain cases they are producers of alcohol.

M. Terreil has studied for a considerable time the action of different saline solutions on minerals with a view of discovering methods of proximate analysis. At a recent meeting of the Academy, he made known the results already obtained in this direction; the note had reference chiefly to the action of ammoniacal salts upon the natural carbonates. The carbonates of baryta, strontia, and lime are easily decomposed by solutions of ammoniacal salts, with the exception of carbonate of ammonia, which leaves them in the state of carbonates. When the acid of the salt, with the base of the carbonate, gives rise to a soluble compound, the decomposition is more rapid. Carbonate of baryta is more easily attacked than carbonate of strontia, and the latter more easily than carbonate of lime. Baryta and strontia are separated in treating the two carbonates with a mixture of chlorhydrate and chromate of ammonia; the strontia is dissolved, and the baryta remains insoluble in the form of chromate. The separation of lime from baryta and strontia is effected with sulphate of ammonia, which transforms the three carbonates into sulphates; the sulphate of lime, which is more soluble in the solution of ammoniacal salt than in water, is dissolved, while the sulphates of baryta and strontia remain insoluble. Carbonate of magnesia is rapidly attacked by ammoniacal salts, as well as by carbonate of ammonia, which dissolves it, although slowly: this property enables the separation of magnesia from the preceding bases to be effected, by treating the mixture of these carbonates by chlorhydrate and carbonate of ammonia, renewing the latter salt as fast as it is volatilised. The carbonate of manganese comports itself with ammoniacal salts like the carbonate of magnesia, rendering the separation by more solvents a difficult matter, but by adding a few drops of sulphydrate of ammonia to the boiling solution of the carbonates in chlorhydrate of ammonia, the sulphide of manganese is precipitated almost completely. M. Terreil draws attention to the fact that when sulphydrate of ammonia is added to a solution containing besides manganese, a considerable quantity of ammoniacal salts, the sulphide of manganese is only precipitated after prolonged ebullition; his experiments have shown that of all ammoniacal salts, the oxalate is the one which most impedes the precipitation of the sulphide of manganese.

The natural carbonate of iron, under the influence of ammoniacal salts, is converted into a salt of iron; the decomposition is slower than in the case of the carbonates already referred to. Under these circumstances, the iron salt produced is in the lowest state of oxidation; for example, spathic iron ore in fine powder, boiled in a solution of chlorhydrate of ammonia, yields a colourless solution, which gives, with ferrocyanide of potassium, a white precipitate. Carbonate of zinc is soluble in all ammoniacal salts, excepting the sulphydrate, which does not dissolve this carbonate even in the presence of free ammonia or carbonate of ammonia; this character enables zinc to be detected and separated from the earths. The separation of zinc from magnesia and manganese can only be effected when phosphate of ammonia and free ammonia are present. Carbonate of lead is easily decomposed by ammoniacal salts; chlorhydrate of ammonia transforms it into chloride, which crystallises out upon cooling. Lead can be separated in this way from the earths, and from magnesia by sulphydrate of ammonia; it is separated from manganese, iron, zinc, and copper by sulphate of ammonia. The green carbonate of copper, *malachite*, and the blue carbonate, *azurite*, are dissolved by solutions of ammoniacal salts, equally in the presence of free ammonia or carbonate; azurite is attacked more rapidly than malachite.

The action of ammoniacal salts on natural carbonates may be summed up as follows: All ammoniacal salts in solution decompose the natural carbonates, by reason of the volatility of the carbonate of ammonia, which is produced by double decomposition; the acid of the ammoniacal salt unites itself to the base of the carbonate, even when this acid forms with the base an insoluble compound. From the foregoing, one sees that by treating the natural carbonates in fine powder with warm solutions of ammoniacal salts, chosen and mixed so that the acids can form, with the bases of the carbonates, soluble and insoluble compounds, these bases may be separated, and an analysis of the natural carbonates be made. M. Terreil promises in a future communication to treat of the analysis of oxides, sulphides, arsenides, and silicates by neutral saline solutions.

M. Buchner has published some facts in connection with the detection of arsenic in cases of poisoning. M. Buchner has several times recognised the presence of sulphide of arsenic in the bodies of persons poisoned by arsenious acid. Certainly this fact has never been observed except where the corpse has been in a more or less advanced state of putrefaction; the sulphurisation would appear to be due to sulphuretted hydrogen, a constant product of putrefactive decomposition. The last observation upon this point M. Buchner has made, was upon the remains of a woman who had been poisoned eleven months previously. The large intestine was in full decomposition, and there were yellow marks upon the mucous membrane, caused by a fine powder which could be removed by washing. This powder resembled the yellow deposit which is produced in arsenical solutions by sulphuretted hydrogen; further, it gave the characteristic reactions of sulphide of arsenic. Examining now whether the arsenic had been administered as sulphide, he concluded in the negative, for the following reasons: The contents of the stomach and small intestine being boiled with hydrochloric, and the vapours from the distillation of the acid collected in water, in a few minutes a quantity of chloride of arsenic was obtained; such would not have been the case with sulphide of arsenic, notwithstanding that this sulphide is not absolutely unacted upon by boiling concentrated hydrochloric acid. The sulphide of arsenic being insoluble in pure water and in acidulated water, it would not be carried into the circulation, also it would not be found in the liver and spleen, both of which in this particular case were saturated with arsenic. A part of the stomach and small intestine cut up and placed in the dialyser with water acidulated with hydrochloric acid, gave at the end of twenty-four hours a solution containing arsenious acid in sensible proportion, a fact proving that all the arsenic had not passed into the state of sulphide.

REPORTS OF SOCIETIES.

CHEMICAL SOCIETY.

Anniversary Meeting, Monday, March 30th.

DR. WARREN DE LA RUE, F.R.S., &c., *President, in the Chair.*

AT this meeting there was a good attendance of members, and the officers of the Society were nearly all present.

The business of the evening commenced with the reading of the President's report, which gave a very satisfactory account of the past year's proceedings, but the obituary notices were, unfortunately, more numerous than usual. The list of members, in comparison with that of last year, stands thus:—

	1867.		1868.
Number of Fellows	499	....	510
Foreign Members	40	....	39
Associates	0	....	2

The number of papers read during the session amounted to forty-eight; and four lectures were delivered.

Five members have voluntarily retired during the year, viz.:—Dr. F. V. Paxton, and Messrs. Anselim Odling, C. N. Ellis, Edward Rea, and W. V. Russell. Eight names were struck off the list of members, by reason of arrears of subscription.

The losses by death included several distinguished Fellows, and one of the founders of the Society. They were Professor Michael Faraday, Dr. C. G. B. Daubeny, Dr. Thomas Clark, Dr. Wm. Herapath, Mr. Robert Warington, F.R.S., Messrs. J. Tennant, Walter Crum, W. H. Gossage, Alfred Noble, and Wm. Winsor, besides an eminent foreign member, Professor Jules Pelouze.

The **President** indicated some of the leading researches published during the year in the several departments of the science, and referred to the progress made towards establishing the new Chemical Theory. The investigations of Graham, Hofmann, Kolbe, Abel, Fittig, Frankland and Duppa, Perkin, and Pettenkoffer and Voigt, were specially mentioned. The discussions upon water analysis had elicited facts which would ultimately prove useful in establishing a new method; and the review of geological phenomena, from a chemist's wide sphere of observation, could not fail to be productive of great results.

The **Treasurer** presented the balance-sheet for the year, which had been audited by Mr. Stephen Darby. The amount received from subscriptions was £540, and some of the disbursements were the following:—

Printing the Journal	£223
Proceedings of the Royal Society	50
Books and Magazines	43

The assets at the present time are a balance at the bankers of £637 1*s.* 11*d.*, and £2347 18*s.* 10*d.* invested in Government consols. The outstanding subscriptions due to the Society are stated at £186 16*s.* 0*d.*

The election of officers was then proceeded with, Dr. Hugo Müller and Dr. T. Stevenson being appointed scrutators. The result was declared to be in accordance with the printed list, or that proposed by the retiring Council. The President was re-elected, and the names of the remaining officers are appended:—

Vice-Presidents, who have filled the office of President: Sir B. C. Brodie, F.R.S.; Thomas Graham, F.R.S.; A. W. Hofmann, LL.D., F.R.S.; W. A. Miller, M.D., F.R.S.; Lyon Playfair, Ph D., C.B., F.R.S.; A. W. Williamson, Ph. D., F.R.S.; Colonel Philip Yorke, F.R.S. *Vice-Presidents:* E. Frankland, Ph.D., F.R.S.; J. H. Gilbert, Ph.D., F.R.S.; J. H. Gladstone, Ph.D., F.R.S.; John Stenhouse, LL.D., F.R.S. *Secretaries:* William Odling, M.B., F.R.S.; A. Vernon Harcourt, M.A. *Foreign Secretary:* F. A. Abel, F.R.S. *Treasurer:* Theophilus Redwood, Ph.D. *Other Members of Council:* E. Atkinson, Ph.D.; F. Crace Calvert, F.R.S.; J. Lothian Bell; Dugald Campbell; W. Crookes, F.R.S.; David Forbes, F.R.S.; G. C. Foster; A. Matthiessen, Ph. D., F.R.S.; E. J. Mills, D.Sc.; H. M. Noad, Ph.D., F.R.S.; W. H. Perkin, F.R.S.; J. Williams.

Mr. E. T. **Chapman** moved a vote of thanks to the President for his services during the past year, which was seconded by Mr. **Tennant**, who took occasion to advise the printing in a separate form, and issue of duplicate copies to each member, of the annual report and address just now delivered by Dr. De la Rue. Such a course had been adopted with advantage in other learned Societies, and tended both to diffuse information respecting the aims of their body, and to do honour to the memory of the great chemists departed.

The vote of thanks was carried by acclamation.

Mr. Tennant's suggestion was afterwards made a substantive proposition, and warmly supported by Mr. Brayley; it was then put to the meeting, and carried unanimously.

Dr. **De la Rue** returned thanks, and in allusion to his wandering for a time from the paths of chemistry into the green fields of astronomy, humorously illustrated the feeling of amazement which overcame him on returning to a chemical career, by comparing his experience to the dream of Rip Van Winkle.

A vote of thanks to the retiring members of Council and a special acknowledgment of Mr. Watts' services, were moved and carried with acclamation. The meeting was then adjourned until the 2nd of April, the papers to be then read having already been announced.

Thursday, April 2, 1868.

Dr. Warren de la Rue, F.R.S., &c., *President, in the Chair.*

The minutes of the last ordinary meeting were read and confirmed, and the library donations were acknowledged. The following gentlemen were balloted for and duly elected Fellows of the Society, viz.: John Tyndall, LL.D., F.R.S., Lecturer on Natural Philosophy in the Royal Institution of Great Britain; Frederic Guthrie, Ph.D., F.R.S.E., Lecturer on Chemistry in the Royal College of Mauritius; William Brantingham Giles, Old Swan, Liverpool. For the first time was read the name of Mr. Thomas Bournes, Teacher of Chemistry, 47, Rigby street, St. Helen's, Lancashire; and for the second time, Francis C. H. Clarke, Lieutenant Royal Artillery, Staff College, Farnborough Station.

Mr. W. H. **Perkin** read a paper "*On the Constitution of Glyoxylic Acid,*" of which Mr. Duppa and himself were joint authors. The starting-point in the formation of this body was dibromacetic acid, and this converted into the silver salt and heated under water, furnished a product described in the original research (1859) as "a new acid having the formula $C_2H_4O_4$."* This body has since been regarded as glyoxylic acid. The authors now resume the description of this acid, and quote analysis in proof of the correctness of its formula. They have further ascertained by comparative examination of the calcium and silver salts that this acid is in all respects identical with the glyoxylic acid obtained by Dr. Debus as a product of the oxidation of alcohol. The method followed in the purification of the dibromacetic acid is fully described in the paper, and consists in the etherification of the crude acid, conversion into amide, and repeated crystallization of the latter, when all the mono-bromacetamide is left in solution. The purified amide is then decomposed by hydrate of potassium, added by small portions at a time, and in a vessel surrounded by ice-water. The ammonia liberated is neutralised by dilute nitric acid, and the solution mixed with nitrate of silver, when the dibromacetate of that metal is precipitated. This is said to be not affected by light, although Dr. Debus asserts the contrary. The silver salt is diffused through a considerable quantity of water, and exposed to the temperature of 100° C. until no more yellow bromide of silver is formed, which filtered off leaves bromoglycolic acid in solution. This, in turn, is again converted into its silver salt, and decomposed in a similar manner, yielding bromide of the metal and pure glyoxylic acid in solution. The authors have discovered a very characteristic test for this acid dependent upon the ease and rapidity with which the aniline salt is decomposed. This combination, at first colourless, lets fall a bright orange-coloured precipitate, either on standing at rest for some time, or immediately upon heating. It is impossible within the limited space at our disposal to do justice to the

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* Journ. Chem. Soc., vol. xii. p. 6.

authors' arguments in support of their views respecting the constitution of glyoxylic acid, which occupy more than half the length of their paper.

Dr. ODLING read a paper "*On a Glyoxalic Amide.*" In the course of an examination into the properties of the remarkable compound $NOCl_2$, obtained by Gay-Lussac as the chief product of the reaction of nitric and hydrochloric acids, he had made preliminary experiments on the action of this substance upon a variety of compounds, with the view of introducing the group NO in exchange for hydrogen; or of otherwise obtaining some evidence from the reactions of the substance in favour or disfavour of Gay-Lussac's formula, which he did not consider as being at present satisfactorily established. From the result of a preliminary experiment, he had been induced to study the reaction of Gay-Lussac's body with alcohol more in detail. His results were at present in an incomplete state, and he would not have ventured to bring them before the Society just yet, but for the bearing they had upon the experiments of Messrs. Perkin and Duppa, with whom he had been in communication.

Absolute alcohol absorbed Gay-Lussac's body abundantly in the cold, apparently without decomposition; but after a time the temperature rose rapidly, and an unmanageably violent reaction set in. At the temperature of 40° or 50°, however, there was no mere absorption of the chloro-nitric vapour, but it acted continuously upon the alcohol, with copious evolution of hydrochloric acid. The product of the reaction was heated on a water-bath, whereby the excess of alcohol containing apparently some chloral, was distilled off, and a syrupy liquid was left, which was further heated for some time on a water-bath, while being treated with a current of dry carbonic acid gas.

On moderate dilution this syrup yielded a watery solution and an oily deposit. The latter was first examined, but he would now refer, first, to the solution. Being extremely acid, it was neutralised with chalk, and the solution of the resulting lime salts evaporated, whereby what appeared to be a magnificent crop of crystals was obtained, but the apparent crystals were in reality masses of crystalliform jelly. The jelly was dissolved in water, the solution precipitated by alcohol, the precipitate redissolved in water, and reprecipitated by alcohol once or twice until obtained free from chlorine. From the aqueous solution of the final precipitate, crystals were obtained, which an ultimate analysis and examination of their properties showed to be glyoxalate of calcium. In particular, they gave the interesting aniline reaction which Messrs. Perkin and Duppa had just described. Dr. Odling was of opinion that the treatment of alcohol with Gay-Lussac's body constituted the most productive process yet described for the preparation of glyoxalic acid. He had not ascertained whether the jelly he had referred to was or was not a definite compound of glyoxalate and chloride of calcium. The results of his analysis of the pure glyoxalate corresponded with those of Dr. Debus and of Messrs. Perkin and Duppa, and accordingly the salt might be represented by either of the formulæ,

$$\frac{Ca}{2}HCO_3.H_2O \text{ or } \frac{Ca}{2}H_3CO_4.$$

The oil having been washed with dilute potash and water, was dried over chloride of calcium, and distilled. A portion boiled between 100° and 160°, without giving an indication of any fixed point; by far the larger portion boiled between 180° and 200°; and another portion boiled between 240° and 250°. The portion boiling between 180° and 200° had alone been submitted to examination. Its rectification was very troublesome, owing to the principal constituent being mixed with one or more substances decomposible by distillation. At length a liquid was obtained, boiling at 189°, which was thought to be pure. Analysis, however, proved that it was very far from pure. Its behaviour with potash having shown it to be an ether, it was accordingly treated with ammonia, in the first instance with alcoholic ammonia in sealed tubes, afterwards with strong aqueous ammonia and sufficient alcohol to cause the two liquids to dissolve in or mix with each other. On spontaneous evaporation, a beautiful crystalline substance was obtained, having much the appearance of chlorate of potassium or nitrate of silver, the perfect crystals occurring as rectangular plates. It melted at about 77°, and was capable of being boiled and distilled; but on attempting to take its vapour density it underwent decomposition. It was very soluble in water and alcohol, forming neutral solutions, which did not evolve ammonia when treated with potash in the cold. The numbers obtained by its analysis were in accordance with the formula $C_6H_{13}NO_3$. From a consideration of its properties and mode of formation, it might be regarded as an amide of Messrs. Perkin and Duppa's acid, in which the two alcoholic hydrogens were replaced by ethyl, thus:—

$H_4C_2O_4$	Glyoxalic acid.
$H_5C_2O_3N$	Its unknown amide.
$Et_2H_3C_2O_3N$	The diethyl-amide.
$Et_2H_2C_2O_4$	The unknown corresponding acid.
$Et_3H\ C_2O_4$	Its ether.

The further examination of the ether from which the amide had been prepared was not complete, but enough had been done to show that in all probability it was the compound $Et_3HC_2O_4$.

It would thus be seen that Dr. Odling's experimental results were in perfect harmony with those of Messrs. Perkin and Duppa. The interpretation of both sets of results, however, he considered still an open question—and a most important one—which further experiment alone could positively solve. But with the imperfect materials at present available, he would state what he considered to be the arguments for and against each view of the constitution of glyoxalic acid. Starting from aldehyd and alcohol, he believed that all chemists entertained the same notion as to the constitution of those compounds, although some chemists expressed their notion by means of reasonable, and others by means of unreasonable formulæ (Laughter). Aldehyd contained two marsh-gas residues, one of which was in its primitive state, while the other had undergone the aldehydic modification, or had lost two atoms of hydrogen in exchange for one atom of oxygen, thus:—

$$HO\underbrace{C.C}H_3.$$

Similarly, alcohol was composed of two marsh-gas residues; one of which was in its primitive state, while the other had undergone the alcoholic modification, or had lost a single atom of hydrogen in exchange for an atom of oxygen, the excessive equivalency of which was supplemented by addition of an atom of hydrogen, thus:—

$$(HO)H_2\underbrace{C.C}H_3.$$

According to Debus, glyoxalic acid was composed of two marsh-gas residues, one of which had undergone the acid modification, or had lost three atoms of hydrogen in exchange for two of oxygen, the excessive equivalency of which was counterbalanced by addition of an atom of hydrogen, while the other marsh-gas residue had undergone the above-described aldehydic modification, thus:—

$$HO\underbrace{C.C}O_2H.$$

But according to Perkin and Duppa, glyoxalic acid was composed of two marsh-gas residues, one of which had undergone the acid modification, while the other had undergone a sort of glycol modification, or had lost two atoms of hydrogen in exchange for two atoms of oxygen, the excessive equivalency of each of which was supplemented by an added atom of hydrogen, thus:—

$$(HO)_2H\underbrace{C.C}O_2H.$$

In favour of Perkin and Duppa's view might be urged, 1st, the complete accordance of their formula with the ascertained composition of glyoxalate of calcium, of diethylated glyoxalic amide and ether, and of most glyoxalates;

2d, its accordance with the ingenious transformations and re-transformations which Messrs. Perkin and Duppa had just described. Against it might be urged, 1st, its *à priori* improbability, on the ground that two atoms of hydrogen cannot be replaced by hydroxyl in marsh-gas itself, and are not known to be so replaced in any constituent marsh-gas residue, although subjected to processes similar to those by which glyoxalic acid is produced; 2nd, its want of accordance with the remarkable aldehydic characters of glyoxalic acid; 3rd, its want of accordance with the composition of crystalline glyoxalate of ammonia.

In favour of Dr. Debus's view might be urged, 1st, the necessary existence of the body represented by his formula, and its necessary possession of the joint aldehydic and acid characters exhibited by glyoxalic acid:—

$\mathrm{HO\underset{\smile}{C}{:}COH}$ .... Glyoxal.

$\mathrm{HO\underset{\smile}{C}.CO_2H}$ .... Glyoxalic acid.

$\mathrm{HO_2\underset{\smile}{C}.CO_2H}$ .... Oxalic acid.

2nd. Its complete accordance with the aldehydic characters of glyoxalic acid, as shown by its power of reducing oxide of silver, of combining with the acid sulphites, and of breaking up under the influence of alkalies into alcohol and acid, just as does aldehyd itself:

$$\mathrm{2HO\underset{\smile}{C}.CH_3 + H_2O = (HO)H_2\underset{\smile}{C}.CH_3 + HO_2\underset{\smile}{C}.CH_3}$$

$$\mathrm{2HO\underset{\smile}{C}.CO_2H + H_2O = (HO)H_2\underset{\smile}{C}.CO_2H + HO_2\underset{\smile}{C}.CO_2H.}$$

3rd. Its accordance with the composition of glyoxalate of ammonia, a salt made by decomposing glyoxalate of calcium with oxalate of ammonia. Against Dr. Debus's view might be urged, 1st, its necessitating the representation of glyoxalate of calcium as containing an atom of water not removable at 160°, a very suspicious circumstance, and the representation of the speaker's amide as containing an atom of ether; 2d, its less direct accordance, but by no means positive discordance with the metamorphoses described by Messrs. Perkin and Duppa.

It must be remembered, however, that the aldehydic marsh-gas residue in aldehyd itself, and in benzoic aldehyd, &c., has the property of uniting with chloride of ethyl and chloride of acetyl, with oxide of ethyl and oxide of acetyl, and that in aldehyd with ammonia also. Hence it is not altogether surprising that the aldehydic residue of Debus's glyoxalic acid, combined as it is with a saline instead of a hydrocarbon residue, should have the property of uniting with an atom of oxide of hydrogen. Viewed in this way, the speaker's ether would be a sort of acetal, and be formed under the same circumstances as acetal, namely, by the oxidation of alcohol.

$$\mathrm{Et_2O.HO\underset{\smile}{C}.CO_2Et.}$$

$$\mathrm{Et_2O.HO\underset{\smile}{C}.CH_3.}$$

Assuming that the ether of acetal has reacted with the aldehydic marsh-gas residue to form an unstable di-ethylated glycol residue $\mathrm{(EtO)_2HC}$, of course the question at issue between Dr. Debus and Messrs. Perkin and Duppa would become, in great measure, a verbal one.

Dr. A. W. **Williamson**, who at this period of the evening occupied the chair, referred to the anomalous constitution of the glyoxylate of ammonia, which did not appear to contain the additional atom of water; but the view advanced by Messrs. Perkin and Duppa received support from the fact that the silver salt, like the acid itself, contained four atoms of oxygen.

The **Chairman** moved a vote of thanks both to Dr. Odling and the gentlemen already named, for the interesting theoretical considerations elicited in the previous discussion.

Mr. W. **Chandler Roberts** read a note "*On the Occurrence of Organic Appearances in Colloid Silica obtained by Dialysis.*" The interesting observations which formed the subject of this paper were elucidated by a series of specimens, both of artificial and natural origin, the structures of which were demonstrated by the aid of a microscope and illustrative drawings. In experimenting upon somewhat large quantities of soluble silicic acid prepared in Graham's dialyser, a portion of the liquid product was evaporated slowly in air to compare with the forms of hydrous silica left by a more rapid operation conducted *in vacuo*. All the specimens of jelly dried in air exhibited dendritic forms, varying in size from 0·2 to 0·5 m.m.; these were at first supposed to afford indications of the passage of colloid into crystalloid silica, but when magnified 90 linear they appeared as radiating fibres, and upon being further magnified 700 times each fibre resolved itself into a collection of elongated beaded cells with clusters of circular cells at intervals. Such a structure would indicate a vegetable growth, and the author concludes that the markings, which are similar to those seen in moss agates and Mocha stones, are due to the growth of fungi or mildew in the partially solidified jelly. The spores of organic life were probably derived from the air, since no evidence of similar structure was visible in the specimens of hydrous silica obtained in the desiccator. These last-named products were very like the opal from Zimapan, but contained 21·4 per cent. of water.

A short note "*On the Solubility of Xanthin* (*uric oxide*) *in dilute Hydrochloric Acid*," by Dr. H. Bence Jones, was next read. Xanthin is usually stated to be insoluble in hydrochloric acid, but the author finds it to be soluble, and had no difficulty in obtaining "six-sided crystals" upon evaporation of the acid. By microscopic examination alone xanthin would be mistaken for uric acid.

In continuation of his recent "*Researches on New and Rare Cornish Minerals*," Professor A. H. Church describes the mineral *Cornwallite*, and gives several analyses by which it is shown to consist of arseniate and hydrate of copper with a small proportion of phosphate. Neglecting the latter its formula may be written thus:—

$$\mathrm{Cu_32AsO_4,2CuH_2O_2,\ aq.}$$

Previous analyses make this mineral appear to have in all *five* atoms of combined water, but the author believes that the error in excess is accounted for by want of care in drying the samples previously to their chemical examination.

The formula of Cornwallite, thus amended, makes it stand in nearly the same relation to Erinite amongst the arseniates, as Ehlite stands to Dihydrite amongst the phosphates. This will appear by the following comparison:—

Cornwallite........	$\mathrm{Cu_32AsO_4}$	$\mathrm{2CuH_2O_2}$ aq.
Erinite............	$\mathrm{Cu_32AsO_4}$	$\mathrm{2CuH_2O_2}$
Ehlite.............	$\mathrm{Cu_32PO_4}$	$\mathrm{2CuH_2O_2}$ aq.
Dihydrite..........	$\mathrm{Cu_32PO_4}$	$\mathrm{CuH_2O_2}$

A vote of thanks having been passed to the authors, the meeting was adjourned until Thursday, 16th instant, when the following papers will be read:—"*On Graphic Formulæ*," by Dr. Guthrie; "*On the Tetra-phosphoric Amides*," by Dr. J. H. Gladstone; "*A New Reaction for the Formation of Isomeric Cyanides*," by Messrs. E. T. Chapman and Miles H. Smith; and, if time permits, one or two other papers.

Thursday, April 16, 1868.

Dr. Warren de la Rue, F.R.S., &c., *President, in the Chair.*

The minutes of the previous meeting were read and confirmed. Amongst the donations to the library was the new catalogue of scientific works recently prepared by order of the Royal Society.

Dr. F. **Guthrie** was formally admitted as a Fellow, after having signed the statute book. No new candidates were proposed, but the name of Mr. Thomas Bournes, Teacher of

Chemistry, 47, Rigby Street, St. Helen's, Lancashire, was read for the second time. The ballot was taken on behalf of Lieutenant Francis C. H. Clarke, Royal Artillery, Staff College, Farnborough Station, who was declared to have been duly elected as a Fellow of the Society.

Professor Guthrie described and exhibited an *Improved Voltastat*, by which the current of a galvanic battery may be maintained perfectly constant and regular by a self-actug arrangement, which will become intelligible by the following description:—A vertical glass cylinder of about the size of a test tube is charged with dilute sulphuric acid, with a layer of mercury below occupying about one-third of its total contents. Partly immersed in the acid liquid is a pair of platinum electrodes insulated by glass fused upon the wires at that portion which passes through the cork stopper of the jar, and a comparatively wide glass tube open at both ends is fixed in the same cork, with its lower extremity dipping below the level of the mercury, whilst another delivery tube with bulb and capillary orifice provides for the slow escape of the mixed gases resulting from the electro-decomposition of the water. This apparatus having been placed in the battery circuit, say of three Bunsen cells, evolves the oxyhydrogen gas with a rapidity which may be easily regulated by the size of the aperture; if, then, the activity of the battery is increased, the larger volume of gas, unable to escape, exerts a greater degree of pressure upon the liquid contents of the cylinder, and the mercury is forced up the open tube, whereby the column of liquid descends and smaller surfaces of the platinum plates are left immersed, and the power of conduction is to a corresponding extent lessened. In this manner the author states that he found no difficulty in maintaining a perfectly uniform current for a period of six or seven hours, and any required adjustment could be made either by altering the size of the apparatus or of its component parts. By collecting the gases evolved this little arrangement could also be made to serve as a voltameter.

The President, in remarking upon the ingenuity displayed in the construction of the apparatus, suggested that, whilst it would be found serviceable in electro-plating and other applications where a somewhat intense current was employed, he doubted its use in the ordinary electrotype process for the deposition of copper, where weak currents only were required.

Professor Guthrie then proceeded to read his paper "*On Graphic Formulæ*," which at the outset he stated to be founded on the same general principle as that of Dr. W. Crum Brown, but would, he conceived, "serve to illustrate the molecular constitution of compound bodies from a somewhat different perspective." The author adopts a new set of pictorial symbols by which to represent the elements themselves, and arranges them in a geometrical fashion to construct the compounds formed by their union. Thus hydrogen in combination is expressed by a single dot, the gas itself by two dots; chlorine, by a pot-hook; iodine, by a small triangle; bromine, by a cross like the sign of multiplication; fluorine, by a couple of commas. Bivalent elements, thus: oxygen, a horizontal dash; sulphur, a waved line; selenium, like sulphur, but more angular. Trivalent elements: nitrogen, a large triangle; phosphorus, similar, but with lines curved inwards. Carbon is designated by a square or four-sided figure. If, then, marsh gas has to be represented, the carbon atom is shown to be saturated by placing a dot, for hydrogen, outside each face of the square. In a similar manner, with ammonia, the triangle of nitrogen has a dot standing off each face. Water is a dash with dots, for hydrogen, above and below; sulphuretted hydrogen, a waved line with two dots similarly placed; hydrobromic acid, a dot and a cross; nitrous oxide, two triangles with a horizontal dash placed between them, the whole figure being placed in a symmetrical (vertical) form; nitric oxide, a single triangle with dash below; and nitric anhydride, two triangles separated by a dash, and having all disengaged faces closed in by the oxygen dash. As yet no specific symbols are proposed for the metallic elements; but the author, later in the evening, adopted for mercury the present crossed sign for that metal. By way of conclusion, Professor Guthrie drew the figure representing triethylamine, which was shown with nitrogen (a triangle) for the nucleus, with a couple of outstanding carbon squares, appropriately dotted, opposed to each face of the triangle. The author claims for his system an increased facility in representing the satisfied and unsatisfied polarities of compound bodies.

Drs. Atkinson, Russell, and Stevenson spoke briefly, and, in a general sense, adversely, as to the desirability of introducing the system to the notice of the student. The last-named gentleman considered that the new symbols would afford little or no help in elucidating the constitution of bodies beyond the methods at present in use, and they would only be to the student something more to learn.

Dr. Odling regretted the absence of Dr. Frankland, who was so warm an advocate of the policy of introducing these pictorial methods of representation. For his own part, he looked upon them much in the light of "picture alphabets," and applicable only to those who, like the juveniles, could not be brought to book without such fascinating aid. His objection, both to this and to the system of Dr. Crum Brown, was that it required the eye of an artist to show the figures to advantage, and even then they might not be arranged properly. There were two ways, for instance, of representing the constitution of *white precipitate*, $HgCl, NH_2$. According to one view, the mercury was made the central atom around which the affinities were severally disposed; but if nitrogen was placed as the nucleus of the system, then we arrived at the anomalous result that chlorine was directly united with it, and mercury even with a double bond; whereas the known properties of the elements would rather point to hydrogen and mercury as those for which the chlorine had the strongest affinity. Dr. Odling humorously remarked that this difficulty could be met in a manner similar to that of a "diplomatic student" at one of the Cambridge examinations, who, when asked whether the sun moved round the earth, or otherwise, answered by saying that "sometimes it went one way and sometimes another."

At the request of the President, Dr. Guthrie sketched upon the board his mode of representing the constitution of *white precipitate*; but two efforts were required before an expression was arrived at which met with general approval.

Dr. J. H. Gladstone then read a paper "*On the Tetraphosphoric Amides.*" These compounds are produced by the action of water upon the amidated oxychlorides of phosphorus, and contain, as their name implies, four atoms of phosphorus united with the other elements in what at first sight appeared extremely complex relations. Their physical condition renders them somewhat difficult of purification—most of them being "sticky flocculent precipitates,"—and it is not to be wondered at that the analytical results are not so sharp and satisfactory as with bodies which can be purified by crystallisation. Amongst the substances described by Dr. Gladstone, are the terammoniated tetraphosphodiamic acid, $P_4N_5H_{17}O_{11}$, viewed thus:—

$$P_4(NH_2)_2(NH_4)_3HO_{11}$$

and a solid acid, to which the undermentioned name and formula apply,

Tetraphospho-tetramic acid, $P_4N_4H_{10}O_9$, viewed as

$$P_4(NH_2)_4H_2O_9.$$

Two silver salts of this acid were prepared, in one of which six atoms of the hydrogen were replaced by the metal.

The President, in reference to the highly complex character of the bodies described by Dr. Gladstone, ventured to suggest that some of those now considered to be individual substances might ultimately prove to be mixtures of simpler and more definite compounds.

Mr. W. H. Perkin saw no inconsistency in the formulæ proposed by Dr. Gladstone; these bodies were constituted on the type of Professor Wurtz's polyethylic alcohols in which the ethylene was replaced by phosphorus compounds.

A paper by Mr. J. Carter Bell was next read. It was entitled, "*On the Solubility and Crystallisation of Plumbic Chloride in Water, and in Water containing various proportions of Hydrochloric Acid.*" The author finds the degree of solubility in pure water to be somewhat greater than hitherto represented; the mean experimental result gave 1 part in 121 parts of water, instead of 135. By boiling with water there is evidence of decomposition resulting in the formation of free hydrochloric acid with oxide of lead, or a basic salt, left in solution. The solubility of the chloride in hydrochloric acid decreases until the amount of acid reaches 15 per cent., when the curve again ascends to a maximum with the pure acid. The author concludes with some observations upon the different forms of crystals obtained by the evaporation of aqueous and hydrochloric solutions.

A vote of thanks having been passed to the authors of the above communications, the meeting was adjourned until Thursday, May 7th, when Mr. Siemens will deliver a discourse "*On the Regenerating Furnace as applied to the Production of Steel.*"

ACADEMY OF SCIENCES.

PARIS, MARCH 9, 1868.

On the Corresponding Terms to Benzoic Acid in the Naphthalic Series. Determination of the Equivalent of Aluminium.

THE following is an abstract of Dr Hofmann's communication to the Academy on the corresponding term to benzoic acid in the naphthalic series. When a mixture of four parts of naphthaline of commerce, and five parts of crystallised oxalic acid is submitted to distillation in an iron pot, the cover of which is furnished with a tube (the kind of vessel used in the manufacture of cyanide of potassium), water and naphthaline come over at the commencement; soon also, an oily substance which is not slow to solidify makes its appearance. This oily substance is composed of naphthylformamide, naphthyloxamide, oxalate of naphthylamine, naphthylamine, and water. A current of steam removes from the oily product notable quantities of an opaque brown oil, having a greater density than that of water. The analysis of this body leads M. Hofmann to call it cyanide of naphthyl, and proves to him that the succession of transformations undergone by naphthylamine when acted upon by oxalic acid is quite analogous to that already shown in the case of aniline and toluidine. The purification of the crude mixture presents no difficulty. Solution of the oil in ether excludes the water; the etherial liquid is evaporated, and the residue distilled. It is only at 218° or 220° that the thermometer becomes stationary; the portion distilling at this temperature soon solidifies. This portion is shown by many properties to be naphthaline, mixed with a small quantity of a substance possessing a peculiar aromatic odour, and boiling at a higher temperature. The point of ebullition soon rises to 290° and 300°; the remainder of the liquid distils in the form of a clear yellow liquid, which becomes a white crystalline mass when left a considerable time in a cold place, or when immersed in a freezing mixture. Once solidified it does not become fluid again at the ordinary temperature. Crystallisation from alcohol will render the body pure. The alcoholic solution mixed with water deposits the oil again. The crystals melt at 33·5°; and the substance boils at 290°. This new crystalline compound corresponds in the naphthalic series to the benzonitrile of the benzoic series. The formula is $C_{11}H_7N$. For this body to be placed among the nitriles, it ought under the action of powerful metallic hydrates, to fix a molecule of water becoming an amide, which by absorbing another molecule of water should give rise to a salt of ammonia; experiments upon these points have confirmed its position. If the nitrile is dissolved in alcoholic soda, only traces of ammonia are disengaged, but upon adding water one reocgnises the formation of a new compound.

The crystals deposited are little soluble in alcohol, and only melted with difficulty. Purified by repeated crystallisations from boiling alcohol, the compound presents itself in the form of white needles. Analysis has given the formula $C_{11}H_9NO$. The product is thus derived from the nitrile by the assimilation of a molecule of water, and is the amide. In making this last substance, by the action of soda, ammonia is evolved. It is only necessary to add hydrochloric acid to the alkaline solution to precipitate a crystalline acid resembling in its properties. vividly, benzoic acid. This precipitate can be obtained quite as readily from the crude nitrile, by treating the latter with an alcoholic solution of soda until ammonia ceases to be evolved, evaporating the alcohol, and decomposing the alkaline liquid by hydrochloric acid. The acid is purified by crystallising from alcohol, or better from boiling-water. The pure acid crystallises in white needles, which melt at 160°. At a higher temperature the acid sublimes; its boiling point is 300°. The acid has scarcely any odour or taste; gently heated, it exhales an odour analogous to that of naphthaline: the vapours excite coughing. Solutions of the substance possess a slightly acid reaction; they decompose alkaline carbonates with facility. M. Hofmann proposes the names menapthoxylic acid and naphthaline-carboxylic —the amide and nitrile would then be menaphthoxyl-amide and menaphthenylnitrile. Several salts of the acid have been made; the composition of the silver salt is represented by the formula $C_{11}H_7AgO_2$. The copper salt is a green precipitate, the lead salt white. When the acid is distilled with caustic baryta, naphthaline and carbonic acid are obtained—$C_{11}H_8O_2 = C_{10}H_8 + CO_2$. If 4 parts of fused menaphthoxylic acid are ground with 5 parts of perchlorido of phosphorus, the two bodies act upon each other at once. The mixture is liquefied at the ordinary temperature; heated moderately, hydrochloric acid and oxychloride of phosphorus are produced. The boiling point rapidly rises to 300°; the fraction distilling between 296° and 298° is pure menaphthoxylic chloride. Its composition is $C_{11}H_7OCl$; it comports itself like most chlorides of the aromatic acids. Exposed to the air, it absorbs moisture, and is gradually transformed into menaphthoxylic acid; addition of water causes the reaction to take place instantaneously. Treated with ammonia, the chloride furnishes menaphthoxylamide, possessing all the properties of the body obtained by the action of an alcoholic solution of potash on the nitrile. When the chloride is placed in contact with an alcoholic solution of aniline, a white crystalline mass results. This compound is $C_{17}H_{13}NO$. The solution of aniline, replaced by naphthylamine, yields a compound of the formula $C_{21}H_{15}NO$. In treating the chloride with absolute alcohol, a compound of the formula $C_{13}H_{12}O_2$ is obtained.

M. Isnard addressed a note on the determination of the equivalent of aluminium. The process employed consisted in attacking the metal by hydrochloric acid. He found that 9 grammes of aluminium attacked by pure hydrochloric acid gave invariably, after calcination, 17 grm. of alumina, whence he concludes that 9 should represent the equivalent of aluminium, hydrogen being unity.

PARIS, MARCH 16, 1868.

Skeletons of Cellulose.—New Mode of forming Organic Sulph-Acids.—Transformation of Uric Acid into Glycocol.—Oxychloride of Silicium.

THE memoirs relating to chemistry, brought before the Academy of Sciences at the meeting on the 16th of March, were the following:—"On a tissue or skeleton of cellulose, directly extracted from an epidermis," by M. Payen. "On a meteorite which fell on the 9th June, 1867, in Algeria," by M. Daubrée. "On a new mode of formation of organic sulph-acids, and on the transformation of uric acid into glycocol," by M. Strecker. "On an oxychloride of silicium." "The reduction of nitrates and sulphates in certain fermentations," by M. Béchamp. "On the cultivation of beet-root for sugar," by M. Mehais. M. Maumené addressed some observations on the subject of potash extracted from suint.

M. Payen referred, in commencing, to the numerous examples of vegetable substances he had made known, in former researches, where the skeletons of cellulose at first are easily obtained, and where the cellulosic substance is possessed of the properties as well the composition of cellulose, and yet afterwards, during growth, foreign matters mask these properties. When nitrogenous matters, fatty and saline, have thoroughly penetrated the cells, the difficulty experienced in separating them is so great that some have considered the mixture of substances free from cellulose, and have in fact believed in the presence of an entirely new proximate principle. M. Payen in very cold weather submitted several tubercles of potato to refrigeration. After thawing, the epidermis was easily removed. By careful treatment with various acids and potash solution, during many days, as well as by washing with water, alcohol, and ether, the membranous substance was obtained in a supple condition and white: specimens were exhibited to the Academy. The substance thus purified gave the reaction—the blue tint with a very weak solution of iodine when acidified with sulphuric acid—due to cellulose.

M. Strecker's method of forming organic sulphacids consists in reacting upon the chlorides of the radicals with sulphites. Several compounds of the sulphacids have been obtained in M. Strecker's laboratory. Iodide of methyl heated to 150° with a solution of sulphite of soda, yielded methyl-sulphite of sodium (methyl-dithionate), according to the equation

$$\text{Ꞓ}H_3I+Na_2S\Theta_3=\text{Ꞓ}H_3SO_3Na+NaI.$$

Bromide of ethylene and sulphite of potash gave disulphethylenate of soda and bromide of potassium. A new acid, which may be called trisulphoglycerilic acid, is produced when trichlorhydrine is reacted upon by sulphite of potash

$$\Theta_3H_5Cl_3+3K_2\text{Ꞩ}\Theta_3=C_3H_5(\text{Ꞩ}\Theta_3K)3+3KCl.$$

The chlorinated acids comport themselves in an analogous manner; monochloracetic acid is transformed by ebullition with a solution of an alkaline sulphite into alkaline sulphacetate. The chlorhydrate of oxide of ethylen furnishes, under the same conditions, isethionic acid. M. Strecker states that all the chlorine, bromine, and iodine, directly united to the carbon, is usually replaced by an equivalent quantity of the radical ($\text{Ꞩ}\Theta_3K$). At the same time it often happens that only a portion is replaced, while the rest remains unattacked. In heating chloroform with a solution of sulphite of potash, the potash salt of sulphodichloromethylic acid was obtained, according to the equation, $\Theta HCl_3+K_2\text{Ꞩ}\Theta_3=\Theta HCl_2\text{Ꞩ}\Theta_3K+KCl$. M. Strecker remarked that his experiments showed that the sulphacids contained the residue $\text{Ꞩ}\Theta_3H$ united directly to the carbon by the sulphur; he thought it probable that the isomeric ethyl-sulphurous acids contained likewise this group, but united to the carbon by the interposition of oxygen.

M. Wurtz presented the foregoing, as well as another note by M. Strecker, "on the transformation of uric acid into glycol." When uric acid is heated with a concentrated solution of hydrochloric acid, or hydriodic acid, preferably the latter, in a sealed tube, to a temperature of 160-170°, it is completely transformed into glycol, carbonic acid, and ammonia. Upon opening the cold tube, a continuous current of carbonic acid is seen to be disengaged. The solution, treated with hydrated oxide of lead, evolves abundance of ammonia, and after removal of the lead by sulphuretted hydrogen yields, upon evaporation, a crystalline residue of glycol. Analysis showed the substance to be identical with that obtained from hippuric acid; the crystalline form and the chemical properties were also in perfect accordance. If, then, hippuric acid be considered as a glycol joined to benzoic acid, uric acid may, in the same way, be figured as a combination of glycol with cyanuric acid; these two acids, characteristic of the urinary secretions of herbivorous and carnivorous animals, are now seen to present more resemblance than could have been supposed.

MM. Friedel and Ladenburg have observed that in passing chloride of silicium through an empty porcelain tube, or one filled with fragments of felspar, heated to a temperature approaching the point of fusion for this mineral, and distilling, the product condensed at the extremity of the apparatus, is a liquid less volatile than the chloride. By repeating the operation a great number of times with the more volatile portions, a notable amount of a liquid boiling above 70° is obtained. This product submitted to fractional distillation is easily separated into chloride of silicium and a liquid chiefly boiling between 136° and 139°. Limpid and fuming in the air, this liquid bears great resemblance to chloride of silicium; it is likewise decomposed by water energetically. Analyses were made by introducing weighed bulbs, full of the liquid, into flasks containing a certain quantity of water; breaking the bulbs afterwards, almost the whole of the silica, when sufficient water was present, remained in solution. The acid liquid, saturated with ammonia, was evaporated on the water-bath; the residue dissolved in water and filtered gave on the one side silica mixed with the glass of the bulb, on the other a solution in which the chlorine was determined. The numbers obtained lead to the formula Si_2OCl_6, from which the new body is seen to be an oxychloride of silicium.

Paris, March, 30, 1868.

Meteorites.—New Compound of Platinum.—Eruption of Vesuvius.

There were few memoirs relating to chemistry at the séance of the 30th March. M. Daubrée gave some further account of various meteorites. M. Balard presented a note by M. Schutzenberger on a new compound of platinum. M. Frémy presented a note by M. Terreil, "On the action of saline solutions on minerals." Besides these there was a geological paper entitled, "On the actual eruption of Vesuvius," from M. Silvestri. The first meteorite M. Daubrée referred to was one found in the Philippine Isles, not far from the village of Mexico, province of Pampanga, said to have fallen in 1859. It appears to be of the common type; it consists of a confused, stony, crystalline mass, chiefly composed of magnesian silicates, in which are disseminated bright particles, having a metallic lustre; some are grey, and are particles of nickeliferous iron, the others, black, are composed of chrome iron; the latter are very numerous. The meteorite is traversed by black veins, which give it a marble-like appearance; it presents great resemblance, both in the pale portions and in the dark veins, to the meteoric stone which fell on the 5th August, 1812, at Chantonnay. The density of the meteorite from the Philippines is 3·61, that of the meteorite from Chantonnay 3·67. Treated by boiling hydrochloric, the meteorite now studied by M. Daubrée leaves 28·5 per cent. of a residue not at present examined; the solution contains magnesia, protoxide of iron, a little oxide of nickel, and a very small quantity of alumina. Another meteorite, which fell at Murcie, in Spain, on the 24th December, 1858, formed the subject of a memoir by MM. Daubrée and S. Meunier. This meteroite is a very remarkable one, and was exhibited at the Universal Exhibition of 1867. Its density is 3·546, and it weighs about 111 kilogrammes. The mass is nearly entire, that is to say, the crust is almost everywhere apparent. This crust does not present the aspect ordinarily presented by stony meteorites; it has evidently suffered profound alteration since its formation. Particles having a metallic lustre are rare, but there are some—they consist of nickeliferous iron. In other places bronze yellow particles are seen, which have the characters of troilite. There exist, also, in this meteorite very brilliant particles possessed of a kind of metallic lustre. These particles also form veins. A careful examination has shown them to be crystals of hyaline. By the blowpipe flame they are melted to a greyish enamel, and give the reactions of silica and alumina. The black portion of the meteorite which was considered least altered, was analysed by M. Meunier. The magnet separated 14·99 per bent. of magnetic matter formed of nickeliferous iron and a trace of phosphide. Foremost among the analytical results, sulphide of iron is observed as making 20·52 per cent.; this is doubtless the cause of the dark colour. A silicate attackable by hydrochloric acid, of the nature of peridote, gives 38·69 per cent., and a silicate resisting this acid, of the nature of pyroxene, 24·64 per cent.

M. Schützenberger, in endeavouring to effect the synthesis of oxychloride of carbon without the intervention of light, made a mixture of dry carbonic oxide and chlorine pass over platinum sponge heated to 400°. Under these conditions, the formation of sensible quantities of oxychloride of carbon takes place, but the platinum does more than exert catalytic action. A solid and volatile compound of platinum is produced, which passes away with the current of gas, and may be collected in the form of a clear yellow flaky powder in the cold part of the tube. As the platinic compound is destroyed at a temperature little above that at which it is formed, to succeed, the current should be rapid. The new substance melts at about 150°, yielding a transparent yellow liquid, which on cooling solidifies to a crystalline yellow mass: in other experiments an analogous product has been obtained, melting at 125°, whence the substance would not seem to be homogeneous. At a temperature of 350 to 400° it boils and distils, but decomposes in great part into metallic platinum and chloroxycarbonic acid. The substance is decomposed by water immediately in the cold, an effervescence of carbonic acid being produced, at the same time that a fine black powder is separated, and the filtered liquid, quite colourless and free from platinum, gives the reactions of a solution of hydrochloric acid. The black powder is pure platinum (representing the whole amount of platinum in the body) possessed of great catalytic power, and below redness, it is converted, sometimes with incandescence, into very coherent metallic platinum. The mode of formation, and the decomposition of this body, under the influence of heat, and with water, leave no doubt as to its constitution. It is a compound of platinum, chlorine, and carbonic oxide. The most simple formula would be $(\Theta\Theta)''$, $\mathrm{Pt}''''\mathrm{Cl}_2$, in which Pt = 66·55, Cl = 23·9, and C = 4·05. Analysis has not confirmed this formula. A specimen crystallised from tetrachloride of carbon, and very pure in appearance, was analysed; it gave Pt, 63·5; Cl, 22·9; C, 5·35. These numbers lead to the formula $(\Theta\Theta)3\ \mathrm{Pt}_2\mathrm{Cl}_4$, in which Pt=63·5, Cl=22·9, C=5·8. The product first obtained was, as has already been mentioned, purified by chloride of carbon; the crystals obtained after several purifications contained less platinum, and gave 60·87 per cent. upon analysis, afterwards a minimum of 58 per cent. of platinum. M. Schützenberger proposes to examine this point further.

M. Terreil's note will be referred to again.

M. Silvestri has examined the phenomena connected with the eruption of Vesuvius closely, and analysed many of the volcanic products. The lava is dark grey coloured, sometimes greenish or reddish on the surface. The substance possesses a crystalline structure; it exerts an energetic action on the magnetic needle. M. Silvestri distinguishes four kinds of lava; the density of these varied between 2·46 and 2·81; in a complete analysis recorded of one, compact lava, water was found to be present to the extent of 2 per cent. Three distinct kinds of sublimates were noticed, viz., white, greyish brown, and green. The white sublimate contained chloride of sodium and chloride of potassium, besides minute traces of chloride of copper. Oxide of copper is present in the greenish brown sublimate, of which it forms 5·85 per cent. The green sublimate contained ·61 per cent. of oxychloride of copper; all these sublimates are mainly composed of chlorides of sodium and potassium. A large quantity of the sublimates was dissolved in water and a series of crystallisations made, in the mother-liquor reduced to a small volume, iodine and bromine were sought for: neither were detected. A spectral examination of the mother-liquor revealed nothing but sodium, potassium, and copper. M. Silvestri made his observations at the end of December, about the time of the maximum activity.

PARIS, APRIL 6, 1868.

A new Bank Paper.—Reaction of Sulphuric Acid upon Iodide of Potassium.—Mode of Development of Heat and Cold.

THE following memoirs were communicated to the Academy on the 6th inst. "Researches on the combinations of molybdic acid," by M. Debray; "Note on the manner in which sulphuric acid and iodide of potassium act when in contact," by M. Houzeau.

M. Durand read a memoir having for its title, "On the mode of development of heat and cold from a physical point of view;" this memoir was sent to the physical section.

M. Armand submitted to the Academy a new bank paper which he considered inimitable; it was sent to the chemical section. M. Debray's memoir was one of great interest; the translation has already been published in your columns.

All chemists know that iodide of potassium is immediately decomposed with liberation of iodine by ordinary sulphuric acid, but M. Houzeau has shown that an extreme degree of dilution paralyses the chemical affinities to such an extent that the dilute solutions may be boiled together without any change occurring either in the iodide or acid. It was not therefore without surprise, M. Houzeau says, that he read in a recent number of the *Comptes Rendus* a note on the pretended reaction which sulphuric acid always exerts, even in the cold, upon iodide of potassium. Even supposing that the author of this note had used extremely dilute solutions, and that he had operated on neutral iodide and on sulphuric acid deprived of nitrous compounds, his result is easily explained. The ether which served to characterise the reaction of the iodide on the acid, is precisely the reagent which should not have been employed, for this it is that provokes the reaction. M. Houzeau remarked that the confusion here between cause and effect was the less inexplicable when M. Schönbein had, several years ago, pointed out ether to be both a producer and a vehicle of oxygenated water. Thus the peroxide of hydrogen which determined the oxidation of the alkali metal of the iodide and set the iodine at liberty, in the disputed experiment was carried by the ether. Far from contradicting the exactitude of his method of determining oxygenated water, this experiment, M. Houzeau says, confirms it, and shows the great degree of sensibility of the iodide for traces of oxygenated water. He was perfectly acquainted with this perturbing cause, and to avoid it he proposed the employment of pure chloroform, which, besides being more sensitive to colouration by iodine, never provokes the mutual reaction of iodide of potassium and sulphuric acid. In conclusion, M. Houzeau maintains the fact to be incontestible, that a mixture of neutral iodide of potassium and pure sulphuric acid remains unaltered in sufficiently dilute solution, and in the conditions indicated in his works on ozone and oxygenated water.

M. Brouzet addressed a note referring to a process for separating good silk-worms' eggs from bad ones. The process consists in treating first with nitrate of silver, and then submitting the eggs to a kind of sorting, by means of their very different densities in water.

GLASGOW CHEMICAL SOCIETY.

THE inaugural meeting of the Glasgow Chemical Society was held on Monday evening last, in the Hall of the Philosophical Society. There was a very large attendance.

The PRESIDENT, Professor Thomas Anderson, M.D., F.R.S.E., occupied the chair.

After the minutes of the former meeting were read and approved of, and nine new associates proposed and admitted into the Society,

Dr. ANDERSON, in a few introductory remarks, thanked the members for having appointed him to the office of President of a Society of whose future success he felt great confidence. He felt assured that the new Society would have a long career of usefulness, and that its members were very fortunate in having, as the first communication to the Society, the paper to be submitted to their notice that evening, by M. Ludwig Mond, on his remarkable process for the recovery of sulphur from the black-ash waste of the alkali works. The President then called upon

M. **Mond**, who is at present practically putting his process in operation in the alkali department of Messrs. Charles Tennant and Company's Chemical Works, St. Rollox. The paper of M. Mond gave, in clear and intelligible English, an elaborate account of his recovery process from the commencement of his labours. It also referred to the other processes which have been brought under the notice of alkali manufacturers from time to time, to effect the same object, and showed wherein they had failed to meet with the success which had in such a marked degree attended the application of his process. In the outset M. Mond referred to the vast importance of the alkali trade, and characterized the St. Rollox Alkali Works as the most important and interesting of their kind in the world, not only on account of their vastness, but because a very considerable number of the most valuable improvements in the manufacture of alkali and its cognate industries have originated or been first adopted in them. The manufacture of bleaching powder, which has become so extensive that it can hardly be now called a secondary product, was invented by the founder of the firm, Mr. Charles Tennant, and is still carried out in the St. Rollox Works on a larger scale than in any other similar establishment in the world. Among the many other improvements which have first been applied in Messrs. Tennant's Works, M. Mond instanced the now famous apparatus for the lixiviation of black-ash, on which the final success of his process altogether depends, and regarded it as remarkable that the first apparatus ever put up for this purpose is at present employed for his sulphur-recovery process. He then mentioned some rather astonishing details illustrating the great development of the alkali trade in Great Britain within the last four years. In the year 1864 the quantity of common salt decomposed in this country was about 288,000 tons, and it rose to about 400,000 tons in the year 1867, or about 40 per cent. This quantity of salt requires about 320,000 tons of oil of vitriol for its decomposition, and this amount contains nearly 100,000 tons of sulphur. At present the sulphur is nearly all obtained from iron and copper pyrites, which are supplied at a much cheaper rate than brimstone, owing to the successful working of one of the largest mines in Spain, by the Tharsis Mining Company, which has been principally formed amongst Glasgow gentlemen, and especially by the intelligence and perseverance of Mr. William Henderson, who has brought his process of copper extraction from the residual burned ore to an unprecedented pitch of perfection. Notwithstanding the extensive use of pyrites in the vitriol manufacture, Sicily still enjoys a sulphur monopoly, and exports annually a very large quantity of that substance—a quantity which last year amounted to upwards of 200,000 tons, of which about 50,000 tons was consumed in Great Britain. M. Mond considered that by his recovery-process British alkali manufacturers might make themselves independent of Sicily as the source of their sulphur supply, inasmuch as they have a material which has hitherto been a source of inconvenience and outlay to those manufacturers in whose operations it is unavoidably produced, and from which the sulphur can be obtained at a much cheaper rate than that at which it can be imported. He then, in a clear and intelligent manner, described the apparatus in which the process is conducted, the *modus operandi* of the process, and the chemical changes which are involved in it. He also practically illustrated the process in the presence of the members, and produced a very decided quantity of sulphur. A variety of specimens, illustrating the various stages of the process, were shown to the members. (The details are essentially the same as those contained in the paper by M. Mond, which appeared in the **Chemical News** for 19th and 26th of July of last year [*Am. Repr.*, Sept., 1867, pp. 117-120]. To this paper our readers are referred.) M. Mond regretted very much that the chemistry of the polythionic acids so intimately connected with his process, had hitherto received so little attention from chemists.

At the conclusion of the paper the President congratulated the author on the great success and simplicity of the process which he had given to the alkali manufacturers, and on the interesting manner in which he had brought it under the notice of the Society, and then at some length gave an account of a sulphur-recovery process which he had seen in operation at Dieuze.

Mr. E. C. C. **Stanford** and one or two other gentlemen spoke, but as the time was far advanced there was very little opportunity for discussing M. Mond's interesting and valuable paper. The author was awarded a hearty vote of thanks.

ROYAL INSTITUTION OF GREAT BRITAIN.

Weekly Evening Meeting, Friday, March 20, 1868.

His Royal Highness the Prince of Wales, K.G., *in the Chair.*

"*On Alloys and their Uses*," by Professor Augustus Matthiessen, F.R.S.

The object of this discourse was to show experimentally why alloys are used in preference to their component metals.

Alloys may be, chemically considered, divided into three classes:

1. Chemical combinations.
2. Mechanical mixtures.
3. Solutions of the one metal in the other which have become solid; or, for shortness sake, solidified solutions of the one metal in the other.

Under the term chemical combination such alloys may be considered which are the result of the combination of two metals when these unite together with great energy and evolution of heat, producing an alloy the physical and chemical properties of which we cannot foresee. As an example of such alloys those of gold, with tin, lead, or zinc may be quoted; for if to melted tin, lead, or zinc, gold be added, the two metals unite together with great energy and produce an alloy which is exceedingly brittle and totally unfit for practical purposes.

It is for this reason that the more expensive metals, silver and copper, are used for alloying gold for the purposes of coinage, &c.

With regard to such alloys which may be looked upon as mechanical mixtures, like oil and water, or rather as ether and water, for no two metals are known which, like oil and water, do not dissolve at all in one another, but a few metals are known which, like ether and water, dissolve slightly in one another, for ether will dissolve a certain amount of water, and water a certain amount of ether. If ether and water be mixed together, say in equal parts, two layers will be formed, the top one being ether containing a little water, the lower one water containing a little ether. Two metals, for instance, which behave in exactly a similar manner to ether and water are lead and zinc, for lead when fused with zinc will dissolve 1·6 per cent. zinc, and zinc in its turn will take up 1·2 per cent. lead.

If these two metals be fused together, say in equal parts, they will separate into two layers, like ether and water, the top one, being the specifically lighter, zinc, with a small percentage of lead, the lower one lead, with a small percentage of zinc. If such an alloy be made and cast in a mould, the difference in the behaviour of the two ends may be easily shown; for the top one is so brittle that it cannot be bent without breaking, whereas the lower one may be bent with ease.

Such chemical combinations and mechanical mixtures are, however, comparatively rare; and for alloys in common use, practice has almost invariably chosen such alloys as may be considered as belonging to the third class, rejecting those of the first and second as worthless for practical purposes.

Under the term solidified solutions of the one metal in the other, such alloys may be considered, which, like the chlorides of potassium and sodium when fused together, produce a mass having some of the physical properties totally different from those of the component salts. It cannot be assumed that the chloride of sodium enters into chemical combination with the chloride of potassium. One important property of a solidified solution is, that the components are homogeneously diffused in one another, so that even under the most powerful microscope they can no longer be distinguished from one another.

Alloys are used because they possess certain physical properties to a far greater extent than their component metals. The physical properties may be divided into two classes.

1. Those which in all cases are imparted to the alloy, approximately in the ratio in which they are possessed by the component metals.

2. Those which in some cases are, and in others are not, imparted to the alloy in the ratio in which they are possessed by the component metals.

To the first belong Specific Gravity, Specific Heat, and Expansion due to heat. It is easy to show this experimentally; the specific gravity of an alloy may be shown to be equal to the means of those of its component metals, by hanging on the one side of a balance the alloy and on the other side the metals composing it unalloyed, and then placing them both in water.

The specific heat of an alloy may be proved equal to that of its components by placing the alloy and its components in boiling water, and then in equal volumes of cold water; when the rise of temperature in the two cases will be found the same, as may be shown by a differential air thermometer.

A brass bar placed in any apparatus for showing expansion by heat is seen to expand exactly as much as a composite bar, of which one portion is of copper, the other of zinc. The length of the zinc portion being proportional to the amount of zinc in brass.

To the second class of physical properties belong, Conduction for Heat and Electricity, Hardness, Tenacity, &c.

As a basis for the conclusion which will be drawn, the electric conducting power for alloys may be taken. Researches into this subject have shown that when tin, lead, zinc, or cadmium are alloyed together, such alloys conduct electricity in the ratio of the relative volumes of the component metals, whilst in all other cases no such simple relation exists between the conducting power of the metals and their alloys. If, for instance, gold be alloyed with silver, say in equal volumes, the conducting power of an alloy will be 15, that of silver being 100, and that of gold 80.

If curves be drawn to represent the conducting power of different series of alloys, three typical forms will be observed: the first represented by nearly a straight line, the second by the letter *L*, and the third by the letter U.

Wiedemann and Franz have proved experimentally that the values obtained for the conducting power of metals and alloys, for heat and electricity, are identically the same; and the truth of this statement may be shown by the following experiment: If bars of gold and silver and some gold-silver alloys be fixed so that one end of all of them is in a hot-water box and the other end in the bulb of a small air-thermometer, the depression in the columns of the liquid in the tubes of the air-thermometers will indicate the relative conducting powers (approximately) of the several bars; and if through the tops of the columns of liquid a line be drawn, such line will form a curve similar to that referred to as obtained for the electric conducting power.

That this is true is thus shown:

By the side of this apparatus is placed another of this construction: Into the bulbs of several air-thermometers are fixed wires of the same size and length, and of the same materials as were used in the heat-conducting experiment. One end of each wire is soldered to one thick copper wire, and the other end to another similar wire. These two wires are connected to the poles of a battery. The current will then divide itself, and a portion will pass through every wire proportional to the conducting power of that wire. This current will heat the wire and cause the liquid in the tubes connected with the air-thermometers to descend, and the line drawn through the top of the columns will be nearly similar to the curve already mentioned, which is formed by the bulbs in which the heat-conducting bars are fixed.

The analogy between the relation existing in this case and in some others may be shown experimentally as follows:

Sonority. When bars of alloys and their component metals are struck, a great difference will be found in the note produced; and in almost every case where the experiment has been made, the most sonorous alloy was found to correspond in composition approximately with that at the turning point of the electric conducting power curve.

Tenacity. When wires of the same diameter of metals and alloys are broken by traction, those of the alloys will require a much greater force than their component metals; and it may be deduced from what is known, that those alloys, the composition of which corresponds to the turning point of the conducting power curve, are more tenacious than any other alloy composed of the same metals.

Elasticity. When spirals of wires of metals and their alloys are weighted to an equal extent, the alloys will be found on removing the weights to possess the property of resuming their original form in a much higher degree than their component metals. Here again the alloys corresponding in composition to those of the turning point of the conducting power curves are the most elastic.

From what has been said, and from the experiments described, the conclusion may be drawn that the chemical composition of the practically-used two-metal alloys correspond to those situated at the turning points of the heat and electric conducting power curves, and that if a two-metal alloy of a special physical property be required, it would be as well to try that alloy, the composition of which would correspond to the turning point of the curve representing the electric conducting power of the alloys of the two metals.

MANCHESTER LITERARY AND PHILOSOPHICAL SOCIETY.

Ordinary Meeting, March 31st, 1868.

EDWARD SCHUNCK, Ph.D., F.R.S., *&c., President, in the Chair.*

"*Description of a Dolerite at Gleaston, in Low Furness,*" by E. W. BINNEY, F.R.S., F.G.S.

DURING the last thirty years the tract of land known as the Hundred of Low Furness has been investigated and described by several geologists. It was one of the earliest fields investigated by the venerable Sedgwick, who has left us a most valuable memoir of his labours in that district. Since then, Mr. Jopling, myself, and Sir R. I. Murchison, and Professor Harkness, have published descriptions of the silurian mountain limestone and permian formations of the country. Miss E. Hodgson has also given us information as to the drift deposits overlying the palæozoic strata. Still, notwithstanding what has been done, it may confidently be asserted that the peninsula comprising the southern part of the Hundred of Low Furness has yet to be carefully examined before its geology can be said to be thoroughly known.

None of the above-named persons appear to have been aware of the occurrence of any trap dykes in this district, judging from their published writings, with the exception of Mr. Jopling, who, in his sketch of the geology of Low Furness and Cartmel, comprehending the Hundred of Lonsdale north of the sands, published in 1843, when speaking of the geology of Gleaston, says:—"Carboniferous limestone abounds, and in the quarries near the castle are many fossils beautifully preserved in the shale beds between those of the limestone; there is also a vein of trap." At page 72 the same author says "there are also appearances of trap near Gleaston, associated with limestone breccia."

In the month of October last, Miss E. Hodgson was so kind as to send me some specimens of rocks from Gleaston, which puzzled her a good deal. Some of the parties to whom she had sent them called them dolomites, whilst others named them traps and greenstones. To the latter opinion Miss Hodgson, I believe, was inclined to add the weight of her sanction. Not having previously seen, or even heard of, the occurrence of any such rocks in the district where they were said to be met with, I went over to examine them, and having been furnished with information by Miss Hodgson, easily found the place where they are exposed at Gleaston Green. At that time Mr. Jopling's book had not been seen by me. The space occupied by these singular rocks, at least so far as at present exposed, is so limited that all that can be seen is very soon ascertained. Specimens were collected, and a few observations made. The former, by the kindness of my friend Professor Roscoe, F.R.S., were analysed for me in the laboratory of Owens College. It is only by the labours of the chemist that geologists can with any certainty decide upon the age and origin of such rocks as those which are met with at Gleaston.

On approaching Gleaston Green from Scales, the mountain limestone appears to occupy the country so far as it can be seen. In a quarry below the old castle on the roadside, this rock in the northern part is very hard, and dips to the west at an angle of 25°, whilst in the southern part, where it is softer, it dips in the same direction at an angle of 16°. Owing to the covering of drift, the limestone is not seen nearer to the mill, but it probably extends further in that direction. At a short distance below the mill, dark-coloured laminated shales are seen in the bank on the roadside, dipping apparently at an angle to the N.N.W. We then come to the rocks at the end of the Green. They appear to run in an east and west direction, and are not now exposed for more than twenty yards. From north to south they may probably extend about forty yards, but certainly for more than half of that distance, towards the beck, they are not now seen until the land rises on the bluff south of the beck, where they reappear as a reddish and bedded trap ash, having an east and west direction, and dipping N.N.W. at angle of about 60°. This ash is succeeded by a coarse breccia of a few yards in thickness, so far as exposed, which dips slightly north of west, at an angle of 25°, and then is covered up by grass so as not to be seen, but no doubt, from sections in the adjoining lane and borings made on the rise of the strata, dark-coloured shales occur again, and the dyke most probably intrudes through these shales, which are in every respect like limestone shales, but no organic remains were observed in them so as to make us certain of their geological age.

Returning to the north side of the beck, nothing is exposed of the district west of the hard rocks seen on the Green, owing to the thick covering of drift in that direction; but Mr. Hodgson has proved by a series of bore-holes, the occurrence of upper permian sandstone, red shale and limestone shale—the first to the S.W., the second to the west, and the third to the N.W. of Gleaston; and Mr. Ashburner has proved limestone shale and limestone in bores to the E. and N.E. of the locality where the rock is found.

In the bluff on the south side of the stream, as previously stated, the rock appears more like a trap ash of a reddish brown colour and exhibits traces of bedding and white lines like carbonate of lime. Immediately adjoining trap ash, and on its rise, occurs the coarse breccia composed of fine-grained siliceous rocks, cemented together with quartz, and like a permian breccia; but although the beds are near together, there was not evidence to show whether the trap ash gradually passed into the breccia or intruded through it, still the breccia appeared to dip in the same direction, but at a much less angle, namely, 25° to a little west of north. This is a very interesting fact to prove; for if the rock graduates into the breccia, it would appear to be of permian age, and most probably a melaphyr, but if it is intrusive, as the evidence on the whole appears to prove, all we can say is that it is of later date.

This breccia is composed of angular pieces of a fine siliceous stone, of a pink colour, more resembling quartzite than anything else, cemented together by small quartz crystals, and containing minute quantities of protoxide of manganese. The form of the fragments is very like that of the rocks in the permian breccia of Rougham Point, near the mountain limestone of Humfray Head, and there consisting for the most part of the neighbouring mountain limestone, but no limestone has yet been met with in the Gleaston breccia as might have been reasonably expected; and the pink quartzite is a rock hitherto unknown in the district. The permian breccia, so far as my experience goes, although sometimes containing volcanic ash, are composed of the rocks now found in the neighbourhood where they occur, and nearly always vary with the older geological rocks of the district. The composition of the Gleaston breccia makes me hesitate in designating it as permian, as it may be some rock altered by the dyke.

The rock in its best state of preservation is remarkably hard, of a reddish brown colour, has a moderately straight fracture, and a pinkish white streak, and its specific gravity is 2·92.

Three average samples of the rock were taken, two from the north and one from the south side of the beck. All of them were more or less decomposed by exposure to air and moisture, but No. 26 much less than Nos. 23 and 24. Professor Roscoe, on analysis, found their present chemical composition to be as follows:—

	South of Stream, No. 23.	North of Stream, No 24.	North of Stream, No. 26.
Silica	45·54	50·96	51·10
Peroxide of iron	24·76	24·20	21·58
Alumina	7·70	14·48	9·40
Lime	13·84	7·32	6·24
Magnesia	0·57	0·55	1·33
Carbonic acid	2·78	1·90	2·70
Alkalies, water (by difference)	4·82	0·59	7·65
	100·00	100·00	100·00

The only rock which I know of a similar composition, is a probable variety of green earth, resembling a decomposed pyroxene, described by Macfarlane in the *Canadian Naturalist*, New Series, vol. iii., No. 1, page 5, in a paper on the cupriferous bed of Portage, Lake Michigan, which consists of—

Silica	46·48
Alumina	17·71
Protoxide of iron	21·17
Lime	9·89
Magnesia trace	—
Alkalies (by difference)	1·97
Water	2·78

Mr. David Forbes, F.R.S., to whom were forwarded small specimens of the rocks and the above analyses, kindly informed me that the rocks were so much decomposed that it was difficult to pronounce with certainty as to what they were, but he was inclined to think that they were an intrusive dolerite of carboniferous age rather than a melaphyr. The iron had been changed from a protoxide into a peroxide, and the lime had resulted from the decomposition of a lime felspar. As Mr. Forbes had found the presence of titanium united with iron in all the carboniferous dolerites he had examined, I took several ounces of the three samples above given, and having heated them in a crucible so as to convert the iron from a per- into a protoxide, extracted it by a magnet. About half an ounce of this iron was very carefully examined by Mr. Thorpe in Dr. Roscoe's laboratory, especially for titanic acid, and no trace of that substance was found. He used the test with microcosmic salt, having separated iron and silica. The absence of titanium in the rock would lead us to believe that it was of later origin than the carboniferous age, but if traces of that metal had been found, it would not only have settled the question as to the age, but it would have shown a connection with the hæmatite iron ores of Whitehaven and Ulverston, all of which contain more or less of titanium as proved by the deposits of that metal found on the sides of old furnaces where hæmatite has been smelted. Is the rock of permian age? It is certainly not much unlike the melaphyr of the German geologists, and the breccia near the dolerite is not greatly different from that of Ballochmyle, described by Mr. A. Geikie, F.R.S., in the *Geological Magazine* for December, 1866, but we could not obtain direct evidence that the breccia gradually passed into the trap, the latter appeared to protrude through it, but certainly the trap and the breccia dipped in the same direction, the one at about 60° and the other at 25° a little West of North. This point can only be satisfactorily determined by cutting a trench and showing the contact of the breccia with the trap. The extent of the dyke can only be traced for a few yards east and west, as previously stated, and none of the hæmatite iron deposits, so far as known, have been found south of it. Its age also appears to be more recent, even supposing it to be permian, than those deposits which for the most part must be considered of carboniferous age. The occurrence of this trap might have been considered to have some connection with the deposition of the iron had it been of carboniferous age, but it is evidently more recent, and therefore could have had nothing to do with it further than disturb or displace it.

"*A Search for Solid Bodies in the Atmosphere*," by R. Angus Smith, Ph.D., F.R.S., &c.

I have so frequently for many years attempted to find, and have found organic substances which have passed from the air into liquids in which they were collected, that perhaps the Society will scarcely attend to another attempt, although it indicates, I think, some progress. It was in the year 1847 that I first collected what I believe was matter from the respiration and perspiration, and found that as it was kept it grew into distinct confirmed forms.

Whilst examining some matters relating to the cattle plague I found one or two remarkable points. I had before that time used aspirators to pass the air through liquids, except in the oxidation experiments. At that time I used simply a bottle which contained a little water. The bottle was filled with the air of the place, and the water shaken in it. The difference of air was remarkable. A very few repetitions would cause the liquid to be muddy, and the particles found in many places were distinctly organic.

It may, however, interest the Society to hear of a few of these previous attempts, the latest made till recently. I shall therefore read from a report to be found in the appendix to that on the cattle plague.

"Mr. Crookes also brought me some cotton through which air from an infected place had passed. It was examined at the same time. Taking cotton in the mass nothing decided was seen; but when it was washed some of the separate films were coated over with small nearly round bodies, presenting no structure, or at least only feeble traces of it, and perhaps to be called cells. I had not sent gun-cotton, as I intended, to Mr. Crookes, fearing the rules of the post; otherwise there would have been more certainty that the bodies spoken of did not exist previously on the cotton. However, Mr. Dancer, who has examined cotton with the microscope oftener than most persons, even of those experienced in the subject, had never observed a similar appearance.

"The liquid had also a number of similar bodies floating in it.

"It was then that Mr. Crookes sent a liquid which he had condensed from the air of an infected cowshed at a space a little above the head of a diseased cow. It was also examined, and it presented similar indications of very numerous small bodies. Not being a professed microscopist I shall not attempt a description, but add that they clearly belonged to the organic world, and were not in all cases mere débris. We found also one body a good deal larger than the rest; it resembled somewhat a paramecium, although clearly not one.

"We found no motion whatever, and only this latter substance could be adduced as an absolute proof of anything organized being present. Next day I examined the same liquid; and, whether from the fact of time being given for development or from other causes, there was a very abundant motion. There were at least six specimens in the field at a time, of a body resembling the euglena, although smaller than I have seen it. When these minute bodies occur it is clear that more may exist, and germs in this early stage are too indefinite to be described. The existence of vital sparks in the organic substances in the air alluded to is all I wish to assert, confirming by a different method the observations of others. It might, of course, be said that since the bottle was opened at Mr. Dancer's, the air at that place may have communicated them. I answer that, before it was opened, a good glass could detect floating matter, some of it, however, as in the microscope proved, indefinite enough.

"Finding this, and fearing that the long time needful to collect liquid from the atmosphere might expose it also to much dust, I used a bottle of about 100 cubic inches dimensions, and putting with it a very little water, not above five cubic centimetres, I pumped out the air of the bottle, allowing the air of the place to enter. This was done six times for each sample, the water shaken each time, and the result examined. This was done with the same bottle that was used in my early experiments with permanganate, and by the same method, except that water instead of that salt was used. At first considerable numbers of moving particles were found; but it was needful to examine the water used, and here occurred a difficulty. It was not until we had carefully treated with chemicals and then distilled the water again and again that we could trust it. Particles seemed to rise with the vapour, and if so, why not with the evaporating water of impure places.

"Having kept an assistant at the work for a week, and having myself examined the air of three cow-houses, I came to the conclusion that the air of cow-houses and stables is to be recognised as containing more particles than the air of the street in which my laboratory is, and of the room in which I sit, and that it contains minute bodies, which sometimes move, if not at first, yet after a time, even if the bottle has not been opened in the interval. There is found in reality a considerable mass of débris with hairs or fine fibres, which even the eye, or at least a good pocket lens, can detect. After making about two dozen trials, we have not been able to obtain it otherwise. Even in the quiet office at the laboratory there seemed some indications.

"I found similar indications in a cow-house with healthy cows; so I do not pretend to have distinguished the poison of Cattle Plague in these forms; but it is clear that where these exist there may be room for any ferment or fomites of disease; and I do not doubt that one class is the poison itself in its earliest stage. It would be interesting to develope it farther.

"I have recorded elsewhere that I condensed the liquid from the air of a flower garden, and found in it, or imagined I found, the smell of flowers. I do not remember that I looked much to the solid or floating particles, thinking them to be blown from the ground, but it does not affect the result, whether they be found constantly in the air or are raised by the action of currents."

Lately I tried the same plan on a larger scale. A bottle of the capacity of 4990 c.c. was filled with air and shaken with water. The bottle was again filled and shaken with the same water, and this was repeated 500 times, nearly equal to 2½ million c.c., or 2,495 litres. As this could not be done in a short time, there was considerable variety of weather, but chiefly dry, with a westerly wind. The operation was conducted behind my laboratory, in the neighbourhood of places not very clear, it is true, but from which the wind was blowing to all parts of the town. I did not observe any dust blowing, but if there were dust, it was such as we may be called on to breathe. The liquid was clouded, and the unaided eye could perceive that particles, very light, were floating. When examined by a microscope the scene was varied in a very high degree—there was evidently organic life. I thought it better to carry the whole to Mr. Dancer, and to leave him to do the rest, as my knowledge of microscopic forms is so trifling compared to his.

Ordinary Meeting, March 31, 1868.

Edward Schunck, Ph.D., F.R.S., &c., *President, in the Chair.*

"*Microscopic Examination of the Solid Particles from the Air of Manchester*," by J. B. **Dancer**, F.R.A.S.

The air had been washed in distilled water, and the solid matter which subsided was collected in a small stoppered bottle, and on the 13th of this month Dr. Smith requested me to examine the matter contained in this water. An illness prevented me from giving it so much attention as I could have wished.

The water containing this air washing was first examined with a power of 50 diameters only, for the purpose of getting a general knowledge of its contents; afterwards magnifying powers varying from 120 to 1,600 diameters were employed.

During the first observations, few living organisms were noticed; but, as it afterwards proved, the germs of plant and animal life (probably in a dormant condition) were present.

I will now endeavour to describe the objects found in this matter, and begin in the order in which they appeared most abundant.

1st. *Fungoid Matter.*—Spores or sporidia appeared in numbers, and, to ascertain as nearly as possible the numerical proportion of these minute bodies in a single drop of the fluid, the contents of the bottle were well shaken, and then one drop was taken up with a pipette; this was spread out by compression to a circle ½ an inch in diameter. A magnifying power was then employed, which gave a field of view of an area exactly 100th of an inch in diameter, and it was found that more than 100 spores were contained in this space; consequently the average number of spores in a single drop would be 250,000. These spores varied from 10,000th to 50,000th of an inch in diameter. The peculiar molecular motion in the spores was observable for a short time, until they settled on to the bottom of the glass plate; they then became motionless.

The Mycelium of these minute fungi were similar to that of rust or mildew (as it is commonly named), such as is found on straw or decaying vegetation.

When the bottle had remained for 36 hours in a room at a temperature of 60° the quantity of fungi had visibly increased, and the delicate mycelial thread-like roots had completely entangled the fibrous objects contained in the bottle and formed them into a mass.

On the third day a number of ciliated zoospores were observed moving freely amongst the sporidiæ. I could not detect any great variety of fungi in the contents of the bottle, but I cannot presume to say that all the visible spores belonged to one species, and as there are more than 2,000 different kinds of fungi it is possible that scores of other species might be present, but not under conditions favourable for their development. Some very pretty chain-like threads of conidia were visible in some of the examinations.

The next in quantity is vegetable tissue. Some of this formed a very interesting object, with a high power, and the greater portion exhibited what is called pitted structure. The larger particles of this had evidently been partially burnt and quite brown in colour, and were from coniferous plants, showing with great distinctness the broad marginal bands surrounding the pits; others had reticulations small in diameter. They reminded me of perforated particles so abundant in some kinds of coal.

The brown or charred objects were probably particles of partially burnt wood used in lighting fires.

Along with these reticulated objects were fragments of vegetation, resembling in structure hay and straw and hay seeds, and some extremely thin and transparent tissue showing no structure. These were doubtless some portions of weather-worn vegetation. A few hairs of leaves of plants and fibres, similar in appearance to flax, were seen, and, as might have been expected in this city, cotton filaments, some white, others coloured, were numerous; red and blue being the predominant colours. A few granules of starch, seen by the aid of the polariscope, and several long elliptical bodies, similar to the pollen of the lily, were noticed. After this dust from the atmosphere had been kept quiet for three or four days, animalculæ made their appearance in considerable numbers, the monads being the most numerous. Amongst these were noticed some comparatively large specimens of paramecium aurelia, in company with some very active rotiferæ; but after a few days the animal life rapidly decreased, and in twelve days no animalculæ could be detected.

Hairs of Animals.—Very few of these were noticed, with the exception of wool; of this both white and coloured specimens were mixed up along with the filaments of cotton.

After each examination as much of the drop of water as could be collected by the pipette was returned to the bottle, in order to ascertain if any new development of animal or vegetable life would take place, and the stopper of the bottle was replaced as quickly as possible to prevent the admission of the particles from the air in the room; and I am tolerably certain that the objects named in this paper are those which the bottle contained when Dr. Smith brought it to me.

The particles floating in the atmosphere will differ in character according to the season of the year, the direction of the wind, and the locality in which they are collected, and, as might be expected, are much less in quantity after rain.

The small amount of fluid now remaining in the bottle emits the peculiar odour of mildew, and at present the fungoid matter appears inactive.

For the purpose of obtaining a rough approximation of the number of spores, or germs of organic matter contained in the fluid received from Dr. Smith, I measured a quantity by the pipette, and found it contained 150 drops of the size used in each examination. Now, I have previously stated that in each drop there were about 250,000 of these spores, and as there were 150 drops, the sum total reaches the startling number of 37½ millions, and these, exclusive of other substances, were collected from 2,495 litres of the air of this city*—a quantity which would be respired in about 10 hours by a man of ordinary size when actively employed. I have to add that there was a marked absence of particles of carbon amongst the collected matter.

ROYAL GEOLOGICAL SOCIETY OF IRELAND.

April 8, 1868.

Dr. Emerson Reynolds read a paper "*On the Formation of Dendrites.*" He had some years since noticed that when solutions of salts, &c., were placed upon a plate of clean glass, and the glass placed between the poles of a Ruhmkorff's coil, the salts gradually work over the surface of the glass in beautiful moss like forms, which in many cases were characteristic of the compound contained in solution. The state of dilution at the same time having some considerable influence. The authors proposed to call them "electric cohesion figures." To produce them we will say that a drop of a solution of cyanide of potassium is put in the centre of a plate of glass, which is then placed upon a sheet of tin-foil. One pole of the coil is then brought into contact with the foil (it is

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* Behind Dr. R. Angus Smith's laboratory.

immaterial which), and the other pole is placed in the centre of the drop; immediately on passing the current the solution begins to creep over the surface of the glass in moss-like convolutions.

The dendritic markings on minerals the author believed were formed under a similar condition. He exhibited a beautiful manganous dendrite taken out of the museum. It was a slab of concoidal limestone, and in Dr. Reynolds' opinion illustrated his electrical explanation conclusively. There was originally a flaw in the limestone which was exactly at right angles with the plane of cleavage. Through these flaws, as was evident by the marks, the manganous solution had percolated, and had perhaps ultimately been the means of making the stone part in two, not, however, in the direction of the flaws, but in the plane of cleavage. The dendrites which were formed upon the surface in this case were produced from the well-known fact that the two surfaces at the instant of their separation are in opposite electrical conditions.

This phenomenon may be illustrated to a certain extent by inserting a drop of the fluid into the interstice of a plate of mica, and then on suddenly parting the plate the dendritic forms are shown. To fix them the author dusts some finely dried pigment over the surface of the still moist plate, and then fixes this by some transparent varnish.

The other paper read was by J. Scott Moore, Esq., J.P.D.S., "*On Man and the Glacial Epoch.*" A modified spectroscope for examining minerals was also shown.

PHILOSOPHICAL SOCIETY OF GLASGOW.

CHEMICAL SECTION.

The usual fortnightly meeting of this section was held in the Philosophical Society's Hall, on Monday evening last; Alexander Whitelaw, Esq., Treasurer, in the chair. After the election and admission of a number of new associates, a paper on "*The Estimation of Potash,*" by Messrs. James Chalmers, chemist to the Kames Gunpowder Company, and Robert R. Tatlock, F.C.S., analytical chemist, Glasgow, was read to the Section. The authors urged that the subject was of much greater importance than the title would seem to indicate. Glasgow and its vicinity form the chief seat of the manufacture of potash salts from kelp, and form the destination of almost all the muriate of potash now manufactured from the interesting deposit in the vicinity of Stassfürth, as well as of the potash salts largely made from French beet-root; and as the value of these salts is fixed by the amount of potash they contain as determined by chemical analysis, it is obvious that in Glasgow, at least, an accurate and uniform method of estimating that base is of the utmost importance, as an indispensable adjunct to the manufacture and sale of potash salts. That such a desideratum had long since been supplied might well be supposed from the various methods of analysis described by eminent authorities, and from the results of the experience of many able chemists who have necessarily been much engaged in the analysis of potash salts. But unpardonable discrepancies constantly occur with regard to the results obtained by chemists of standing and experience, even when operating on the same carefully mixed and uniform sample; and these point to the conclusion that either the instructions given by those who may almost be regarded as infallible authorities have been misunderstood, and but imperfectly carried out, or, that in many instances the details of the methods as laid down are so imperfect as to be useless, if not even misleading.

The experience of the authors of the memoir, in the analysis of potash salts, has extended over a period of many years, during which they have conjointly made thousands of potash estimations, and hence they urge that they may be fairly entitled to claim some acquaintance with the subject.

By long and careful attention to the results obtained by other chemists, confirmed by their own experience, the authors have invariably found that the general tendency is to report potash too high, and the object of the paper was not only to trace the cause of this seemingly constant error, but also to furnish from the results of a laborious and protracted course of experiments, the true means of obviating that tendency, and obtaining not only constant, but, so to speak, absolutely correct results. In the analysis of salts not particularly rich in potash compounds, a too high result is most frequently obtained, but escapes detection under cover of the extraneous salts present,—the blame of the excess of potash reported being thrown upon the soda salts, which are not directly estimated. This error is usually discovered only when approximately pure salts are under investigation, when the analysis comes out impossibly high. It is almost superfluous to remark that any process which does not give from 99·9 to 100·15 per cent. with pure salt, is totally inadmissible. The authors have both a conviction and a positive assurance that the methods adopted by chemists—at least such methods as have come under their observation—do not give results so close to the truth as those just quoted; and it is from this reason that they were induced to investigate the whole subject, and bring the results of the investigation under the notice of the Section.

They admit that such remarks may seem to involve rather strong and severe strictures on experienced analysts, and that such strictures should not be hastily made when the accuracy and definite nature of modern chemical analysis are considered; but on finding differences of from 1 to 2 per cent. by different analysts, and results giving a total of 100 per cent. in a muriate of potash from the potassic chloride and water alone, while soda salts, insoluble matter, and sulphate of potash—a salt having a higher equivalent than potassic chloride—were ignored, they felt warranted in saying that serious errors were made. They claim no superior sagacity in chemical matters, but simply affirm that their positions and other circumstances directed their attention to the subject, and compelled them to investigate it. The errors and conflicting results are not necessarily the consequence of careless analysis or manipulation, but are chiefly due to an unsuspected source of error in the reagent employed. That these defects in "potash analyses" have so long escaped investigation, and even general observation, is perhaps owing to an unaccountable and mistaken reliance in what is generally called a "full analysis" of a muriate or other potash salt. In many cases of full analyses of compounds, when the sum of the various determinations amounts to 100 per cent., or very nearly, the analysis is generally accepted as trustworthy, and deservedly so if each of the ingredients or elements is estimated separately, and not calculated from the amount of another element; yet still, in the analysis of a commercial potash salt results approaching a total of 100 per cent. give no reliable check on the accuracy of a potash determination, as there is no *practical* method of determining soda, even indirectly, in presence of potash. After enlarging on the methods of estimating soda in presence of potash, the authors considered the general subject under the following heads:—

I. The chemical principles involved in the methods employed.

II. The manipulation of the process.

III. The calculation of results.

I. The authors regard the familiar method of determining potash in presence of soda—the one now almost exclusively practised, namely, that of precipitating potash as potassio-platinic chloride—as being superior to all others; but the details of it, as laid down in some analytical works, appear to the authors too meagre for guidance to correct results; and, indeed, with the exception of the writers of a few scattered notes, they know of no chemical authors, except Fresenius, who habitually subject analytical processes to a searching examination. The directions generally given are—to evaporate the potash solution to dryness with excess of platinic chloride, and to digest the residue in alcohol before filtering. As this treatment is inapplicable to salts containing an appreciable amount of soda, most practised analysts avoid this source of error, by stopping the evaporation somewhat short of dryness, and digesting the residue in strong aqueous solution of platinic chloride, which dissolves sodium compounds, but practically leaves the potassium precipitate intact. This process is capable of giving very correct results, if all the other essential points are attended to. Of the other conditions requisite to ensure accuracy, the most important is the purity of the platinic chloride solution; indeed, the authors regard this as the key-stone of the entire process—impure platinum and false results being as inseparably associated as crime and punishment. As pure platinic chloride solution is not the rule, but the rare exception, false results must be alarmingly numerous; and as most, if not all, of the methods usually followed in recovering platinum from spent solutions and precipitates are the means of introducing impurities that are not easily removed, the authors first briefly noticed those methods, and the objections to them, as founded on numerous trials made by themselves. The usual methods are four in number:—

1st. Reduction by nascent hydrogen produced by the action of zinc on dilute hydric sulphate.

2d. Reduction by alcohol in presence of excess of sodic hydrate.

3d. Reduction by cane sugar, or by glucose, in a solution strongly alkaline by sodic carbonate.

4th. Reduction by ignition of the precipitated and evaporated fluid washings in a Hessian or other crucible, as recommended by Miller, Abel, and Williams.

The authors tried all these methods most extensively, testing the platinic solution in the most rigorous way by making repeated estimations with perfectly pure potassic chloride. They used the compound sold by Griffin as chemically pure, but further purified it by successive crystallisations from distilled water. It contained the normal amount of chlorine; fluorine was sought for but not found. Solution of platinic chloride was prepared from Johnson and Matthey's spongy platinum, boiled in nitric acid, and washed before dissolving. This solution gave with pure potassic chloride results bordering on 102 per cent., using the equivalents accepted by certain practical authorities; nor could these results be brought much nearer the truth. This was certainly an alarming state of things, and showed that ordinary spongy platinum is not in a fit state for preparing pure platinic chloride.

Using the first method of recovering platinum, the authors found that if commercial zinc was employed, pure potassic chloride gave with the platinic chloride results far too high. They average from 101·67 to 102·05 per cent. Using purified zinc, Griffin's best, sold as being free from arsenic and for use in Marsh's test, but giving distinct indications of the presence of cobalt, the results were nearer the truth, but still too high, being from 101·37 to 101·58 per cent. The chief objection to this method is the great difficulty of procuring zinc free from lead. On one occasion one of the authors obtained a crop of chloride of lead crystals, weighing upwards of 90 grains, from the accumulated insoluble matters left on the filters during the filtration of platinic chloride solution prepared from the metal recovered by the zinc method.

It is the second method, or rather a modification of it, which the authors have used for some time in the recovery of platinum to be used in the analysis of commercial potash samples. They render the solution of platinum waste strongly alkaline by sodic hydrate, and boil with alcohol. The resulting platinum black is further purified by boiling in dilute nitric acid and soda solution, with intermediate and final washings with water. Thus prepared, the platinum invariably gave high results. The following are examples, pure potassic chloride and recognised factors being used:—101·77 to 101·95 per cent., and 101·12 to 101·24 per cent., according to the degree of purification by the acids.

The third method, as recommended by Böttger,* did not give pure platinum. The authors used both glucose and common white cane sugar. One set of analyses gave, with pure potassic chloride, from 101·3 to 101·6 per cent. In a second set—the purifying being carried to a prolonged degree—the results were from 100·76 to 100·94 per cent. To a certain extent, the authors deemed these results to be satisfactory; but if calculated by Stas's equivalents, they gave from 101·22 to 101·4 per cent. In this series of trials, carefully made—the manipulation being unchallengeable,—there were only four cases in which fair results were obtained with pure salts, by using platinic chloride prepared from platinum which had not been previously ignited to remove organic matter.

The authors deem the fourth method to be imperfect from the great difficulty of completely decomposing the potassio-platinic chloride. Simple ignition being insufficient, ignition along with nitre was tried. In some of the experiments with platinum thus obtained the following percentages were given:—100·03, 100·11, 100·16, and 100·22; but the results were always high when using less than four drachms of water to dissolve the precipitated muriate.

Many experiments—some detailed by the authors—were performed in duplicate in order to determine the best method of reduction and purification of the platinum. But very early in their inquiry they were led to suspect other sources of error; these they also investigated, and ultimately obtained results varying from 100·08 to 100·001, which are certainly satisfactory alike to science and commerce.

II. In respect of the manipulation of the process the authors object to the method of operating on such a small quantity as 10 grs., as it gives inconstant and unreliable results. (1.) Any error of the balance is greatly multiplied in calculating to per cent. (2.) The whole of the insoluble matter is necessarily included in the weight of the potassio-platinic chloride, or, worse still, the small quantity must be dissolved and filtered, and the solution evaporated—a most tedious and unsatisfactory mode of procedure. (3.) Electrical repulsion in dry and freshly wiped watch-glasses used in weighing, frequently causes a loss of the dried powder, and such powder is always hygrometric, and thus errors may occur in two ways. (4.) The amount dried from which the sample is taken is too small. The authors prefer to follow the advice of Fresenius and other authorities, and take, say, 500 grs., which they dissolve in a small quantity of water. They filter the solution into a 5,000 gr. flask which they fill up to the proper level with the washings and water at 60° Fah., and by means of a graduated pipette they remove for precipitation an aliquot part of the whole solution. In their paper the authors detailed the mode of performing the volumetric operations, and replied to the objections raised to the use of the pipette, and gave some details regarding actual pipette measurements. One of these was 99·97 volumes of water delivering 100 volumes of muriate solution.

III. It is not enough to master the methods of obtaining pure platinic chloride, and to manipulate the analysis of a potash salt correctly, as error would still result if wrong equivalents or incorrect factors be used for calculating the results. The authors have found that some chemists of high standing and experience in practical analysis use factors which are not only not based upon exact experiments, but give results from ·5 to ·75 per cent. too high when using pure potassic chloride, and when every other step in the process is rigidly correct. They then give the equivalents of platinum, potassium, and chlorine, as used by various authorities, and regard as most trustworthy those given by Stas, at the mention of whose name in connection with combining numbers, they suggest that every good chemist should cross himself and look devout. They said they would like to know where the factors ·194 and 1·585, as used by some analysts, were obtained. Those used by the authors are ·1925 and 1·584, and are based on Stas's numbers. No others, in their opinion, will give correct results, seeing that they have been determined with every refinement of which modern science is capable.

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* Chemical News, vol. xi., p. 168. (Eng. Ed.)

The final conclusions arrived at by the authors are :—

1. That the methods of analysis taught and practised in some laboratories are very imperfect.
2. That the use of the factor ·194, or any others than those founded on Stas's equivalents, is erroneous, and not based on reliable experiments.
3. That it is necessary to check the process used, and to be satisfied of the purity of reagents and other disturbing causes, by experiments with pure potassic chloride or other potassium salt; and that in no case should results be reported unless controlled by such experiments.

In the discussion which followed the reading of the paper, several members having much experience in potash analysis, supported the views of the authors, and highly complimented them upon the extraordinary industry displayed in the elaborate series of experiments referred to in the paper, and on the rigid care taken to avoid all sources of error. Messrs. Chalmers and Tatlock were heartily thanked for their interesting and valuable communication.

CHEMICAL NOTICES FROM FOREIGN SOURCES.

Neurin and Sincalin.—Claus and Keesé have made some experiments on sincalin with a view of elucidating the nature of its relation to neurin. After a careful comparison of the various derivatives of the two, particularly of the chloro-platinate and aurate, the authors come to the conclusion that they are identical.—(*Journ. pr. Chem.*, cii. 24.)

Naphthalene.—H. Vohl. Perfectly pure naphthalene has the specific gravity at 19° C.=1·15173; it fuses at 79°·25, and boils at 217° to 218°. The fused material absorbs large quantities of air,—that is to say, a mixture of nitrogen and oxygen containing nearly 50 per cent. of oxygen,—which escapes again on cooling just before solidification takes place. Fused or boiling naphthalene is a powerful solvent and medium for crystallisation for a variety of substances, as sulphur, phosphorus, sulphides, iodine, indigo, &c. As regards the detection of naphthalene the author makes use of the following reaction: to naphthalene is added nitric monohydrate, the mixture diluted with much water, the precipitate washed with water, finally with diluted alcohol (1 alcohol of 90 per cent., 3 water), the residue mixed with a few drops of aqueous potassic hydrate and sulphide, and evaporated to dryness; this residue on addition of alcohol gives a brilliant violet tincture.—(*Ibid.* cii. 29.)

Double Chloride of Thallium and Iron.—Wöhler. When freshly precipitated and still moist thallic chloride is added to a concentrated solution of ferric chloride containing much strong chlorhydric acid in excess, a red precipitate is formed, the composition of which is $3\,TlCl+Fe_2Cl_6$. Another way for preparing this compound consists in fusing thallic chloride in the vapour of ferric chloride. The double chloride dissolves in hot strong chlorhydric acid, and separates on cooling in red prismatic crystals. Water decomposes it immediately, throwing down white thallic chloride.—(*Ann. Chem. Pharm.*, cxliv. 250.)

Cerium.—Wöhler. On fusing the chlorides of the cerite metals with sodium, globules of reduced metal are obtained, which seem to consist principally of cerium; the colour of the metal is between that of iron and lead, and it is nearly as soft as lead; its specific gravity is about 5·5. If a globule is heated suddenly to a high temperature in a blow-pipe flame, combustion takes place, accompanied with an explosion, and sparks of most intense luminosity are thrown out. From the portion of the flux in which the metal is found imbedded, a glittering dark purple crystalline powder may be isolated by digestion with water; this compound is an oxychloride of cerium, its composition being represented by the formula, $Ce\,Cl+2\,CeO$.—(*Ibid.* cxliv. 251.)

Determination of Ammonia.—O. Meister shows that ammonia or its salts, when in a greatly diluted state, as in waters for example, may be successfully determined by evaporating one or two litres of the solution in question with the addition of about 5 grammes of sulphuric acid, and distilling the residue with a solution of sodic hydrate, previously boiled, into sulphuric or chlorhydric acid of known strength.—(*Naturf. Gesellsch. Zurich*, 1867, 172.)

Distillation.—P. Pellogio describes a contrivance by means of which the troublesome "bumping" peculiar to certain liquids when under distillation may be entirely prevented. It consists of a glass tube as wide as practicable, inserted through the tubulus, and reaching nearly to the bottom of the retort, and having the upper end bent at a right angle, and drawn out to nearly capillary dimensions, thus establishing a communication between the outer air and the interior of the retort. With the help of this arrangement such liquids as methylic alcohol, sulphuric acid, petroleum residues, &c., distil as smoothly as alcohol or water.—(*Zeitschr. Analyt. Chem.*, vi. 396.)

Acrolein.—A. Claus. If a solution of potassic hydrate, alcoholic or aqueous, is saturated with acrolein, and sulphuric acid be added, a precipitate of hexacrolic acid is obtained, and from the mother-liquor acrylic acid may be distilled off.—(*Naturf. Gesellsch. Freib.*, i. Br. 1867.)

Oxidation of Amylic Alcohol.—A. Claus. In a cylinder were placed, without mixing, nitric acid (specific gravity 1·5), water, and amylic alcohol. After about four months the smell of the alcohol had disappeared, that of amylic valerianate having taken its place. The mixture was then diluted with water and half of it distilled off; the distillate consisted chiefly of the ether, and the residue on further concentration gave off much nitric acid vapour, and on cooling separated crystals of oxalic acid.—(*Ibid.* 1867.)

Isomerism of the Hydrocarbons Θ_2H_{10} and Θ_4H_8.—A. Butlerow. There can be only two isomers of the composition Θ_4H_{10}; the one is ethyle

$$\left\{\begin{array}{l}\Theta H_2(\Theta H_3)\\ \Theta H_2(\Theta H_3)\end{array}\right.$$

the other trimethylformene $\Theta(\Theta H_3)_3H$, obtained by the author from trimethylcarbinol (tertiary pseudo-butylic alcohol) by the action of zinc on trimethylcarbinylic iodide. The reactions of the two compounds prove them to be isomeric, not identical. The action of chlorine, for instance, gives rise to the formation of early products, of which that derived from trimethylformene is lighter than water, that from ethyle heavier; and on heating those chloro-derivatives with water to 100° C. trimethylcarbinol is formed in the one case, and scarcely any action is observed in the other.

The number of isomeric butylenes according to the author is nine; that derived from trimethylcarbinol by the action upon it (its iodide) of alcoholic potassic hydrate, has the formula,

$$\Theta\left\{\begin{array}{l}(\Theta H_3)_2\\ \Theta H_2\end{array}\right.$$

and is converted into pseudopropylcarbinol (primary pseudobutylic alcohol), on oxidation with hypochlorous acid.—(*Ann. Chem. Pharm.* cxliv. 1.)

Synthesis of Alcohols.—E. Linemann. The synthesis of fatty alcohols from the lower members of the series by way of successive conversions of the alcohol (methylic) into cyanide, amide of next higher alcohol, and alcohol, is of little practical value on account of the great loss experienced in the last stage of the process. The conversion of the amide into alcohol by means of an excess of nitrous acid (Hofmann), whereby nitrite of alcohol is produced, is accompanied by a rapid evolution of nitrogen which carries off most of the volatile alcoholic nitrite. The author has discovered a process by which more than one-fourth of the amide is obtained as

[English Edition, Vol. XVII., No. 438, pages 200, 201; No. 435, pages 165, 166; No. 436, page 181.]

alcohol. It consists in boiling the nitrite of the amide with slightly acidulated water, whereby it splits up into nitrogen and alcohol, the alcohol being prevented from evaporation by the water present. The conversion of the amide into nitrite is effected by decomposing its chlorhydrate with argentic nitrite. A. Siersch by means of this method converted ethylic alcohol into isopropylic alcohol.—*Ibid.* cxliv. 129, 137.

Stannic Diethyl-dimethyle.—N. Morgunoff. Methyl-caproyl and acetyl-amyle, $\Theta_7H_{14}\Theta$, according to Popoff's experiments are identical, which fact proves the equality of the four carbon affinities. Morgunoff has from the same point of view examined the two stannic diethyl-dimethyle

$$Sn\left\{\begin{array}{l}\Theta H_3)_2\\ \Theta_2H_5)_2\end{array}\right.$$

as obtained either by acting upon standiethylic diiodide with zincic methide or upon standiethylic diiodide with zincic ethide, and he has found that both methods lead to the same result, and that therefore the four affinities of the tetratomic tin, like those of carbon, are of equal value.—*Ibid.* cxliv., 157.

NOTICES OF BOOKS.

Chemical Notes for the Lecture-room On Heat. By Thomas Wood, Ph.D., F.C.S. London: Longmans & Co.

It is a pleasure for us to record that this text-book has met with the success that we predicted for it. As a consequence a second edition has been published in a comparatively short space of time, and Dr. Wood has by careful revision considerably improved it. As we could never attach any very clear or definite meaning to the word "*cram*," as applied to tuition, we forbear to discuss in what way such an expression could attach itself to a work like that under our notice. It is, however, within the experience of every observer that cramming, in teaching, not unfrequently is used as synonymous with method, conciseness, and want of verbiage, by persons who we presume have never followed such a highly reprehensible course.

An expression which *is* definite may convey clearly the avowed object of this book, which is one that we may boldly say is a legitimate—even more, a desirable one—and that is *drill.* Many minds require the same fact to be placed before them over and over again in the same light and in the same words; but this is not equivalent to saying that such a *drilling* is applicable to leading and superior minds in which originality is to be hoped for. Dr. Wood certainly can with justice claim to have carried out thoroughly a plan that he has clearly sketched out for himself, and this we take to be no small merit in a writer of science. The majority of our readers will, we think, acknowledge the justice and truth of what Dr. Wood asserts in his preface.

"Many years' experience in teaching has convinced me that the average boy can only be provided with a very limited amount of producible information on any subject for an examination. A small book, therefore, with which he may become so familiar as to be able to refer to it with ease and rapidity, and which he can almost get by heart, is the thing required, and the present edition is offered as such."

In the next edition we would suggest that the author should properly punctuate the title of his book. Many who have not had the advantage of Dr. Wood's explanation might be puzzled to know the meaning of a "Lecture-room On Heat."

Scientific Blue Books. No. 1, *Abridgments of Specifications of Patents.*

It may be urged with justice, we are afraid, that scientific men in general, with the exception perhaps of authors, are totally ignorant of the fact that there exist scientific blue books, the value of which is very great, as by consulting such we invariably get evidence the accuracy of which no one can fairly dispute. Thousands of valuable scientific blue books are destroyed as waste-paper, owing mainly to the want of appreciation by the scientific public.

The original books of specifications of patents are far too costly and cumbersome for any but a large public library; thus the specification of patents filed during the operation of the Patent Law Amendment Act, from October 1, 1852, to June 30, 1866, are comprised in 43,955 blue books, or 1,428 thick volumes imperial octavo, to be obtained at a price of £1290.

The very first idea that is impressed upon the mind by this really impressive, not to say oppressive, fact, is that some of all this mass of matter must be of value; and if this value exists, the papers would be well worth wading through once and for ever by competent authorities, with the view of sifting the grain from the chaff in the first place, and in the second of converting that grain into material that may be easily digested. The second of these processes is continually being carried on by authors of technological works in every department of science; but the short digest of cumbersome works is not so well adapted to the needs of these, as to the students of such works who may desire to have authentic records of any technological process which may in turn serve as a reference to a still more detailed description if such be necessary. This work is now being conscientiously and thoroughly done in a systematic manner, and we are only discharging an obvious duty in doing the utmost in our power to prevent a premature close to a good project.

The abridgments of specifications of patented inventions carefully classified are miniature blue books, of duodecimo size; each affords at once a chronological, alphabetical, subject-matter, and reference index to each class, the work being done by well qualified compilers; omissions that are wholly unavoidable from the mass of the material will be supplied in second editions. In addition we find an introduction to each volume, which further gives a short digest of the various discoveries made from time to time in each branch. The prices of these works are extremely moderate, and a copy of the more important ones should be possessed by all practical chemists who devote themselves to the various branches of technology At present twenty-nine classes have been published, and among those of more immediate interest are those of "Preservation of Food," "Manufacture of Iron and Steel," "Bleaching, Dyeing, and Printing," "Electricity and Magnetism; their Generation and Applications," "Production and Applications of Gas," "Metals and Alloys," "Photography," "Plating and Coating of Metals," "Oils, Animals, Vegetable, and Mineral." The following we learn, with numerous others, are in course of preparation: "Preparation and Combustion of Fuel," "Steam Engines," "Stone, Marble, and Cements," "Acids, Alkalies, Oxides, and Salts."

If due appreciation attend these efforts, doubtless others will appear in course of time. Valuable as these abridgments are to all scientific men, they will prove to be invaluable to those who desire to know what processes really are patented and what are not. We would urge upon the authorities, however, that as these volumes bear evidence of great industry, considerable powers of accuracy, and require the rather rare quality of condensation with judgment, and without important omissions on the part of the compiler, it would only be a matter of justice to give the credit due to the author, in every case, by appending his name as such. The practice of working by deputy is only to be encouraged when the said deputy is exposed to a just criticism of his own share in the work, which criticism should be limited to such personal work. By a subdivision of labour, again, it so frequently happens that any responsibility is easily lost sight of, and inaccuracy is a frequent result.

We are told by Mr. Woodcroft that the most recent chemical names of substances are placed in italics after the names that have been obtained from the respective specifications;

"this addition is rendered necessary by the universal adoption of the new chemical nomenclature."

We hope that this addition will not at present be made to the substance of the specifications themselves: even the most ardent radical in chemistry would not care to have a string of synonyms after every mention of such a substance as, say, calomel, which, if burdened with aliases, after the manner of legal definitions, would, we are afraid, present a very criminal appearance indeed.

CORRESPONDENCE.

Preservation of Meat.

To the Editor of the Chemical News.

Sir,—Having worked in conjunction with Prof. Gamgee at the development of the meat preserving process, I can answer for him the inquiries of your correspondent. The sheep sent over from England were preserved in substantially the same way as the meat mentioned in the Chemical News, vol. xv., p. 135 (*Eng. Ed.*), viz., by treatment with carbonic oxide and sulphurous acid. The carcases were whole, and merely packed in wooden boxes, containing soft material to prevent bruising. The details of the process are of course given in the English, United States, and other patent specifications, to which I must refer all seeking information.—I am, &c.,

Walter Noel Hartley.

March 30, 1868.

Royal School of Mines.

To the Editor of the Chemical News.

Sir,—In his last letter, "A. L. E." proposes, *en passant*, a change in the curriculum of this School, to which, it appears to me, much greater prominence is due. I refer to the passage in which he mentions the desirability of increasing the present staff of professors by two, viz., on botany and mathematics.

When the late Professor Forbes was alive, the lectures of general natural history were rightly named, inasmuch as they included both zoology and botany; but Professor Huxley, his successor, does not include botany in his course. Now, I suppose none would blame the Council for preferring such a man as Professor Huxley to another of inferior talents who would include both subjects; and had the Council supplemented their election of Professor Huxley to the chair of zoology by another election to the botanical chair, all would have been satisfied. As it is, however, the whole of the botany taught in this School is limited to the lectures on palæontology, where it is, *ex necessitate*, only briefly treated.

Such a step would be a boon, not only to the students of this school (for then I should have little excuse for thus trespassing on your columns), but to the whole mass of London students. For as the present course on zoology, yearly delivered by Professor Huxley, is unequalled, I think I may with safety say, in the British Islands, so, I suppose, would it be the case with the botanical lectures. The difficulty of obtaining, at the present time, any course of lectures on this subject, which goes into the science at all deeply, is too well known to those who have tried to find such lectures, to need comment from me.

I find that the length of my letter forbids me entering into detail on the subject of the mathematical course; I therefore leave it, and with less regret, since the relations of mathematics to physics and mechanics are so very apparent.

I will not waste your space by apologies for trespassing thereon; and thanking "A. L. E." for raising the discussion, and yourselves for so kindly opening your columns thereto,—I am, &c.,

An Exhibitioner, R.S.M.

The Permanganate Water-Test.

To the Editor of the Chemical News.

Sir,—Will you permit me to make a few observations on the paper which appeared in your number of the 27th of March (*Am. Repr.*, May, '68, page 221), on the nature and examination of the organic matter in potable waters? It is from the sanitary point of view that I propose to consider the subject, because, as was remarked by the author of that paper, it is the fitness or unfitness of water for drinking purposes, and the quality and effects on the health of the organic matter contained in water, rather than its quantity, which are the points that give importance to the subject.

Some seventeen years ago, Professor Forchhammer, of Copenhagen, proposed to estimate the quantity of soluble organic matter in water by permanganate of potash. After a considerable lapse of time his proposal was extensively adopted and relied upon by some chemists, as a means of volumetrically determining the amount of organic impurities present in water. Subsequent observations have, however, demonstrated that for the purpose of estimating the quantity of organic matter contained in water, permanganate of potash cannot be relied upon.

In the meantime (some twelve years ago) Mr. Condy, of Battersea, having discovered the disinfecting properties of the alkaline permanganates, proposed to apply his solution of those salts (Condy's Fluid) to the determination of the quality of the soluble organic impurities of potable waters, and succeeded in satisfactorily demonstrating that his method was not only a quick and ready, but likewise a reliable sanitary test for soluble organic matter in water. Having been adopted in several of the public services, and used extensively among medical men during a period of many years, his process must be considered firmly established. I could myself detail many striking proofs of the sanitary value of this test which have come under my notice in the course of my experience as a lecturer on hygienic subjects at various institutions throughout Great Britain, but will confine myself to a recent instance. Having been supplied, through the kindness of Dr. Gimson, with specimens of water from all the wells of Terling, I easily detected the infected waters by this test alone, and on handing him my results they were found to be exactly borne out by his experience of the course taken by the serious epidemic of typhoid fever which has been prevalent there during the last three or four months.

While, therefore, permanganate of potash has been found comparatively worthless for estimating the quantity of organic matter in water, its value as a quick and ready sanitary test for the quality of the organic impurities of potable waters has been placed beyond doubt.

It would, consequently, seem to me that the way in which the question of the utility of the permanganate test has been dismissed, in the paper under consideration, is calculated to propagate error and to discredit one most useful and reliable application of it, for showing inadequate reason that another and totally different application has proved to be faulty and comparatively worthless.

The study of the effects of permanganate on organic matter shows that, as a rule, it acts with great rapidity on such matter when in a putrescent or offensive state, but slowly on sound or harmless organic substances. Thus its complete action upon water mixed with pure organic matter would be a matter of days; whereas, on the other hand, if the sample be allowed to stand till decomposition sets in, the permanganate would act with great rapidity on the decomposed portion. Dr. Angus Smith, in a pamphlet on this subject privately printed and circulated (but not published, so far as I am aware), says: "The organic matter which decomposes the chameleon in a minute or two, must be carefully noted; but generally there is a greater quantity which decomposes very slowly: the result obtained for the latter is, I believe, of less value. Generally considerable permanency is obtained in ten or fifteen minutes; then the slow decomposition begins of quite another quantity of organic matter, requiring hours or even days. The amount decomposed instantly is a true measure of the putridity." It is a most important matter, therefore, in considering the action of permanganate on water, to keep clearly in view these several actions which actually give the real practical value to that substance as a test for, and destroyer of, dangerous organic matter. It affords the most rapid and ready mode of detecting the offensive and dangerous substances, while it leaves the inoffensive and harmless matters comparatively untouched. It is evident that when a speedy test for the existence in water of impurities to which the origin of disease in a household may be due, a reagent is not wanted which would expend its chemical force on such harmless matters as sugar, gum, starch, and the like, but one which can seek out and reveal offensive organic substances, which, in the words of the paper in question, are often of "such deleterious nature, that our ideas of the most virulent matter fall short of the horrible results that these invisible poisons can accomplish."

By turning to the CHEMICAL NEWS of the 7th of February last (*Am. Repr.*, April, 1868, page 184), it will be seen that, at a meeting of the Manchester Literary and Philosophical Society, Dr. Angus Smith confirmed his above-stated views, by declaring that "the condition of organic matter in water can be estimated for sanitary purposes sufficiently by permanganate of potash," which is precisely the proposition first advanced by Mr. Condy ten years ago.

Some little time since Professor Attfield published through the medium of the *Times*, a somewhat ingenious extemporary method of recognising pollution in water, by means of the sense of smell. But the time required by this method is sometimes so great, and the nose, especially among persons who have not been used to discriminate faint odours by the olfactory organ, is so inferior to the eye, that were it even more accurate and reliable than it is, Professor Attfield's test, though well worth knowing as an excellent make-shift in certain exceptional cases, may be set aside as having nothing to recommend it for ordinary occasions.—I am, &c.,

JOHN MUTER,
Author of "The Alkaline Permanganates and their Medicinal Uses."

Richmond Terrace, March 30th, 1868.

Detection of Adulterated Flour.

To the Editor of the CHEMICAL NEWS.

SIR,—Your querist, "Q," asks for a method of detecting the adulteration of wheaten flour with the flour of the seeds of the leguminosæ, *i.e.*, beans, lentils, peas, &c., other than the detection of the difference of the starch globules of the latter meals by means of the microscope. The wheaten flour suspected to be thus adulterated is made into a paste with water, the paste is placed in a clean linen cloth and is kneaded under a constant stream of fresh water, until the water runs off quite limpid; in other words, all the starch and matters soluble in water are washed out, and the gluten is retained in the cloth. The following points deserve to be noticed:—

(*a*) Whether the paste does not emit a peculiar smell not met with in paste made with unadulterated flour.

(*b*) Whether it exhibits a peculiar fatty appearance.

(*c*) Whether the water does not exhibit a soapy appearance.

(*d*) Whether the gluten which remains on the cloth exhibits the proper degree of toughness and elasticity, so characteristic of the gluten from pure, sound wheaten flour.

The water which has served for washing, is collected, and after having been well and vigorously stirred up, is divided into two equal portions. One of these is left standing exposed to a temperature of from 70° to 86° F., in order to try whether foul fermentation does not set in; this does *not* take place at all, in case the flour under examination, and treated as described, were sound wheaten flour, since in that case only lactic acid is formed under the conditions just alluded to; the other portion of the water is first diluted with some distilled water, rendered alkaline by the addition of some liquid ammonia. The fluid is then left quietly standing, until all the starch is settled down, is next filtered, is then submitted to evaporation on a water-bath, until a kind of pellicle, or skin, is observed to be formed on its surface; it is then cooled down, filtered, in order to separate coagulated albumen, while to the clear filtrate next acetic acid, in slight excess, is added. If the addition of this acid causes a precipitate, it is possible that such might be due to the presence of legumin, owing to the adulteration of the wheaten flour by means of the ground-up seeds of leguminosæ; but since the precipitate might be due to immixtures in the wheaten flour of buck-wheat meal, colza-cake meal, indian-corn meal, or even accidental presence of chloride of sodium, it is requisite to collect the precipitate on a filter, to wash it with warm distilled water, and to submit it to the following tests:—

(*a*) To observe whether it is colourless, and devoid of smell and taste.

(*b*) Whether on drying it becomes horny, hard, and translucent.

(*c*) Whether it is tinged blue by iodine.

(*d*) Whether it is, or is *not*, soluble in hot or cold water.

(*e*) Also, insoluble in alcohol (pure alcohol, not methylated spirit).

(*f*) Soluble in a weak solution of caustic potassa in ammonia, and precipitable therefrom by means of hydrochloric, nitric, and acetic acids, all of which reactions refer to legumin.

The precipitate or sediment of the washings of the flour under examination is next divided into two portions; the smaller of the two portions is again divided into two parts, and to one of these is added a solution of caustic potassa, containing 10 per cent. of solid alkali, while to the other is added some dilute hydrochloric acid; in both instances the starch is by these means dissolved, and if some of the fluid is placed, under proper conditions, under a microscope with a magnifying power of 300, there will, in case any meal of peas, beans, lentils had been present, be seen the remnants of the broken-up cellular tissue peculiar to these seeds, and exhibiting a peculiar net-like texture. The larger portion of the sediment may be carefully washed out with water, so as to obtain six different wash-waters, all depositing sediments of starch, the last, or sixth of which will contain the peculiar starch of the seeds just alluded to. Fresenius has called attention to a peculiar difference in the ash of the wheaten meal, or flour suspected to be adulterated with leguminosæ seeds. Such ash is—1st, deliquescent; 2d, its aqueous solution tinges turmeric paper brown; 3d, there will appear, on addition of nitrate of silver solution to such ash, a precipitate of chloride of silver, which, on exposure to daylight, becomes discoloured. Pure and sound wheaten flour does not contain any chlorides at all; and though the solution of its ash yields a precipitate with nitrate of silver, that precipitate remains unchanged by exposure to daylight. Reddened litmus paper is rendered blue when brought into contact with the solution of the ash of pure, sound wheaten flour, but turmeric paper remains unchanged. Louyet further observes that the quantity, also, of the ash may serve as a criterion; dried old wheaten flour only yields 1 per cent. of ash, rye meal about the same; but the meal of peas and beans yields never less than 3 per cent.; the addition therefore of even 10 per cent., by weight, of meal of peas or beans to wheaten flour will sensibly alter the quantity of ash the latter ought to leave behind on incineration executed with proper care.

It may therefore be assumed that, if wheaten flour yields more than one, but below two per cent. of ash, it is adulterated with meal of peas, &c., since an intentional adulteration with metallic or earthy matter will almost always be so carried out as to bring the ash far above 1 per cent.

Donny states that if wheaten flour is adulterated with the meal of white beans (haricot beans), or with that of lentils, provided, however, the quantity thereof be not below 5 per cent., and such meal is so placed in a deep porcelain basin as to cover with a slight thin layer the sides of such vessel, while the bottom is left uncovered, that then when first a few drops of strong nitric acid are allowed to evaporate from the bottom, and immediately after some ammonia, reddish specks will make their appearance in the flour along the sides of the vessel, which red specks indicate the presence of the meal of these two substances. A better test is to exhaust the suspected flour with pure warm alcohol, to evaporate the latter, to treat the alcoholic extract with ether, and to expose the residue thereof, while air has free access to the vapours first of nitric acid, next of ammonia, the residue will, if either the meal of white beans or lentils had been present, exhibit a beautiful amaranth pink colouration.—I am, &c.,

Dr. A. Adriani.

Water-Tests.

To the Editor of the Chemical News.

Sir,—Your correspondent, Mr. J. Muter, gives me the credit of having published, through the medium of the *Times*, an extemporary method of recognising pollution in water by the sense of smell. Mr. Muter has misread the letter to which he alludes. I merely reminded the country householder that he usually contented himself with a glance and a sniff at his water-bottle, and suggested that eyes and nose would be better detectives if he previously well shook the water or even placed it in a warm place for a few hours. Mr. Muter's letter to you is similar to one he sent the previous week to the *Medical Times and Gazette;* the sentences in which I am mentioned are word for word the same. In answer to that letter a kind friend has replied, this week, as follows.—I am, &c.,

John Attfield.

Testing Water for Organic Impurities.

To the Editor of the Chemical News.

Sir,—In connection with remarks on "country wells," Professor Attfield simply stated, in his letter to the *Times*, that "polluted water does not generally betray its condition till possessed of a strong odour; earlier intimation may, however, be obtained by the following tests:—Half fill a common water-bottle, cover its mouth with the hand, violently shake for a minute, and quickly apply the nose. If nothing unpleasant is detected, tightly cork the bottle, set it aside in a warm place at about the temperature of one's body for a couple or three days, and repeat the shaking, &c. Water of very bad quality may thus be recognised without the trouble and expense of analysis." Of course householders would get still earlier intimation, or else the comforting assurance that the water contained no organic impurity, by seeking professional assistance; but such a statement by an analyst in a leading newspaper would have been scarcely ethical. I have found Professor Attfield's hints of very great use, and am convinced that few persons besides your correspondent, Mr. J. B. Muter, could possibly have received from them the impression that any water free from odour is fit to drink.—I am, &c.,

Sanitas.

Royal School of Mines.

To the Editor of the Chemical News.

Sir,—Will you kindly allow me to draw attention to one question which appears to have escaped notice, viz.—What is the meaning of the designation Royal School of Mines?

The term R. Mining School, or School for Miners, I could understand; but this one I cannot. I know what a school of boys is, and I have heard of a school of whales, but what is a School of Mines? It is true that there exists an Ecole des Mines, but I do not know why our corresponding institution should bear a name which is a literal translation of the above title.

Now that the School is just beginning to be known, it seems almost a pity to propose any alteration, still I really believe that it would be preferable to call it the "College of Science," as suggested by "Delta," than to let it still retain its present inappropriate and absurd name, more especially as only a small fraction of the students ever have anything to do with mines or mining in after life.

My apology for again encroaching upon your valuable space must be the deep interest which I take in the Institution and everything connected with it.—I am, &c.,

A. L. E.

Permanganate of Potash and Organic Matter in Water.

To the Editor of the Chemical News.

Sir,—I have just read a letter in your number of the week before last (*American Reprint*, June, 1868, page 288), in which, after all that has been said to the contrary, the abovenamed substance is still spoken of in terms of approval as affording a means of estimating organic matter in water. As my opinion on the point differs in all respects from that expressed by the writer, and finding, more especially, that he has left untouched a main element in the question (if it be one), I shall be obliged if you will permit me to say a few words thereupon in this week's number of the Chemical News.

Soon after this method was proposed by the Danish professor, I made a series of experiments with it on a variety of naturally impure waters, but on finding the results so utterly irreconcilable with those arrived at by careful incineration of other portions of the same samples, I gave it up; though I must add that I did not then arrive at the source of its leading fallacy.

After these experiments, happening to meet the late Professor Clark, of Aberdeen, who took considerable interest in most matters connected with water analysis, and mentioning my results to him, I soon found that he also had arrived at a similar opinion as to the fallacious character of this mode of estimation.

Though I have frequently had occasion to express my unwillingness to be bound in any way by results obtained by the method, yet continuing to hear of its being much in use, I began to think that, with all my care, I had possibly mistaken some important points; and hence I was led to make another series of experiments a few years after.

Meanwhile, I am bound to say that I never found permanganate in much favour amongst the hard working "laboratory men," but chiefly amongst officers of health, public lecturers, and others, too glad to find this ready though empirical mode of operation to be easily induced to give it up, or at any time question its accuracy too closely.

On several occasions where I have found results put forward in evidence as to organic matter, greatly disagreeing with those of others, it usually came out, on cross-examination, that they were obtained by permanganate; but on finding its accuracy doubted, I have heard some of the firmer men add—that they had estimated by incineration as well, and that the two processes substantially agreed—a result, I may add, which I have never been able to realise. However, discrepant statements of this class have greatly tended to shake the faith of legal functionaries, as well as the public, in chemists' conclusions.

Some years ago, and since the introduction of the permanganate mode, a chemist of some public standing, but who had a good deal of public lecturing to attend to, gave evidence to a government committee, on which I was engaged, as to the quality of a certain water. He stated that it contained seven grains of organic matter in a gallon, whilst I on the other hand, had been unable to find more than one grain. Both of our samples were taken at the same place, and on the same day, all of which I had to state subsequently in my evidence to the same committee. This led to the water being analysed by other chemists, none of whom I may mention found the seven grains of organic matter at first stated. Though I strongly believed that the doubtful results in this instance had been arrived at by the use of permanganate, yet from the absence of counsel on government committees I was unable to have the question put. This and some other circumstances of a like nature, induced me to enter on the second series of experiments on permanganate to which I have already referred.

On this, the second occasion, the samples of water were synthetised for the experiments, that is to say, given portions of the usual earthy salts were dissolved in doubly distilled water, to which were added measured portions of various impure as well as pure organic decoctions, so as to accurately represent varieties of impure water. In some of the experiments, the organic matter was represented by soakings of putrescent manure, but in all considerable care was taken that the precise quality of the water used in each should be accurately known, irrespectively of either of the modes of analytical examination employed.

At this moment I have neither time nor inclination to hunt up, or put into shape, my notes of those experiments, but which will, I am sure, be deemed of less consequence when I have cited one example of the fallacious action of this very beautiful salt of potash, and which it will be seen must necessarily apply to most if not all waters.

I need hardly say, in this journal, that all potable water in this and in most countries, contains some iron, and not unfrequently traces of manganese—though the latter, being so small, is often estimated as iron. At the same time, with the exception of so-called chalybeate waters, either of these metals are usually found so very sparingly as to render their presence inappreciable to all but the analyst. Notwithstanding which, a third or a quarter of a grain of iron in a gallon is far from an unusual quantity; whilst in chalk waters, which are the freest from iron, may be found a tenth of a grain, though this quantity is often set down as "a trace." It is also to be remembered that iron in water is usually estimated as "oxide," but in which state, I need not say, it never exists in the water, but usually as a colourless super-protocarbonate; that is to say, the iron found in the residuum of our evaporation in the state of oxide, after incineration, existed in the water as white proto-carbonate, and which is held in solution as chalk is, by an extra atom of carbonic acid. Grains of the white protocarbonate pervade sand and soils to a much greater extent than is supposed, and, while in this state, is readily taken up by the water.

Permit me now to recapitulate the action of the permanganate, which I need hardly repeat here is briefly this:—It acts in water by readily imparting a portion of its oxygen to bodies requiring the same. As soon, however, as this oxygen is given out, the permanganate loses its splendid colour, and hence, obviously, arose its employment as a test. Now, as organic matter when dissolved in water has an affinity, or, let us say, an appetite for oxygen, so that when permanganate is added to a water in which this impurity exists, this manganic salt loses its colour, and the quantity of it thus decolourised is said to stamp the quantity of the organic matter present in the water. In a word, the presence of the one is said to be in a direct ratio with the loss sustained by the other.

Now, this might be all very well if it were true that impure organic matter was the only class of substance incident to water, and to which the permanganate is disposed to give out its oxygen, and thus deprive itself of its beauty. Such, however, is very far from being so, for, as we shall see, the salt in question is far more disposed to give out its oxygen to a really salubrious body which is as much incident to water as organic matter. Indeed, much more so if we take organic matter of the worst class.

As I have already stated—and it is only repeating what all admit—every potable water may be said to contain some iron, and this usually as a proto-salt. Now, as a tenth of a grain of a proto-salt of iron or manganese, dissolved in water, will rapidly decolourise as much permanganate as three or four grains of even putrescible organic matter, I leave it to your correspondent and those who so tenaciously hold to this mode of estimation, to say whether the decolourisation of permanganate which takes place in a given sample of water, is due to noxious organic matter or to really salubrious iron, either of which it may obviously contain? But surely all this must be already familiar to the readers of your journal.

However, should the author of the letter in question (Mr. Muter) entertain any doubt on the matter, he may easily improvise an experiment.

Let him take a grain of proto-sulphate of iron—being the easiest come-atable proto-salt—and dissolve it in a gallon of any water, hard or soft, though distilled will be the most reliable, and then let him add his permanganate. I venture to say that if he were operating on water with the quality of which he was unacquainted, he would set this distilled water down as unfit for human use, as being greatly contaminated with putrescible organic matter. I may remark that a grain of protosulphate of iron with its seven atoms of water, and its acid, will very nearly represent the tenth of a grain of oxide, so often found in most waters. In some of my experiments I used, for the sake of accuracy, mineral proto carbonate dissolved in carbonic acid under pressure, but I find any proto-salt is affected similarly by the permanganate. But above all, I find that this very salt is used by the volumetric men, as a mode of estimating iron; but how, in the face of this, it ever could have been used for separating another body in which the first was so likely to be present, and for which it has an equal affinity, is I confess a puzzle to me, unless, indeed, there is some mode of masking the one, whilst the other performs the required duty.

Let me add in conclusion, that this objection to the use of permanganate is far from being the only one, though it forms a most striking one.—I am, &c.,

THOS. SPENCER.

32, Euston Square, London, April 15th, 1868.

P. S.—I omitted to state that the water said to contain seven grains of organic matter came from highly ferruginous gravel, and contained a considerable amount of iron. It is probable, therefore, that if tested by permanganate, this iron would figure in the analysis as the organic matter in question.

Royal School of Mines.

To the Editor of the CHEMICAL NEWS.

SIR,—We hear much about the School of Mines in Jermyn Street. Can any one give me a comparative sketch of the advantages and cost of entrance of the Ecole des Mines in Paris? I have never seen it, but I hear rumour of supplies of oxygen, of economy for students, and of the greatest courtesy to strangers, of which I would fain learn more.

A great want in London for students and experimentalists who have advanced beyond a certain point, is an upper class-room, so to speak, where they can work at peace, uninterrupted by very young students, and be supplied with reliable reagents, so avoiding the worry and loss of time involved in having to test everything used, and possibly make it for themselves after all, and a comfortable room for weighing both on the balance and in their mind.

I hear reports of a new laboratory in King's College, and of one at the Royal College of Chemistry. The class of men interested in investigations, but not prepared to set up their own laboratories, must be large, judging from the number I have met. Surely if their requirements were met, a large class would be formed who would make such a laboratory by no means a losing concern, especially with attendants who would do some of the "dog-work," and an assistant whose time was entirely devoted to the interests of the class.—I am, &c.,

AN ASPIRANT.

Putrescible Matter in Water; Sanitary Water Tests.

To the Editor of the CHEMICAL NEWS.

SIR,—I have on my premises two supplies of water, namely, one from the Southwark Waterworks, the other from a surface well. The pipe water, when shaken in a wide-mouthed bottle, partially filled, has no appreciably unpleasant smell. When the bottle containing it is set aside in a warm place for three days and then shaken, the contents give out a very faint offensive odour, which is proof that organic matter held in solution by it has become putrescent. When the freshly drawn pipe water is slightly tinged with a permanganate solution, it gradually loses the colour imparted by that substance, which generally is an indication that it contains organic matter in a non-putrescent condition. Being treated with a further quantity of permanganate and allowed to stand for twenty-four hours, this water, when then set aside in a warm place for three days, is found to be no longer capable of undergoing putrescence. On being shaken up in the bottle it now possesses not the slightest trace of offensive odour.

The well water, when shaken in a partially filled, wide-mouthed bottle, is also free from any appreciably offensive smell. When the bottle containing it is set aside in a warm place for three days, the contents give out a strong odour of putrescence. In this condition it decolourises permanganate rapidly, and in considerable quantities.

The freshly drawn well water, on being treated with permanganate, decomposes that substance with rapidity, showing that it may be considered to be largely polluted with organic matter of a putrescible nature. When permanganate is added until permanence of colour is obtained, and the water at the end of four-and-twenty hours is allowed to stand sufficiently long to cause the disappearance of the colour produced by the testing solution, it is found to be no longer susceptible of undergoing putrescence. On being now shaken up in the bottle the well-water possesses quite as little trace of offensive odour as the pipe-water after treatment by permanganate. After treatment with the testing solution, neither the pipe-water nor the well-water is capable of decolourising permanganate except by prolonged contact: the less stable organic matters contained in them having been burnt up. These experiments show that both of the above methods of testing exhibit the presence in water of putrescible organic matter—the one, after a considerable lapse of time, to the nose, the other, immediately, to the eye. If it were necessary to choose between the two, several considerations, such as those alluded to by Mr. Muter, would seem to incline the choice in favour of the latter as a popular water test. But so far from this being necessary, these two methods will be found in practice to supplement each other most usefully, and when employed together to furnish results which are sufficiently exact for most sanitary purposes, and as regards the detection of putrescible matter perhaps more to be relied on than some of the refined analytical processes of modern chemistry.—I am, &c.,

H. B. Condy.

Battersea, April 12, 1868.

Science Teaching.

To the Editor of the Chemical News.

Sir,—Some months ago you published the conclusion to lectures on chemistry delivered at Eton College, and again in No. 435 (*American Reprint*, May, 1868, pages 228–9) you publish the conclusion to lectures on "Heat" delivered in the same place.

If we may judge of the lectures themselves from these extracts, I would say that such lectures are not adapted to attract boys to science. I think that most of your readers will agree with me here.

At the present time, when science is so much talked about as an item of school education, I feel constrained to enter my humble protest against a system of teaching it in high places, which I consider to be calculated to bring it into disfavour.

The current of modern thought tends to the popularisation of science, and the teacher who envelopes science in too learned language is looking back towards the Sodom of the middle ages.—I am, &c.,

D.

Dr. Guthrie's Graphic Formulæ.

To the Editor of the Chemical News.

Sir,—I conceive the following objections may be urged against the adoption of Professor Guthrie's system of Graphic Formulæ:—

The dot (for hydrogen) is already engaged in Berzelius' scheme to represent oxygen. Guthrie's symbol for oxygen, a horizontal dash, might be easily mistaken for the algebraical sign of minus; the cross, bromine, for the sign of multiplication; the dash and two dots, representing water, are exactly like the sign of division; and the commas for fluorine are likely to be confounded with the double dash now used to indicate the diatomicity of an element. The distinction made between nitrogen and iodine is only a variation in the size of the triangle; and the modified form of the larger triangle (with lines curved inwards instead of straight), intended to represent phosphorus, would often be confounded with nitrogen in roughly executed manuscript; and the same remark applies to the proposed distinction between sulphur and selenium.

Again, the scheme is incomplete, for we ought at least to have been supplied with symbols to represent arsenic, antimony, boron, and silicon, even if the author had not yet decided upon the proper course to be taken with the remaining metallic elements. Much might also be said on the score of want of originality, for the early chemical and alchemic works teem with graphic illustrations and codes of symbols, based upon the same general principle; thus in "Nicholson's Dictionary of Chemistry," dated 1795, there are at the end of the second volume two large folio plates showing "the characters to be made use of in chemistry," as proposed by Haffenfratz and Adet; and "Table vii.—The chemical signs as they occur in the writings of Bergman." Curiously enough, the first of these authorities adopted the horizontal dash as the symbol for oxygen; they used triangles, circles, and squares in every conceivable manner, and actually provided a series of "characters to express such new and simple substances as may hereafter be discovered."—I am, &c.,

F. C. S.

April 18, 1868.

Ozone.

To the Editor of the Chemical News.

Sir,—The following are the more salient points in the development of ozone during the first three months of the present year:—

January 1st to morn. of 12th. Small amounts, except on the nights of the 3rd and 6th, when there was a tendency to an increasing development. Aft. of 12th—22nd, period with large amounts during the nights (9.30 p.m.—9.30 a.m.) and small amounts during the days (9.30 a.m.—9.30 p.m.). The maximum was found on the night of 18th, and large amounts on the nights of 13th, 14th, 17th, and 19th; 23rd—29th small amounts, except on aft. of 24th when a large amount was present, and on aft. of 27th when there was a tendency to an increasing development. No ozone on 23rd, aft. of 26th, and morn. of 27th. 30th—Feb. 3rd, large amounts. 3rd—16th, a variable period: the periods of increased and decreased development were short and unsettled. Large amounts on 5th and 7th, and on the nights of the 8th and 15th. No ozone aft. of 8th, morn. of 9th, throughout the 12th, morn. of 13th, aft. of 15th, and morn. of 16th. 17th—23rd, large amounts, especially during the nights.

23rd—28th, small amounts. No ozone on aft. of 23rd to 28th—March 17th, large amounts, except on aft. of 2nd, when no ozone was found, and throughout the 3rd when about the average quantity was present. From the 4th—14th there was a very well marked and settled period of ozone. 18th—morn. of 22nd, small amounts; aft. of 22nd—morn. of 23rd, large amounts; aft. of 23rd—25th, small amounts. No ozone aft. of 24th and morn. of 25th. Night of 26th; large amounts, aft of 26th—31st, a variable period with no ozone on aft. of 28th, throughout the 29th and on the morn. of 30th, and considerable amounts on afts. of 30th and 31st. Speaking generally, from the 18th—31st the development of ozone was variable.

The ozone period from the 4th—14th of March was very well marked: it occurred towards the close of a long period of equatorial wind, which shortly afterwards passed into the polar current.

During the whole of the three months the amount of ozone developed during the night (9.30 p.m.—9.30 a.m.) very considerably exceeded that developed during the day (9.30 a.m.—9 30 p.m.). This excess in the amount developed by night over that developed by day was greatest in January and least in March.—I am, &c.

R. C. C. Lippincott.

Eastbourne, April 4, 1868.

MISCELLANEOUS.

Patents by Scientific Men.—" Wollaston was fond of amassing money. There have not, indeed, been wanting accusations to the effect that if he had sought less after wealth, he would have done more for science. How far these charges are true, we have no means of judging, as it does not appear from the published accounts in what exact way he made his money. That it was chiefly by the platina process is certain; but whether he engaged in the manufacture himself, or only superintended it, we do not know. On this point we would only remark, that there is something, to say the least of it, very partial and unfair in the way in which obloquy is cast upon men of science, if they appropriate to themselves some of the wealth which their discoveries procure for others. If a successful naval or military hero is lavishly pensioned out of the public purse, no one complains. It is not thought strange that a great painter or sculptor, while he justly declares his productions are worth untold gold, should, nevertheless, demand a modicum of coin from his admirers. Neither is the poet or musician blamed who sells his works to the highest bidder. But if a chemist, for whom there are few pensions and no peerages, thinks to help out a scanty or insufficient income, by manufacturing gunpowder, like Davy, or magnesia, like Henry, or malleable platina, like Wollaston, or guano, like Liebig, the detractors assail him at once. He has lowered the dignity of his science, and, it would seem, should starve rather than degrade his vocation. That vocation, so far at least as the practical fruits of his own labours are concerned, is to be a kind of jackal to start game which others are to follow—a beagle to hunt down prey which others may devour. Surely there is but scanty justice here, and some forgetfulness of a sacred text:—'Thou shalt not muzzle the mouth of the ox that treadeth out the corn, &c.' "—*From the British Quarterly Review, No.* vii. (*August*, 1846), *Article* 3, *p.* 104.

Sir David Brewster's Last Words.—Sir J. Simpson says :—" It seems to me that I carry almost a mission from him to us—from the dead to the living; for when I last visited him at Allerly, when he was within a few hours of death, when he was already pulseless, his mind was perfectly entire, and perfectly composed; and on asking him, among other matters, if he wished any particular scientific friend to take charge of his remaining scientific papers and notes, he answered me, 'No; I have done what every scientific man should do—viz., published almost all my observations, of any value, just as they have occurred."

A Model Scientific Writer.—Professor Fraser says with regard to Sir David Brewster's great precision, energy, and determination of thought—that during the seven years that he (Professor Fraser) was editor of the *North British Review*, Sir David Brewster contributed an article to every number; and that he did far more—that he stated the day when his first slip of paper would come, and the day when it would be finished. His manuscripts came as they were written—day after day, and sheet after sheet—and without the necessity of the revisal of those preceding. He thus worked with the precision and regularity of a mechanical rather than a mental machine.—*Scientific Review.*

M. Panizzi and Men of Science.—In a letter to the *Times*, defending a statement made in Parliament that Mr. Panizzi, principal librarian of the British Museum, "had not scrupled to express his contempt for men of science," Mr. W. H. Gregory, M.P., writes:—" Three short passages from Mr. Panizzi's evidence before the select committee on the British Museum in 1836 will prove the correctness of my expressions. In answer 4,929 Mr. Panizzi gives his opinion of scientific men in these words :—' Scientific men are jealous of their authority; they are dogmatical and narrow-minded, and as they think themselves infallible they would never consult an officer. I speak from what I have known of them.' '4,930. The scientific men would spoil the men of rank or drive them away from the Board. I speak seriously, and from experience. An officer would have no chance against a scientific man who should take a crotchet, and they are all crotchety.' '4,933. I never saw scientific men go right or view things as other people do. I think the trustees would be much better without them.' "

Chemical Nomenclature.—M. Dumas, the new secretary of the Académie des Sciences, observes—" If every one of us took the fancy of combining with his name that of his great-grandfather, of his grandfather, of his father, and his mother. a singular complication would be found in our registers of births. A lifetime would be passed in learning the names of the persons with whom we were acquainted in our own neighbourhood. As to knowing the names of the inhabitants of a town, that would be an utter impossibility. This is, however, what our *savants* who pursue organic chemistry have to accomplish, so that their language has now arrived at a point of barbarism that cannot be surpassed. Now, would it not be desirable, in all points of view, to adopt a generic word, and to group around such word the names of species in proportion as science extends her conquests? I am particularly interested in organic chemistry, but I declare that time is entirely wanting to me to peruse, while comprehending them, the various memoirs on the science which come under my notice. The complication and insupportable length of the names employed are the sole causes of this."—*Medical Times and Gazette*, March 21st.

Poisoning with Oxalic Acid.—The case of poisoning at Bristol, here recorded, is remarkable in many respects:—

1st. The patient took ¾ of an ounce avoirdupois.

2nd. She died ten minutes afterwards, or very shortly after.

3rd. She vomited almost all the poisoning material, as the coats of the stomach retained by absorption only 2 gr. of the oxalic acid.

4th. There was nothing to be found in the contents of the stomach, which were merely effused blood. The stomach was intensely red, inflamed in that short period.

5th. She tested the contents of her own stomach by having vomited into a bucket of water holding a great quantity of lime in solution. " The vomited matter was like milk," when seen on the floor; and when she vomited into the bucket "it appeared to turn the water into milk."

This did not come out in the evidence, as the girl vomited into the pail in which they were in the habit of washing the glasses and cups used in the bar, and of course the landlord did not want to damage his business by giving such evidence.

The floor was wooden, not of stone, and the oxalic acid was dissolved in hot water, highly charged with lime; and it acted as an instantaneous emetic, and came up almost as it was swallowed, a milky-looking fluid, capable of precipitating a large quantity of lime. At the inquest, the following evidence was taken:—

William James Pester deposed: The deceased, Sarah Salmon, was in my employ as barmaid and housekeeper for nearly twelve months. On Saturday evening she died. I saw her at 20 minutes to seven o'clock, and she then appeared to be quite well. Shortly afterwards she became very sick, and continued so until her death. I went out shortly after half-past six o'clock, and came back about seven o'clock, and I then found her very sick, but still sensible. I asked what was the matter with her, but she did not reply. She was assisted upstairs by the servant, and lay down on the bed. She died shortly afterwards, within ten minutes of her going upstairs.

I sent her up a glass of brandy and water. I sent for Mr. Fardon, and afterwards for Dr. Herapath. She seemed rather peculiar and excited during the whole of Saturday, especially in the afternoon. Her conduct was very different to her ordinary demeanour, and attracted my attention, she being generally very reserved in her manner. She scarcely spoke half a dozen words to me during the day, and then only when I asked her a question. She stood in the bar leaning against the counter during the afternoon with her arms folded. She would not wait upon the parlour customers, and she would not move out of the way when I wished to enter the bar until I asked her to do so. No one saw her take anything.

Emma Thomas stated: I am in the service of Mr. Pester as general servant. On Saturday, about seven o'clock in the evening, I noticed that she was very sick in the bar. I helped her upstairs, and she lay on the bed, the sickness still continuing. She died in about 10 or 15 minutes after I got her upstairs. When I first saw her there was a half-pint cup turned upside down in a tray in the bar. It had been recently washed in cold water. No one else could have washed it but the deceased.

Dr. Herapath asked if there was anything peculiar in the vomited matter. Mr. Pester stated that it presented a white, milky appearance—just like lime water.

William Brass: About half-past six o'clock I met Mr. Pester's servant, Emma Thomas, in Castle Street. She asked me if I would get three-pennyworth of oxalic acid for her at the druggist's shop. I complied with her request. I gave it to the girl Thomas, and she delivered it, in my presence, to Miss Salmon.

Dr. Herapath, F.R.S.: I was called in to the deceased a little after seven o'clock. I arrived about half-past seven o'clock, and she was then dead, and had been dead some fifteen minutes. I was told that she had been vomiting, and vomited matter was shown me. The vomit was a very remarkable one—mucus, with curdled, dark stuff—which led me to suspect altered blood. There did not appear to be any particles of food in the vomit. I have since examined the vomit, and I have found oxalic acid in small quantities in it. I have obtained crystals from the vomit, so small, however, that it required the microscope to discover them. I have examined the stomach. It presented an intensely blood-red appearance, and the fluid contained in it was blackened, curdled, altered blood. There were some very small white patches existing on the stomach, and the vessels were braced out and darkened as if by hardened blood. The blood was so altered in character that it was in fact insoluble. If the oxalic acid had been taken in water strongly impregnated with lime salts, the white appearance presented by the vomit would be accounted for, as the lime would be precipitated as oxalate of lime. I have never seen an instance of poisoning by oxalic acid before, but I have experimented upon animals. It is a very rapid case of poisoning—one of the most rapid on record. I have no doubt that she was poisoned by oxalic acid. The deep colour of the stomach was caused by the intense irritation, and next by the exudation of blood. The precipitation, darkening, and curdling of the blood are the first symptoms produced by this poison. One-sixth of the quantity taken in this case is recorded as having killed a person, and the shortest time on record is eight minutes. The time in this case I should think was about 15 or 20 minutes.

Mr. Pester: No, I don't think it was so long as that; not more than ten minutes. Mr. Pester also stated that the water drawn from his well and used in his house contained lime in large quantities.

Dr. Herapath: I have not analysed the contents of the stomach. She died unquestionably from collapse, which would be produced by oxalic acid.

Dr. Herapath remarked that the deceased, in his opinion, suffered from impulsive insanity.

The jury returned a verdict of "Temporary insanity."

Stopper Cord.—Stopper cord consists of conical rolls of very elastic rubber, about 4 feet in length, and varying in diameter from one-half an inch at one end to an inch and a half at the other. Stoppers of any diameter between these limits may be cut from the roll and bored with a common brass cock borer, which must be moistened with water to prevent adhesion to the rubber. The stoppers are found to be air-tight under the pressure of 15 lbs. to the inch, provided the contact between the tube and stopper is at least half an inch in length.

CONTEMPORARY SCIENTIFIC PRESS.

[Under this heading it is intended to give the titles of all the chemical papers which are published in the principal scientific periodicals of the Continent. Articles which are merely reprints or abstracts of papers already noticed will be omitted. Abstracts of the more important papers here announced will appear in future numbers of the Chemical News.]

Archives des Sciences. November 25, 1867.

M. Delafontaine: "*On some new and little known Molybdates, and on the principal Fluoxymolybdates.*"—J. L. Prevost: "*Researches on the Poisonous Action of Veratrine.*"

Journal des Fabricants de Papier. December, 1867.

E. Bourdilliat: "*On Testing the Chemical Products used in Paper Making. (Continuation.) Vegetable Colouring Matters.*"—Massia: "*A new Composition for rendering Canvas Waterproof.*"

Comptes Rendus. December 9, 1867.

A. Secchi: "*On Stellar Spectra, and on Shooting Stars.*"—E. Bourgoin: "*On the Electrolysis of Acetic Acid.*"

Monatsbericht der Koniglich Preussischen Akademie der Wissenschaften zu Berlin.

C. A. Martius: "*On Binitronaphthol.*"

Sitzungsberichte der Kaiserlichen Akademie der Wissenschaften zu Wien. (Mathematisch-Naturwissenschaftliche Classe.)
June, 1867.

E. Boricky: "*A Contribution to the History of the Formation of the Phosphates of Iron: Dufrenite, Beraunite, and Kakoxene from the Hrbek Mine near St. Benigna, Bohemia.*"—V. von Zepharovich: "*Mineralogical Notes. Part II.: 1. Barrandite from Cerhovic and Sphærite from Zajecov. 2. Boulangerite and Jamesonite from Pribram. 3. Mispickel. 4. Löllingite and Leucopyrite.*"

Bulletin de la Société Chimique de Paris. November, 1867.

Berthelot: "*On the Simultaneous Formation of Homologous Bodies in Pyrogenous Reactions.*"—E. Perret: "*On Refining Crude Camphor.*"

Annalen der Chemie und Pharmacie. November, 1867.

E. Linnemann: "*On the Transformation of Amine Bases into the corresponding Monatomic Alcohols.*"—A. Siersch: "*On the Transformation of Ethyl-Alcohol into Propyl-Alcohol.*"—A. Saytzeff: "*On the Action of Iodide of Methyl on Sulphide of Amyl-Ethyl.*" "*On the Action of Nitric Acid on Sulphide of Methyl and Sulphide of Ethyl.*"—N. Morgunoff: "*On Stannidimethyldiethyl.*"—F. Beilstein and U. Kreusler: "*On Para-nitrotoluylic Acid and its Derivatives.*"—C. Schorlemmer: "*Contributions to the Knowledge of the Hydrocarbons.*"—M. Fleischer: "*On Thionessal.*"—L. Scheller: "*On some Double Salts of Sulphate of Uranium.*"—F. Wohler: "*On a Compound of Chloride of Thallium with Perchloride of Iron.*"—"*Contributions to the Knowledge of Cerium.*"

Supplement.

L. Meyer: "*On the Molecular Volume of Chemical Compounds.*"

[English Edition, Vol. XVII., No. 438, pages 204, 205; No. 436, page 182; No. 435, page 170.]

—H. Schiff: "*Researches on the Boracic Ethers.*"—J. Erdmann: "*On the Composition of the Wood of Pinus abies.*"—H. L. Buff: "*On the Transformation of Monochlorhydrin into Propyl-glycol and Lactic Acid, and of Bichlorhydrin into Isopropylic Alcohol and Acetone.*"

Journal für Praktische Chemie. December, 1867.

C. O. Grass: "*Researches on the Analysis of Inflammable Gases, with especial Reference to that of Illuminating Gas.*"—C. Winkler: "*Contributions to the Knowledge of Indium.*"—C. Barford: "*On Gallate of Silver.*"

Annales du Génie Civil. December, 1867.

Lardani: "*An Improvement in the Manufacture of Sulphuric Acid.*"—Luhmann: "*An Improvement in Blast Furnaces.*"—Coste and Taupin de Rosnay: "*A Method of Obtaining Ammonia from Sewage and from the Waste Waters of Manufactories.*"—Perier and Possoz: "*On the Use of Lime for Preserving Beet Juice.*"

Bulletin de la Société d'Encouragement. October, 1867.

Guerard Deslauriers: "*On the Use of Bisulphide of Carbon for Estimating the Quantity of Tar, Pitch, and Resin contained in Artificial Fuel.*"—S. de Luca: "*On some Important Products obtained from the Olive and from the Australian Myrtle.*"

Comptes Rendus. December 16, 1867.

A. Wurtz: "*On the Synthesis of Neurine.*"—C. G. Wheeler: "*On the Action of Aqueous Hypochlorous Acid on Essence of Turpentine and on Camphor.*"

No. 26.

E. Demance: "*On the Amalgamation of Zinc Plates for Voltaic Batteries.*"—P. Van Tieghem: "*On the Transformation of Tannic Acid into Gallic Acid by Fermentation.*"

Sitzungsberichte der koniglich Bayerischen Akademie der Wissenschaften zu München. (*Mathematischphysikalische Classe.*)

March 2, 1867.

Voit: "*On the Relations of Creatine and Creatinine to Urea in the Animal Body, and on the Nature of Uræmia.*"

May 4.

Frischmann: "*On Twin Crystals of Chrysoberyl.*"—A. Vogel: "*Observations on the Solubility of some Silicates.*"

July 6.

Seidel: "*A Contribution to the Knowledge of the Limits of Accuracy of Chemical Balances.*"—C. Voit: "*On the Deposition of Uric Acid from Urine.*"

Comptes Rendus. December 30, 1867.

F. Pisani: "*On Woodwardite from Cornwall.*"—E. Bourgoin: "*On the Electrolysis of Tartaric Acid.*"

Poggendorff's Annalen der Physik. December, 1867.

R. Weber: "*On some Compounds of Bichloride of Titanium.*"

Journal für Praktische Chemie. December, 1867.

E. Salkoski: "*On the Estimation of Hippuric Acid as Hippurate of Iron.*"—H. Laspeyres: "*On the Composition of Prehnite.*"—T. S. Hunt: "*On J. D. Whelpley and J. J. Storer's New Method of Treating Ores.*"—J. H. Gladstone: "*On Pyrophosphoric Acid* —H. Ritthausen: "*On the Use of a Solution of Sulphate of Copper and Potash as a Test for Protein Compounds.*"—C. Bigott and R. Fittig: "*On the Synthesis of some New Hydrocarbons.*"

Annales de Chimie et de Physique. December, 1867.

Boussingault: "*On the Decomposition of certain Sulphates at a High Temperature.*"

Le Technologiste. October, 1867.

E. Beanes: "*On the Use of Ozone for Whitening Sugar, Syrup, and Molasses.*"

Comptes Rendus. January 6, 1868.

A. Houzeau: "*On the Estimation of Minute Quantities of Peroxide of Hydrogen.*"—H. Gal: "*On the Action of Chloride of Cyanogen on Zinc-Ethyl.*"

January 13, 1868.

A. Rommier: "*On Xylindeine, a New Colouring Matter Extracted from Decayed Wood.*"

January 20, 1868.

A. W. Hoffmann: "*On the Compounds Isomeric with the Sulphocyanic Ethers: 1. Oil of Mustard of the Ethylic Series.*"

Monatsbericht der Koniglich Preussischen Akademie der Wissenschaften zu Berlin. September and October, 1867.

Hofmann: "*On a new Series of Isomers of the Nitriles.*"—"*Contributions to the Knowledge of Methyl-aldehyde.*"

Poggendorff's Annalen der Physik. December, 1867.

C. Rammelsberg: "*On the Phosphites.*"

Annalen der Chemie und Pharmacie. December, 1867.

F. Beilstein: "*On Xylol and its Derivatives.*"—H. Y. de Schepper: "*On Sulphydrate of Xylil (Sulphoxenol).*"—A. Vollrath: "*On Chloride of Xylol, Chloride of Tolyl, and their Derivatives.*"—W. Holleman: "*On Dichloride of Xylol and Trichloride of Xylol.*" F. Beilstein: "*On Nitroxylol and its Derivatives.*"—G. Deumelandt: "*On Xylidine and its Derivatives.*"—E. Luhmann: "*On Dinitroxylol and Trinitroxylol.*"—R. Fittig and J. Konig: "*On Ethylbenzol and Diethylbenzol.*"—G. Stadeler: "*On the Constitution of Phenyl-sulphuric Acid.*"—H. Limpricht: "*On the Amines of Benzyl Alcohol.*"—H. Wichelhaus: "*On the Constitution of Organic Acids containing Three Atoms of Carbon.*"

January, 1868.

A. Grabowski: "*On the Tannic Acid of Oak Bark.*"—R. Otto and O. von Gruber: "*On Toluol-Sulphurous Acid.*" "*On the Estimation of Sulphur in Organic Substances by means of Chromate of Copper.*"—R. Otto: "*On Bichlorsulphobenzide.*"—E. Linkemann: "*On the Preparation of the Fatty Alcohols from their Primary Members.*" "*On Artificial Methyl Alcohol.*"—A. Siersch: "*On the Preparation of the Fatty Alcohols from their Primary Members.*" "*On the Transformation of Methyl Alcohol into Ethyl Alcohol.*"—W. Heintz: "*On the most Simple Process for Preparing Glycolamidic Acids from Monochloracetic Acid.*"—L. Scheffer: "*On Obtaining a Double Phosphate of Zinc and Soda by Fusion.*"—A. von Flemming: "*Note on Sulphochloride of Phosphorus.*"—H. Schweikert: "*Note on Phosphate of Soda and Ammonia, and on the Separation of Phosphoric Acid from Oxide of Zinc.*"—K. Birnbaum: "*On the Combinations of Ethylene and its Homologues with Bichloride of Platinum.*"—A. Geibel and H. L. Buff: "*On a Hydrocarbon analogous to Ethylene, obtained from Chloride of Hexylidene.*"

Annales des Mines. 1867.

W. Eggertz: "*Note on the Estimation of Sulphur in Iron and Iron Ores.*"

Comptes Rendus. January 27, 1868.

J. Reiset: "*Chemical Researches on the Respiration of Cattle.*" (*Continuation.*) "*On the Influence of Food on the Respiration of Cattle.*" "*A Comparative Study of the Respiration of Calves fed on Milk, and Calves at Grass.*" "*On the Nature of the Gases of the Hoove in Cattle, and on a Remedy for the same.*" "*Note on the Production of Nitric Oxide during the Fermentation of Beet Juice.*" "*On the Estimation of Ammonia in Beet Juice.*"—L. Marignac: "*On the Reduction of Niobium and Tantalum Compounds.*"—H. Deville: "*On the Extraction of Niobium.*"—H. Debray: "*Researches on Dissociation.*"

February 3, 1868.

T. Schlœsing: "*On the Decomposition of Nitrates during Fermentation.*"

February 10, 1868.

E. Becquerel: "*Fourth Memoir on some newly discovered Electro-Chemical Effects of Capillary Action.*"—Dubrunfaut: "*Memoir on a Nitrogenous Substance possessing greater Activity than Diastase contained in Malt, and on the Preparation and Application of the same in Manufactures.*"—"*On the Distillation of Beetroot, and on the Formation of Nitric Oxide during the Fermentation of the same.*"—A. Chauveau: "*On the Nature of Vaccine Virus—Experimental Determination of the Elements which constitute the Active Principle of the Virus of Small Pox.*"

Annalen der Chemie und Pharmacie. Supplement.

J. von Liebig: "*On some Methods of Silvering Glass.*"—W. von Schneider: "*A Method of Obtaining Pure Platinum and Iridium.*"—H. Kopp: "*On the Boiling Points of the Hydrocarbons.*"—H. Schiff: "*On Aldehyde Bases.*"—E. Erlenmeyer: "*On the relative Constitution of the Butylic and Amylic Alcohol of Fermentation.*"

Bulletin de la Société Chimique de Paris. December, 1867.

Berthelot: "*On a new Thermometer for Measuring High Temperatures.*"—A. Scheurer-Kestner: "*On a Crystallised Stannate of Sodium.*"—Berthelot: "*On Alcoholate of Baryta.*"—"*On the Oxidation of Organic Acids.*"—J. B. Grange: "*On the Quantity of Urea contained in the Urine of Persons Suffering from Chlorosis.*"—Gondolo: "*A Modification of Boussingault's Process for Manufacturing Oxygen and Nitrogen by passing a Current of Atmospheric Air over Caustic Baryta.*"—Bothe: "*On Tessié du Mothay and O. Maréchal's Process for the Manufacture of Oxygen.*"—Jacquemart: "*A new Process for the Manufacture of Sulphite of Alumina.*"—A. Girard: "*A Process for the Manufacture of White Lead.*"—Juette and de Ponteves: "*A Process for the Manufacture of Tartaric Acid from Refuse Grape Skins.*"—Dubart: "*A Method of Preparing Essence of Bitter Almonds for Use in Perfumery.*"—P. Schutzenberger: "*A Method of Re-making Waste Paper.*"—Perier, Possoz, Cail, and Co.: "*An Improved Method of Using Lime for the Preservation of Saccharine.*"

[English Edition, Vol. XVII., No. 435, page 170: No. 437, page 194; No. 438, pages 205, 206.]

NOTES AND QUERIES.

Estimation of Chlorine.—Can any one inform me of any more ready and reliable mode of estimating the feebly combined chlorine in bleaching powder, than the old protosulphate of iron test?—S. Dunn.

Palm Oil for Softening Dyed Yarns.—Would any of our kind friends inform me if there is any method to make palm oil mix with water, without the use of alkalies; if not, the best means to mix the above, so that it will not be injurious to colours?—Needful.

Electro-deposition of Iron.—Having occasion to electro-deposit iron on to a metallic surface of copper, and having at hand a solution of ferrous sulphate, slightly acidulated with sulphuric acid, and consisting of 1 part of the crystalline salt to 5 parts of water, I desired to ascertain, before making a stronger and, perhaps, more suitable solution for the purpose, whether the deposit from this solution would be reguline. On applying the galvanic current from three of Smee's battery cells (arranged in series) to this solution in the cold, I found that a reguline deposit of white, silvery-looking iron was obtained, but with the evolution of hydrogen gas in considerable quantity. Although certain alkaline solutions are known that will readily give up their metal in a reguline form during the rapid evolution, of hydrogen, thus forming exceptions to "Law I." in "Smee's Electro-Metallurgy," (3rd ed., p. 150) this is the first instance that has been met with, to my knowledge, of a solution similar to the above, and containing no alkali or other metal than the one to be thrown down, giving, by electrolysis, a reguline metallic deposit during the evolution of hydrogen. Each Smee's cell had 18 square inches effective area of positive surface; but the area of the anode and cathode, respectively, was 2 square inches. The battery exciting liquid was 1 part of oil of vitriol to 20 parts of water. As evidenced by a galvanometer in the circuit, the addition of a solution of sulphate of ammonium to the solution increased its conducting power; with this latter solution a very serviceable coating was obtained, but still with the evolution of hydrogen gas. I give these details in order that others may try the experiment, if they feel so inclined.—W. H. Walenn, F.C.S., 19, Talbot Road, Tufnell Park West, N.

Safflower.—This material contains, in its natural condition, two colouring substances, one insoluble in water, known as carthamin, the pink dye; the other, soluble in water, a yellow colouring matter. In order to obtain the pink dye, it is in the first place requisite that the safflower should be as free as possible from immixtures, as, for instance, seeds, the leaves of the plant, or other flowers, hay, straw, and similar substances. The carthamin is obtained in the following manner:—Safflower is exhausted with a very weak solution of carbonate of soda; in this solution pieces of cotton wool are placed, and the alkali is next neutralised by dilute acetic, or sulphuric acid; the cotton wool thus becomes pink dyed, and the dye is removed from it again by means of weak solution of carbonate of soda, which solution, after the removal, of course, of the cotton, which has given up its colour, is neutralised by means of a dilute acid; a precipitate thus ensues which is the carthamin; this may be purified by repeating the last treatment. The pink dye thus obtained is very beautiful, and especially applied to silk; only the dye is one of the most fugitive known. Repeatedly purified carthamin, mixed with French chalk, is often used by women to colour their cheeks.

Prussian Blue Paste.—Pure blue, even in paste, provided it be not too thin, will always, when prepared with care, exhibit that particularly coppery gloss which is also possessed by some kinds of indigo of good quality. This property is caused by a peculiar reflection of the light, and appears to have its origin in the peculiar state of aggregation of the particles of these substances, both of which, as is well known, are very difficult to ground to an impalpable powder. As a rule, the Prussian blue of commerce is, in chemical parlance, a mixture of neutral and basic Prussian blue. The coppery gloss is a peculiar property of well made blue, and cannot be brought on by artificial means.

Extraction of Grease by Bisulphide of Carbon.—Can any of your correspondents furnish me with any information upon this subject, or with the name and address of any firm, in this country or elsewhere, who make the apparatus employed for the purpose?—C. L. W.

Production of Carbonic Acid.—I should recommend your correspondent, "G.," to use magnesite, a natural carbonate of magnesia, for obtaining carbonic acid required. The gas obtained by subjecting this mineral in a retort to a red heat, is pure and odourless, and the resulting magnesia will be found more valuable than the original material. But the supply of magnesite is not large.—J. A. X.

Extract of Madder.—Can any of your readers inform me how the extract of madder which is now being used, is made?—W. B.

Estimation of the Sulphur Acids.—Can any reader suggest a method by which NaS, NaO,SO_2 and NaO,S_2O_2 can be detected and estimated separately in ordinary commercial "soda-ash?"—W. Blackril.

Unit of Momentum.—I will be much obliged if any one will inform me what the value of the unit of "momentum" is in avoirdupois weight. Our books on Mechanics tell us that momentum, or "quantity of motion," is equal to the product of the quantity of matter into the velocity, thus, M=QV. Now, suppose 1 lb. of matter moves 1 foot per sec., what is its moving force or "momentum" in avoirdupois ounces?—J. R. B., Davidson College, N. C.

Information Wanted.—A correspondent at Halifax, Nova Scotia, wishes to be put in communication with parties who would give reliable information 1. On the best ways of utilising the waste in iron-works, arising from the operations of planing, turning, boring, &c. 2. On the best way of manufacturing chloride of lime. 3. On the best way of manufacturing paper pulp from wood.

Palm Oil for Softening Dyed Yarns.—Palm oil and other oils may be made to mix with water without the use of alkalies, by subjugating, or incorporating with the oil albuminous substances, best yolk of eggs along with a small quantity of glycerine, and sometimes a solution of gum may be useful; in this way a magma is obtained which may be gradually mixed with water, and will form what is called an emulsion, in fact a mixture of water and oil, or better and more correctly, oil minutely divided through water by the aid of albumen. It is a well known fact that the yolk of eggs contains a large quantity of oil in its natural state, to which its colour is due, and which oil undoubtedly also assists the forming of an emulsion.—Dr. A. A.

Estimation of Chlorine.—Mr. Dunn will undoubtedly find in the manifold published works on Analytical Chemistry, Gay-Lussac's method, as also those of Mohr and Wagner, and perhaps he may try if he likes Runge's method, which is not so generally known, and is executed in the following manner:—Two grammes of the bleaching powder to be tested are well mixed with water, and the fluid so obtained mixed with a solution of protochloride of iron freshly made by dissolving 0·6 grm. of pure iron wire in pure hydrochloric acid, next pure hydrochloric acid in excess is added, and the fluid boiled in a flask, after previous addition of a piece of rather thick, perfectly clean, and brightly polished sheet copper, of a weight of about 4 grammes: the boiling is continued until the at first darkish colour of the fluid has become bright green; the copper is then removed from the flask, washed with distilled water, dried, and weighed. A loss in the weight of copper of 63·4=2Cu, is equal to 35·5 chlorine in the bleaching powder. This method is based on the fact, that under the conditions described, the chlorine of the bleaching powder first changes the protochloride of iron into perchloride, which in its turn is again reduced to protochloride by the metallic copper, whereby some of the latter becomes dissolved; for every 2 equivalents of copper dissolved in this way there is 1 equivalent of chlorine in the bleaching powder.—Dr. A. A.

ANSWERS TO CORRESPONDENTS.

Helta.—Use ammonia in preference to potash for precipitating the earths. You can always get rid of ammoniacal salts by ignition towards the close of an analysis.

John S. M.—The Chemical Society's Journal is no longer published by Baillière, but by Van Voorst.

Assayer.—German silver cannot be assayed in the dry way, owing to the difficulty of removing the nickel. In assaying brass and gun-metal the oxides of zinc and tin are so difficultly fusible that the dry process is also inapplicable to these alloys. An alloy of copper and silver, or copper and gold, may be assayed by cupelling with lead, and determining the copper by the loss.

Walter H.—A private letter will be sent if you forward your address.

Mineral.—The ore contains copper in considerable quantity, and also some silver. If you wish to have a quantitative analysis of it, will you be good enough to communicate with the Editor?

A. L. Steavenson.—We shall be pleased to hear further from this correspondent on the subject mentioned towards the conclusion of his letter. Such practical experience as he can furnish will be very valuable.

K.—1. Griffin and Sons. 2. Only the firm you have named. 3. Precipitate nitrite of silver by mixing nitrite of potash with nitrate of silver; wash and re-crystallise. 4. Consult the index. 5. We do not remember positively, but in some cases there was a slight colour.

W. S. S. W.—Use gun-cotton instead of powder for blasting the rock; that will enable you to get over the difficulties.

O. George.—This correspondent, who wrote respecting a trade process, is informed that the information can be sent if he will give his private address. We cannot publish it.

Querist.—It is a matter for a lawyer, rather than an editor, to advise about.

Tyro.—The mineral is iron pyrites.

Communications have been received from I. S. Muter; Dr. Adolph Ott; W. Angell; W. F. Barrett; H. Henderson (with enclosure); H. W. Lloyd Tanner; W. N. Hartley; M. Stanilas Meunier (with enclosures); J. Hargreaves (with enclosures and newspapers); T. F. Higginson; John Horsley (with enclosure); Charles Cochrane; W. N. Smythe; Nicholson, Maule & Co.; Dr. Adriani (with enclosures); A. P. Hurlstone; W. Selton; W. Skey (with enclosures); J. Williams; J. Stevenson; W. Bockhart Smith; Messrs. Longmans & Co.; Neil Matthieson; A. Coppins; J. Robertson & Co.; J. J. Lundy & Co.; W. Hall; Rev. R. Kirwan; J. C. Burrell, Sydney, N. S. Wales (with enclosure); Magnesium Metal Co.; Dr. Watts; Dr. Dupré; C. M. King; Rev. B. W. Gibsone, M. A.; J. H. Atherton; W. A. Simpson (with enclosure); F. A. Abel, F.R.S.; A. Bird (with enclosure); W. Lant Carpenter (with enclosure); A. A. Fesquet; Dr. A. Adriani; R. C. C. Lippincott (with enclosure); Professor Heaton; W. Blackril; J. M. Reid, Halifax, N. S.; J. Mayer (with enclosure); W. W. Reeves (with enclosure); A. L. Steavenson (with enclosure); W. H. Harrison; Howard Grubb; A. Le Sueur; T. Spencer (with enclosures); W. Bayliffe; J. Wilson; A. Wykes; Dr. Reimann; G. Bird, M.D.; J. Samuelson; H. Williams; T. Reader; J. G. Major; W. A. Smith; E. K. Muspratt; W. J. Morgan; J. E. Taylor; W. Blackie; Ernest Le Barbier; T. R. Fraser, M.D., Halifax (with newspapers); A. Vacher; T. Prentice and Co.; W. Bailey and Son; Harvey and Reynolds; D. W. Edwards; W. C. Deane.

Books received:—"Official Record of the Intercolonial Exhibition of Australasia," Melbourne: Blundell & Co.; "A Treatise on the Metallurgy of Iron," by H. Bauerman, F.G.S. London: Virtue & Co.; Popular Science Review;" "Pharmaceutical Journal;" "Scientific American;" "American Journal of Mining;" "American Artisan."

[English Edition, Vol. XVII., No. 435, page 170; No. 436, page 182; No. 437, page 194; No. 438, page 206; No. 435, page 170; No. 437, page 194; No. 435, page 170; No. 437, page 194; No. 435, page 170.]

AMERICAN DRUGGISTS' PRICE-CURRENT.

NEW YORK, JUNE 1, 1868.

JOBBERS' PRICES.

Aceton	per oz		to	85
Acid, Acetic, No. 8	per lb	22	to	30
sp. gr. 1,047 U.S.P.	per lb		to	30
Chemically Pure	per lb		to	50
Glacial	per lb		to	1 60
Benzoic, German	per oz		to	35
Boracic, pure	per lb		to	85
Citric	per lb		to	1 15
Fluoric, 1 lb bottles	per lb		to	2 20
Formic	per lb		to	3 50
Gallic	per lb		to	4 25
Hydrophosphorous	per lb		to	4 25
Lactic	per lb		to	4 25
Muriatic, 18 degrees	per lb		to	5
chemical pure	per lb		to	40
Nitric, 38 degrees	per lb		to	14
chemical pure	per lb		to	40
Oxalic, patent	per lb		to	37
Phosphoric, glacial	per lb		to	3 00
Prussic	per oz		to	14
Sulphuric	per lb	4	to	5
chemical pure	per lb		to	60
Valerian	per oz		to	1 40
Tartaric, gold	per lb		to	78
Aconite Leaves	per lb		to	28
Aconitia	per dr		to	5 25
Agaric Alba	per lb		to	80
Alcohol, 95 per ct	per gal		to	4 00
Aloes, Cape	per gal		to	23
Socotrine	per gal	90	to	1 20
Alum, Roman	per gal		to	11
lump	per gal		to	4½
Ambergris, gray	per oz		to	16 50
Ammonia Carbonate, bulk	per lb		to	23
in jars	per lb		to	24½
Muriate	per lb		to	14
Ammonia Aqua, 20 degrees	per lb		to	12
26 degrees	per lb		to	22
Hypophosphite	per lb		to	4 25
Oxalate	per lb		to	2 25
Phosphate	per lb		to	2 00
Sulphate	per lb		to	10
Ammonium Valerian, Crystals	per oz		to	1 50
Ammonium Bromide	per oz		to	4 25
Hydrosulphuret	per lb		to	75
Iodide	per lb		to	10 50
Amygdalin	per oz		to	3 75
Antimony and Potass	per lb		to	1 15
Butter	per lb		to	83
Arnica Leaves	per lb		to	20
Arrow Root, Bermuda	per lb	60	to	65
St. Vincent	per lb	20	to	22
Arsenic, white powdered	per lb		to	10
red pulv	per lb		to	35
red, lump	per lb		to	20
Arsenic Solution, Fowler's	per lb		to	18
Iodide	per lb		to	15 00
Sol., Donovan's	per lb		to	35
Asbestos	per lb		to	15
Asparagin	per oz		to	3 50
Atropia	per dr		to	3 75
Sulphate	per dr		to	3 75
Valerian	per dr		to	5 00
Balsam Fir	per gal		to	7 00
Copaiva	per lb		to	1 00
Peruvian	per lb		to	4 00
Tolu, true	per lb		to	1 50
Barbadoes Tar	per lb		to	30
Bark, Elm	per lb		to	20
Bark, Calisaya, quill	per lb		to	1 50
Red, quill	per lb		to	2 00
Pitayo	per lb		to	80
Cascarilla	per lb		to	12
Mezereon	per lb		to	30
Sassafras	per lb		to	15
Baryta Muriate	per lb		to	30
Nitrate	per lb		to	40
Bay Rum	per gal		to	4 00
Bebeerin, pure	per oz		to	5 50
Sulphate	per oz		to	3 50
Belladonna Leaves	per lb	22	to	25
Bicarbouate Soda	per lb		to	8
Bichromate Potash	per lb		to	22
Bismuth Metallic	per lb		to	3 00
and Ammonia Citrate soluble	per oz		to	90
and Ammonia Citrate Solution	per lb		to	1 15
Oxychloride	per lb		to	6 00
Subcarbon	per lb		to	6 75
Sub-Nitrate	per lb		to	6 85
Tannate	per oz		to	1 50
Valerianate	per oz		to	2 50
Black Drops	per lb		to	4 75
Blue Mass	per lb		to	65
Bole, Armenia, true	per lb	8	to	10
Borax, refined	per lb	36	to	38
Brimstone, roll	per lb	4	to	4½
Bromine	per lb		to	4 50
Brucia	per oz		to	3 75
Buchu Leaves, long	per lb		to	60
short	per lb		to	36
Burgundy Pitch, true	per lb	14	to	15
Cadmium, Bromide	per oz		to	60
Iodide	per oz		to	75
Metallic	per lb		to	4 60
Sulphate	per lb		to	9 00
Caffeine	per oz		to	9 50
Calcium Chloride	per lb		to	90
Iodide	per lb		to	8 50
Calomel, Hydrosub	per lb	1 00	to	1 05
Camphor, Refined	per lb	1 15	to	1 25
Cannella Alba	per lb		to	16
Cantharides	per lb		to	1 80
Carbon Bi-Sulphuret	per lb		to	38
Cascarilla Bark	per lb		to	12
Cassia Buds	per lb		to	1 10
Castor Oil	per gal	2 20	to	2 25
Caustic Soda	per lb	8½	to	9
Centaury Minor	per lb		to	38
Cerium, Oxalate	per oz		to	1 60
Nitrate	per oz		to	1 75
Chalk, Precip., English	per lb		to	28
Cherry Laurel Water	per lb		to	58
Chlorate Potass, English	per lb		to	57
Chloride Lime	per lb		to	6
Chloroform	per lb		to	1 90
Cinnamon, Ceylon, true	per lb		to	1 65
Citrine Ointment	per lb		to	58
Civet	per oz		to	5 50
Cobalt	per lb		to	25
Cocculus Indicus	per lb		to	35
Cocoa Butter	per lb		to	1 05
Codeine	per dr		to	2 50
Cod Liver Oil	per gal		to	2 00
Cod Liver Oil, ("Shore Oil")	per gal		to	1 90
Cod Liver Oil, J. C. Baker & Co.'s	per doz		to	8 00
" " "	per gross		to	90 00
" " " 5 gross,	per gr		to	87 00
Cod Live rOil, Hazard & Caswell's	per doz		to	7 50
" " "	per gr		to	90 00
Collodion	per lb		to	1 80
Cantharidal	per doz		to	4 75
Colocynth	per lb		to	70
Confectio Rosæ	per lb		to	48
Sennæ	per lb		to	48
Conium Leaves	per lb		to	25
Conin	per oz		to	7 50
Copper Ammoniated	per lb		to	1 20
Black Oxide	per lb		to	2 20
Carbonate	per lb		to	2 20
Sulphur, pure	per lb		to	38
Copperas	per lb	2	to	3
Corrosive Sublimate	per lb		to	95
Cream Tartar, Cryst	per lb		to	45
Cubebs	per lb		to	35
Cubebin	per dr		to	2 25
Cuttlefish Bone	per lb		to	23
Digitalis Herb	per lb		to	18
Digitaline	per dr		to	3 65
Dover's Powder	per lb		to	2 30
Dragon's Blood, mass	per lb		to	90
reeds	per lb		to	1 15
Dulcamara Stems	per lb		to	18
Emetine	per oz		to	3 65
Emery Corn	per lb		to	11
Flour	per lb		to	8
Epsom Salts	per lb		to	4¼
Ergot, new	per lb		to	1 15
Ergotine	per oz		to	1 15
Ether, Acetic	per lb		to	1 05
Butyric, concentrated	per lb		to	4 25
Butyraceous	per lb		to	1 30
Chloric	per lb		to	1 05
concentrated	per lb		to	1 65
Formic	per lb		to	4 25
Sulphuric	per lb		to	1 05
washed	per lb		to	1 15
concentrated	per lb		to	1 25
Extr. Jockey Club, Chiris	per lb		to	3 65
Extr. Ess. Bouquet, Chiris	per lb		to	3 75
Extr. Banana, superior	per lb		to	1 50
Extr. Orange, superior	per lb		to	1 50
Fluor Spar	per lb		to	16
Flowers, Althea	per lb		to	42
Arnica	per lb		to	22
Borrage	per lb		to	95
Flowers, Chamomile, German	per lb	22	to	28
Chamomile, Roman, 1867	per lb	55	to	60
Lavender	per lb	9	to	11
Malva, large	per lb	35	to	40
small	per lb		to	45
Rosemary	per lb		to	70
Tiliae	per lb		to	70
Violet	per lb		to	68
Fusel Oil, purified	per lb		to	2 50
Ferro - Phosphorated Elixir of Calisaya	per doz		to	12 00
Bark, Hazard & Caswell's,	per grs		to	144 00
Gamboge	per grs		to	2 75
Gelatine, French Pink	per lb		to	1 65
White French	per lb	80	to	1 10
Cox's	per grs		to	28 50
Ginger, Jamaica, bleached	per lb		to	22

Ginseng	per lb	80	to	1 00
Glauber Salts	per lb		to	8
Glycerine, common	per lb		to	35
concentrated	per lb		to	50
"Bowers"	per lb		to	70
"Price's"	per lb		to	1 20
Glycerole Hypophosphite	per lb		to	1 75
Grains D'Ambrette	per lb		to	60
Paradise	per lb		to	35
Gum Acroides	per lb		to	24
Amber	per lb		to	50
Ammoniac	per lb	40	to	75
Arabic, Turkey, sorts	per lb		to	40
1st picked, Trieste	per lb		to	90
2d " "	per lb		to	65
3d " "	per lb		to	50
Barbary	per lb		to	35
Assafœtida	per lb		to	45
Benzoin, common	per lb	80	to	90
prime	per lb	1 00	to	1 10
white marbled	per lb	1 10	to	1 15
Copal, Accra	per lb		to	
Benguela	per lb		to	85
Kowrie	per lb		to	45
Damar, Batavia	per lb		to	50
Singapore	per lb		to	45
Elemi, Aromatic	per lb		to	50
Euphorbium	per lb		to	25
Galbanum	per lb		to	95
strained	per lb		to	1 00
Gedda (gold)	per lb		to	28
Guaiacum	per lb		to	44
strained	per lb		to	47
Kino	per lb		to	1 20
Mastic	per lb		to	4 25
Myrrh, Turkey	per lb		to	60
Olibanum	per lb		to	80
tears	per lb		to	40
Sandarac	per lb		to	60
Shellac, Campbell's D. C	per lb	60	to	62
Garnet	per lb		to	55
No. 2	per lb		to	45
Native	per lb		to	45
Senegal	per lb		to	50
Tragacanth, common	per lb		to	35
flake	per lb	1 00	to	1 50
flaky sorts	per lb		to	60
Harlem Oil, Dutch	per grs		to	5 50
Hoffman's Anodyne	per lb		to	84
Hydriodate Potash, A and B	per lb		to	6 00
Conrad's (gold)	per lb		to	5 75
Hyoscyami Leaves	per lb		to	24
Hypophosphite Ammon	per lb		to	4 25
Iron	per lb		to	8 50
Lime	per lb		to	4 50
Manganese	per lb		to	14 00
Potash	per lb		to	4 40
Soda	per lb		to	4 40
Iceland Moss	per lb	10	to	12
Indian Hemp, true	per lb		to	1 60
Insect Powder, true	per lb		to	1 00
Iodine, Resublimed	per lb		to	6 50
Crude, in bulk	per lb		to	6 25
Irish Moss	per lb	8	to	10
Iron, Alum	per lb		to	1 60
by Hydrogen	per lb		to	2 80
Carb. Proto	per lb		to	45
Precip	per lb		to	25
Citrate and Ammonia	per lb		to	1 60
Magnesia	per lb		to	1 35
Quiniæ	per lb		to	12 50
Strychnine	per lb		to	12 50
Hypophosphite	per lb	8 40	to	8 50
Iodide	per lb		to	8 25
Syrup	per lb		to	80
Lactate	per lb		to	3 25
Phosphate, Precipitate	per lb		to	67
Pyrophosphate	per lb		to	1 60
Syrup	per lb		to	65
Sesquichloride	per lb		to	1 45
Sol	per lb		to	60
Sesquinitrate	per lb		to	44
Subsulphate	per lb		to	1 70
Sulphate, pure	per lb		to	9
Exsiccat	per lb		to	17
Sulphuret	per lb		to	31½
Superphosphate Syrup	per lb		to	65
Tannate	per lb		to	6 60
India Ink	per lb		to	1 75
Isinglass, American	per lb		to	1 75
Russian, true	per lb		to	6 50
Juniper Berries	per lb		to	4½
Juniper Tar Soap, Hazard & Caswell's	per doz		to	3 50
Kreosote, white	per lb		to	1 20
Lactucarium	per lb		to	12 00
Lead Acetate, pure	per lb		to	1 00
Licorice Paste, solid	per lb		to	42
Sicily	per lb		to	30
Calabria	per lb		to	42
imitation	per lb		to	37
Barracco	per lb		to	42
P. S	per lb		to	46
Lime, Carbonate, Precipitate	per lb		to	24
	per lb		to	4 25

Lime, Phosphate, Precipitate	per lb		to	40
Sulphite	per lb		to	18
Lime Juice	per gal		to	80
Lint, Taylor's	per lb		to	1 30
Lapis Calaminaris	per lb		to	8
Laurel Berries	per lb		to	10
Leaves	per lb		to	10
Liquid Styrax	per lb		to	60
Long Pepper	per lb		to	50
Lunar Caustic, pure	per oz		to	1 35
67 per cent., N. S	per oz		to	98
Lycopodium	per lb		to	65
Magnesia Carbonate	per lb		to	40
Calcined	per lb	95	to	1 20
ponderous	per lb		to	1 30
Citrate	per lb		to	1 75
Sulphite	per lb		to	1 20
Manganese, powdered	per lb		to	8
Saxony	per lb		to	8
Manna, small flake, '66	per lb		to	1 40
large flake, '65	per lb		to	1 90
shorts, new	per lb		to	1 00
Matico Leaves, true	per lb		to	44
Mercury	per lb		to	85
cum Creta	per lb		to	68
Magnesia	per lb		to	1 22
Cyanuret	per lb		to	5 80
Sulphuret	per lb		to	90
Mercurial Ointment (½M)	per lb		to	70
(⅓M)	per lb		to	60
Morphia Sulphate	per oz		to	7 85
Acetate	per oz		to	7 85
Muriate	per oz		to	7 85
Valerianate	per oz		to	9 50
Musk, true	per oz		to	16 00
in grain true	per oz		to	33 00
Nux Vomica	per lb		to	18
Oil, Amber, Crude	per lb		to	60
Almonds (Expressed) Allen's	per lb		to	1 00
Essential, Allen's	per lb		to	17 50
Anise	per lb		to	4 50
Bergamot	per lb	9 50	to	10 25
FF, new crop	per lb	7 50	to	8 50
Bergamot, Donner's	per lb	8 50	to	9 75
Bergamot,—Sanderson's	per lb	9 75	to	10 25
Cade	per lb		to	1 00
Cajeput	per lb		to	2 00
Camphor	per lb		to	1 75
Caraway	per lb		to	2 76
Seed	per lb		to	5 50
Cassia	per lb	4 00	to	4 25
Cinnamon, true	per oz		to	1 50
Citronella, prime	per lb		to	2 80
Winter's	per lb		to	3 50
Copaiva	per lb		to	3 00
Croton	per lb		to	4 15
Cubebs	per lb		to	4 50
Cummin	per lb		to	10 00
Fennel	per lb		to	3 00
Geranium	per lb	12 00	to	22 00
Chiris	per lb		to	26 00
Prepared	per lb		to	25 00
Turkish	per lb	14 00	to	18 00
Jessamine	per lb		to	3 50
Juniper	per lb		to	1 25
Berries, true	per lb		to	3 50
Lavender, Garden, forte	per lb		to	1 65
fine	per lb		to	1 85
Flowers, Chiris, No. 1	per lb		to	3 75
Lavender Spike	per lb		to	1 00
Laurel, Expressed	per lb		to	90
Lemon, Donner's	per lb		to	4 25
Lemon,—G. R. & Co's	per lb		to	4 25
—Sanderson's (new)	per lb		to	4 25
Lemongrass,—Winter's	per lb		to	7 00
Mace, Expressed	per lb		to	2 50
Marjoram	per lb		to	1 75
Myrrbane	per lb		to	2 00
Neroli Bigarade	per oz		to	4 75
Chiris	per oz		to	4 75
Petit Grain	per lb		to	28 00
Olive, pure, casks	per gal		to	3 00
Marseilles, quarts	per box		to	6 25
pints	per box		to	7 75
Orange	per lb		to	4 10
Origanum	per lb	75	to	1 50
Patchouly	per oz		to	4 00
Pennyroyal	per lb	4 50	to	8 00
Peppermint, pure	per lb		to	6 00
Rhodium	per lb		to	10 00
Rose, Kissaulick	per oz		to	11 50
Rosemary, French	per lb		to	1 75
Trieste	per lb		to	1 15
Chiris	per lb		to	2 25
Sabine, pure	per lb		to	2 00
Sassafras, cans	per lb		to	1 30
Sessame, Salad, fine	per lb		to	2 25
Spearmint, Hotchkiss	per lb		to	8 50
Spike	per lb		to	
Succinum, crude	per lb		to	60
rectified	per lb		to	65
Tanzy,—"Eastman's"	per lb		to	5 50
Thyme, white, pure	per lb		to	2 75

Oil Wintergreen, Van Deusen Bro's	per lb		to	4 50
Wormwood	per lb	8 00	to	10 00
Wormseed, Western	per lb		to	2 75
Baltimore	per lb		to	4 25
Black Pepper	per lb		to	12 00
Cognac	per oz		to	8 50
Ergot	per oz		to	25
Opium (gold)	per lb		to	11 00
Orange Buds or Apples	per lb		to	18
Curacoa Ribs	per lb		to	28
Otto Rose, pure	per oz		to	11 50
commercial	per oz		to	7 50
Peppers, Zanzibar	per lb		to	33
Phosphorus	per lb		to	1 25
Amorphous	per lb		to	8 25
Piperin	per oz		to	1 75
Podophyllin	per oz		to	90
Poppy Heads	per lb		to	25
Potassa Acetate	per lb		to	90
Bicarbonate	per lb		to	42
Carbonate	per lb		to	25
Caustic, common	per lb		to	70
white	per lb		to	1 00
Citrate	per lb		to	1 00
cum Calce, powdered	per lb		to	75
Hypophosphite	per lb		to	4 25
Permanganate, ordinary	per lb		to	80
Phosphate	per lb		to	3 00
Prussiate	per lb		to	40
Sulphate	per lb		to	16
Tartrate	per lb		to	1 05
Potassium	per oz		to	8 75
Bromide	per lb		to	2 50
Cyanide, fus	per lb		to	85
gran	per lb		to	1 80
Iodide	per lb		to	5 40
Sulphuret	per lb		to	85
Quinine Citrate, with Iron	per oz		to	85
Sulphate, American	per oz		to	2 20
French	per oz		to	2 15
Quassia, rasped	per lb		to	6
Red Chalk Fingers	per lb	6½	to	7
Red Precipitate	per lb		to	1 25
Resin of Jalap, pure	per lb		to	28 00
Rochelle Salt	per lb	48	to	50
Roots, Aconite	per lb		to	24
Alkanet	per lb	17	to	18
Althea	per lb	22	to	30
Angelica	per lb		to	35
Calamus	per lb	20	to	60
Colchicum	per lb		to	20
Colombo	per lb		to	22
Culveris	per lb		to	24
Dandelion	per lb		to	30
Galangal	per lb		to	12
Gentian	per lb		to	10
Ginger, Race, African	per lb		to	20
Jamaica, Bleached	per lb		to	35
Golden Seal	per lb		to	30
Hellebore black	per lb		to	16
white	per lb		to	30
Ipecacuanhæ	per lb		to	3 50
powdered	per lb		to	3 75
Jalap	per lb		to	2 50
powdered	per lb		to	2 40
Licorice	per lb		to	18
Mandrake	per lb		to	15
Orris, Florentine	per lb		to	15
Verona	per lb		to	14
Pink	per lb		to	32
Rhatany	per lb		to	30
Rhubarb, E. I	per lb	2 75	to	4 00
Turkey	per lb		to	24 00
Sarsaparilla, Honduras	per lb		to	54
Mexican	per lb		to	28
Turbeth	per lb		to	60
Valerian, English	per lb		to	55
Dutch	per lb		to	40
German	per lb		to	24
Vermont	per lb		to	45
Snake, Virginia	per lb		to	55
Seneca	per lb		to	42
Rose Leaves	per lb		to	2 25
Rosemary Leaves	per lb		to	12
Rubigo Ferri	per lb		to	10
Saffron, American, new	per lb		to	95
Spanish, true	per lb		to	18 00
Sago, Pearl	per lb		to	10
Salicin	per oz		to	60
Sal Acetoscella	per lb		to	55
Ammoniac	per lb		to	15
Soda, Newcastle	per lb		to	4
Santonine	per lb		to	21 00
Sassafras Bark	per lb		to	15
Scammony, virg., true	per lb		to	20 00
Seeds, Anise	per lb		to	28
star	per lb		to	55
Canary, Dutch	per bush	6 00	to	6 25
Smyrna	per bush		to	6 25
Cardamom, Malabar	per lb		to	4 50
Carui	per lb		to	23
Celery	per lb		to	65
Clover	per lb		to	16
Colchicum	per lb		to	24
Seeds, Coriander	per lb		to	16
Cummin	per lb		to	20
Fennel	per lb		to	20
Fœnugreek	per lb		to	12
Hemp	per bush		to	3 50
Linseed, American clean	per tierce		to	
rough	per bush		to	3 25
Bombay (gold)	per bush		to	2 85
Calcutta (gold)	per bush		to	2 65
Mustard, brown	per lb		to	18
white	per lb		to	18
Rape	per bush		to	5 00
Timothy	per bush		to	5 00
Worm	per lb		to	23
Seidlitz Mixture	per lb		to	48
Senna, Tinnevelly	per lb		to	28
Alexandria	per lb		to	33
E. I	per lb		to	28
Smalts, Blue	per lb		to	22
Snuff, Lorrillard's Maccaboy	per lb		to	78
Coarse Rappee	per lb		to	1 00
Irish High Toast	per lb		to	85
Fresh Scotch	per lb		to	85
Soap, Castile, Mottled	per lb		to	20
White	per lb		to	25
floating	per lb		to	25
Low's Brown Windsor	per grs		to	16 00
Soda Acetate	per lb		to	85
Chlorate	per lb		to	2 15
Chloride, Liquor	per gal		to	45
Citrate	per lb		to	1 00
Hydrosulphate	per lb		to	1 05
Hypophosphite	per lb		to	4 10
Hyposulphite	per lb		to	10
Nitrate, pure	per lb		to	22
Phosphate	per lb		to	31
Pyrophosphate	per lb		to	1 25
Sulphite	per lb		to	32
Ash	per lb		to	4½
Sodium	per lb		to	11 00
Iodide	per lb		to	8 00
Spirit Ammonia	per lb		to	70
Aromatic	per lb		to	75
Lavender	per lb		to	70
Nitre Dulc	per lb		to	60
Rosemary	per lb		to	70
Sponges, Bahama	per lb		to	90
Bathing, Formes	per lb		to	4 00
Coarse Brown	per lb		to	60
Fine, medium	per lb	6 00	to	7 00
Surgeon's	per lb	4 00	to	7 00
Zimoca	per lb	2 00	to	8 00
Cup, Turkey	per lb	20 00	to	30 00
Trieste	per lb	4 50	to	18 00
Fine Toilet, bleached	per lb	12 00	to	15 00
Fine Trieste, small	per lb	4 00	to	4 50
Glove	per lb	1 75	to	2 00
Grass	per lb	20	to	25
Sheep's wool	per lb	1 25	to	1 40
Sur Choix	per lb		to	5 50
Squills	per lb		to	12
St. John's Bread	per lb		to	8
Strontia Muriate	per lb		to	33
Nitrate	per lb		to	33
Oxalate	per lb		to	1 30
Strychnia Acetate	per oz		to	4 25
Citrate	per oz		to	80
Nitrate	per oz		to	4 25
Pure, crystallized	per oz		to	1 37
powdered	per oz		to	3 60
Sulphate	per oz		to	4 25
Valerianate	per oz		to	5 50
Styrax Calamita	per lb		to	55
Sugar of Lead	per lb		to	40
Sugar of Milk	per lb		to	58
Sulphur Sublime	per lb		to	6
Tamarinds	per lb		to	10
Tannin	per lb		to	3 60
Tapioca, East India, white	per lb		to	12
Pearl	per lb		to	14
Tartar Emetic, powdered	per lb		to	1 00
crystallized	per lb		to	1 20
Tin Foil, thin	per lb		to	45
French, No. 15	per lb		to	70
Tobacco	per lb		to	40
Tonqua Beans, Para	per lb		to	85
Augustora	per lb		to	1 90
Uva Ursi, American	per lb		to	12
French	per lb		to	13
Vanilla Beans, Bourbon	per lb		to	11 00
Mexican	per lb		to	14 00
Venice Turpentine	per lb		to	27
Veratria	per oz		to	5 25
Vitriol Blue	per lb	10½	to	11
Green	per lb		to	2
White	per lb		to	9
Wax, White,—J. I. Elkens	per lb		to	75
No. 2	per lb		to	72
Phillip's	per lb		to	90
Yellow	per lb		to	52
White Wax,—Leonhardt's	per lb		to	82
Ockmid	per lb		to	78
Sun-bleached	per lb		to	67
White Precipitate	per lb		to	1 60
White Pepper	per lb		to	58

Wine, Colchicum Seeds	per lb		to	1 50
Wood Naphtha	per lb		to	95
Wormwood Herb	per lb		to	25
Yellow Bark	per lb		to	80
Dock	per lb		to	25
Zaffre	per lb		to	1 15
Zinc, Acetate	per lb		to	1 25
Chloride	per lb		to	1 80

DYES AND DYESTUFFS.

Aniline Blue	per lb		to	8 00
Red	per lb		to	7 00
Violet	per lb		to	8 00
Annatto	per lb	1 00	to	1 80
Cochineal, Honduras	per lb		to	1 50
Mexican	per lb		to	1 45
Cudbear, pure	per lb	28	to	45
Cutch, Pegue	per lb		to	18
Gambier	per lb		to	8
Indigo, Bengal, fine	per lb	2 75	to	3 00
good	per lb	2 50	to	2 60
middling	per lb	2 00	to	2 10
Madras, fine	per lb	1 50	to	1 55
ordinary	per lb	1 00	to	1 25
Kurpah	per lb		to	
Guatemala	per lb	1 68	to	1 75
Caraccas	per lb	1 48	to	1 65
Lac Dye, good to fine	per lb	55	to	60
Logwood, Campeachy	per ton		to	24 00
Honduras	per ton		to	28 00
Jamaica	per ton		to	20 00
Laguna	per ton		to	
St. Domingo	per ton	19 00	to	20 00
Chip	per lb	2	to	2½
Extract	per lb	10½	to	12½
" in bulk	per lb	10½	to	10½
Lima Wood (gold)	per bbl	70 00	to	71 00
Madder, Dutch	per lb	20	to	21
French	per lb	20	to	22
Nutgalls, Blue, Aleppo	per lb	40	to	42
Orchille	per lb	30	to	35
Persian Berries	per lb	50	to	55
Safflower	per lb	60	to	65
Sapanwood	per lb	12	to	15
Turmeric	per lb	28	to	45
Ultramarine	per lb	12½	to	15
Woad	per lb		to	

DRUGGISTS' GLASSWARE.

[PACKAGE PRICES.]

Green Bottles and vials	50	percentage	discount.
German Flint Bottles and vials	30	"	"
Flint Bottles and vials	25	"	"
Furniture Ware	10	"	"
Perfumer's Ware	25	"	"
Chemical Ware	net	"	"
Syringes	10	"	"
Homœopathic vials	10	"	"

NAVAL STORES.

Pitch, City	per bbl	3 75	to	4 00
Rosin, Extra Pale	per 280 lbs	8 00	to	9 00
Pale	"	7 00	to	8 00
No. 1	"	5 00	to	6 00
No. 2	"	4 00	to	5 25
Strained	"	4 00	to	5 00
Common	"		to	3 75
Spirits, Turpentine (North Carolina)	per gal		to	60
Turpentine, Soft	per 280 lbs		to	8 00

OILS.

Linseed Oil, American	per gal		to	1 25
English	per gal		to	1 25
Palm Oil	per gal		to	14
Paraffine Lubricating Oil	per gal		to	40
Sperm, Crude	per gal		to	2 20
Sperm, Winter, unbleached	per gal		to	2 30
Lard Oil Prime, City	per gal		to	1 60
Red Oil, City distilled	per gal		to	70
Red Oil, Saponified	per gal		to	78
Whale, Crude	per gal		to	85
Whale, Bleached, Winter	per gal		to	95

PAINTS (DRY).

Asphaltum, opt	per lb		to	7
Barytes, Foreign	per ton		to	45 00
Barytes, American	per lb		to	2
Black Lead	per lb	8	to	10
Black Ivory, drop, fair	per lb	10	to	13
good	per lb	18	to	20
best	per lb	24	to	28
Blue Celestial, good	per lb		to	14
Chinese	per lb		to	1 00
Prussian, fair to best	per lb	25	to	1 00
Ultramarine, fair to best	per lb	30	to	60
Chalk, Lump	per ton		to	27 60
China Clay	per ton		to	38 00
Chalk	per bbl		to	4 50
Green Paris, fair to best	per lb	35	to	50
Green Chrome, fair to best	per lb	38	to	42
Lamp Black—Coach Painter's—L. Martin & Co.'s	per lb	23	to	25
Lamp Black, ordinary	per paper	8	to	12
Litharge, powdered, American & English	per lb	11	to	11½
Ochre, Yellow, French, dry	per lb	2½	to	3¼
Red Venetian	per lb	3½	to	4½
Red Indian, fair to best	per lb	11	to	17
Red Lead, American	per lb	11	to	12
English	per lb	14	to	15
Rose Pink	per lb	18	to	20
Sienna, American	per lb	7	to	9
Italian, B'nt	per lb	18	to	22
Raw	per lb	15	to	20
Umber, Crude, Turkey	per lb	5	to	7
burnt	per lb	9	to	10
Tieman's Calif. Vermilion	per lb		to	1 20
Pure Carmine	per lb		to	18 00
Soluble Blue	per lb		to	1 25
Vermilion, English, pale	per lb		to	1 40
deep	per lb		to	1 85
American	per lb		to	35
Chinese	per lb		to	1 35
Trieste	per lb		to	1 20
White, China	per lb		to	23
Cremnitz	per lb		to	30
Lead, pure	per lb	13	to	14
good	per lb	12	to	12½
Paris	per lb	3½	to	4
Zinc, American	per lb	10	to	12
Zinc, French	per lb	14	to	16
Whiting	per lb	2½	to	3

PAINTS (IN OIL).

Black coach	per lb	28	to	30
Blue, Chinese	per lb	90	to	1 00
Prussian, fair to best	per lb	35	to	60
Brown, Van Dyke, fair to best	per lb	20	to	23
Dryer, Patent, American	per lb	12½	to	14
English	per lb	12½	to	15
Green, Chrome	per lb	13	to	27
Imperial	per lb	15	to	18
Paris	per lb	33	to	42
Verdigris	per lb	25	to	63
Putty, in bladders	per lb	5½	to	6
in bulk	per lb	5¼	to	5½
Red Venetian, fair to best	per lb	8	to	16
Sienna, burnt, fair to best	per lb	22	to	35
White Lead, English, B. B	per lb		to	16
American, pure	per lb	13½	to	14
good	per lb	11	to	12½
fair	per lb	9	to	10
White Zinc, American	per lb	10	to	13
French	per lb	15	to	15½
Yellow Ochre	per lb	9	to	10
Chrome, fair to best	per lb	16	to	32

SPICES.

Cassia, in mats (gold)	per lb	56	to	58
Cassia, (gold—in bond)	per lb	30	to	32
Cloves, (gold)	per lb		to	23
Ginger, Race, African (gold)	per lb	11	to	12
Mace, (gold)	per lb	95	to	1 00
Nutmegs, No. 1, (gold)	per lb	92	to	96
Pepper, (gold)	per lb	26	to	27
Pepper, (gold—in bond)	per lb	9	to	10
Pimento, Jamaica, (gold)	per lb	21	to	22
Pimento, (gold—in bond)	per lb	4½	to	5

WINDOW GLASS.

American Window—1st, 2d, 3d and 4th qualities.

6 by 8 to 8 by 10	Per fifty feet	$ 6 25	to	3 75
8 by 11 to 10 by 15	"	6 75	to	4 75
11 by 14 to 12 by 18	"	7 50	to	5 50
13 by 16 to 16 by 24	"	8 50	to	6 00
18 by 22 to 18 by 30	"	10 00	to	7 00
20 by 30 to 24 by 30	"	12 50	to	8 00
24 by 31 to 24 by 36	"	14 00	to	9 00
25 by 36 to 26 by 40	"	16 00	to	10 00
28 by 40 to 30 by 48	"	18 00	to	14 00
24 by 54 to 32 by 56	"	20 50	to	16 00
32 by 58 to 34 by 60	"	24 00	to	18 00
34 by 62 to 40 by 60	"	26 00	to	21 00

The above is subject to a discount of 25 per cent.

French Window—1st, 2d, 3d and 4th qualities. (Single thick.)

6 by 8 to 8 by 10	Per fifty feet	$ 6 25	to	4 75
8 by 11 to 10 by 15	"	6 75	to	5 00
11 by 14 to 12 by 18	"	7 50	to	5 50
13 by 18 to 16 by 24	"	8 50	to	6 00
18 by 22 to 18 by 30	"	10 00	to	7 00
20 by 30 to 24 by 30	"	12 00	to	[illegible]
24 by 31 to 24 by 36	"	14 00	[illegible]	[illegible]
25 by 36 to 30 by 48	"	16 00	[illegible]	[illegible] 00
28 by 40 to 30 by 48 (3 qlts)	"	18 00	[illegible]	14 00
24 by 54 to 32 by 56 (3 qlts)	"	20 50	to	16 00
32 by 56 to 36 by 60 (3 qlts)	"	24 00	to	18 00
34 by 52 to 40 by 60 (3 qlts)	"	26 00	to	21 00

Subject to a discount of 20 per cent. English sells at 10 per cent. discount off the above rates.